UNIVERSIDADE DE LISBOA

ISEG - Lisbon School of Economics & Management

Essays on Macroprudential Policy and Growth

André Manuel Ventura Soares Teixeira

Orientador: Prof. Doutor António Manuel Pedro Afonso

Tese elaborada para obtenção do grau de Doutor em *Economia*

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2024

Essays on Macroprudential Policy and Growth

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ISEG - Lisbon School of Economics & Management

This dissertation is submitted for the degree of
Doctor of Philosophy in Economics

May 2024

To my boy Pedro,
for reminding me what truly matters

Declaration

I hereby declare that, except where specific reference is made to the work of others, the contents of this dissertation are original and have not been submitted in whole or in part to any other degree or qualification in this or any other university. This dissertation contains 42,753 words including appendices, equations, figures, footnotes, references, and tables.

André Teixeira
May 2024

Statement of Work

I declare that Chapter 1 is published in the *International Journal of Central Banking* (SJR Q1, CABS 3, 5-year IF: 1.49) as Teixeira, A., and Venter, Z. (2023), “Macroprudential Policy and Aggregate Demand”, *International Journal of Central Banking*, 19(4): 1-41. An earlier version of the paper was awarded the INFER Young Economist Prize 2022 (best paper by an economist under age 32).

I declare that Chapter 2 is published in the *Journal of Policy Modeling* (SJR Q1, CABS 2, 5-year IF: 2.72) as Teixeira, A. (2023), "The Real Effects of Macroprudential Policy: Evidence from Micro Data", *Journal of Policy Modeling*, 44(6): 1165-1178.

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Finally, I declare that Chapter 4 is not yet published.

These papers were presented and discussed in the following conferences (in chronological order): IFABS 2023 (Oxford); 11th UECE Conference on Economic and Financial Adjustments (Lisbon); 15th Annual Meeting of the Portuguese Economic Journal (Terceira); 24th INFER Annual Conference (Timisoara); 23rd INFER Annual Conference (Lisbon).

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Preface

In the last decade or so, macroprudential policy (MaPP) has become one of the most powerful weapons in the arsenal of crisis economics. But what do we know about its effects on growth? How does MaPP affect individual decisions to save and borrow? Does MaPP impact the redistribution of wealth? And, for that matter, how does MAPP affect private and public debt? A convincing answer to these questions is crucial to guide and constrain central banks.

One may wonder why the effects of MaPP on growth are not yet understood. One answer may be that MaPP only became a prominent issue in policy debate after the Great Recession. However, a more charitable explanation is that conventional analysis of MaPP is fraught with difficulties. Why? Because MaPP is often implemented in response to contemporaneous events. Furthermore, each country decides when to implement MaPP. This makes it especially difficult to estimate the impact of MaPP in a setting with multiple countries and variation in treatment timing. I suspect this is why the literature has been almost silent about the *causal* effects of MaPP until now.

This thesis attempts to fill this void. The first chapter investigates the impact of MaPP at the aggregate level, particularly on consumption and investment. The next chapter examines how MaPP influences individual behaviour based on micro-level data. The third chapter is concerned with the redistributive effects of MaPP. The final chapter shows that there is a "paradox" in MaPP, whereby prudential regulation reduces private debt while increasing government debt. This thesis is, above all else, an attempt to offer useful policy guidance.

Abstract

The relationship between MaPP and growth is examined in four related chapters. Each chapter uses a novel empirical approach to estimate the *causal* effects of MaPP on some of the most important determinants of growth.

The first chapter is a joint work with Zoë Venter and it is published in the *International Journal of Central Banking*. Using a novel difference-in-differences with staggered adoption, we show that MaPP reduces household consumption and increases firm investment. We find that some prudential tools have pernicious effects on consumption, while others lead to higher firm investment. The effects of MaPP are found to be relatively mild in the short run but become more pronounced in the long run. These findings point to a weaker macroeconomic impact of MaPP than suggested in previous studies.

The second chapter is published in the *Journal of Policy Modeling*. I use micro-level data from 122 countries to examine how MaPP affects individual behaviour. I find that people save more and borrow less after the adoption of MaPP. These effects are then disaggregated by policy tool, interest rate, and country income level. The results show that the effects of MaPP depend more on the design of the policy tool and the country's income level and less on the interest rate. These findings stand up to a variety of endogeneity tests that include propensity score matching and an instrumental variable approach.

The third chapter is published in the *Journal of Financial Stability*. I examine the effects of MaPP on wealth inequality based on a large sample of 171 countries. I find that, after the adoption of MaPP, wealth concentration in the treated countries increases by 3.4 percentage points in a decade. This finding is explained by a rise in the wealth share of the top 1% combined with a sharp decline in the wealth share of the bottom 50%. These effects are stronger for prudential rules based on income, particularly in advanced economies.

The fourth and final chapter is a joint work with António Afonso and is under review in a similar-level journal. We investigate the impact of banking prudential regulation on sovereign risk. As long as prudential regulation improves financial stability, it lowers sovereign risk and enables governments to increase their spending. Consequently, countries with prudential regulation have lower primary budget balances and accumulate more government debt over time. These results suggest that prudential regulation reduces private debt, while paradoxically increasing public debt. We explore several explanations for this paradox. Our results indicate that prudential regulation induces governments to accumulate debt because it improves a nation's credit rating and its borrowing conditions in sovereign bond markets.

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Chapter 1

Macroprudential Policy and Aggregate Demand

1.1. Introduction

Macroprudential policy (MaPP) is back in fashion, and rightly so. Few economists today would dispute that MaPP is a powerful weapon in the arsenal of crisis economics. But what do we know about its effects on aggregate demand? How does it affect consumption and investment? Answering these questions is crucial to assess the overall impact of MaPP.

So far, the existing literature has focused on the effects of MaPP on output (e.g., Lim et al., 2011; Cerutti et al., 2017). Not surprisingly, these studies find an inverse relationship between the adoption of MaPP and economic growth. Their story is intuitively simple but it has important implications: MaPP constrains credit with searing consequences on growth. This poses the question: should we evaluate the effectiveness of MaPP through the lens of output growth? After all, the goal of MaPP is to tame credit and slow down growth. A more pertinent question to ask is whether MaPP affects households and firms in a similar way.

In this paper, we argue that the effects of MaPP on consumption and investment depend – directly or indirectly – on the financial constraints imposed on households and firms. If MaPP tightens borrowing constraints for everyone in the same way, both households and firms will be forced to save more and borrow less. This should lead to a decline in both consumption and investment. However, if MaPP makes access to credit more difficult for households than for firms, consumption is likely to fall but investment should remain stable or even increase if banks expand credit to the corporate sector. The converse could also be true: if MaPP makes access to credit more difficult for firms than for households, investment is likely to plummet and consumption should remain stable or even increase if banks shift their lending to households. In these last two scenarios, MaPP may have a profound effect on consumption and investment. By how much is an empirical issue, which we address via a novel difference-in-differences (DiD) approach.

The purpose of this paper is to extend the existing literature in three directions. First, we isolate the effects of MaPP on spending components of aggregate demand, particularly on consumption and investment. A shortcoming of previous papers is that they do not explain how MaPP influences private spending and imperils growth. Relative to these papers, we directly link the adoption of MaPP to fluctuations in household consumption and firm investment.

Second, we depart from traditional regressions and time series models to establish causality from MaPP to aggregate demand. This is important because MaPP is usually implemented in response to contemporaneous events. By using the first wide-scale staggered DiD in a policy setting, we are able to estimate the effects of MaPP in a setting with multiple countries and variation in treatment timing.

Finally, we distinguish between the short- and long-run effects of MaPP. Separating out the two is an empirically difficult matter, but we are able to estimate a single interpretable treatment effect parameter that accounts for the dynamic effects of MaPP. This allows us to examine the effects of MaPP over time and determine whether they are more pronounced in the short or long run. This distinction between short- and long-run effects has received surprisingly little attention in the literature, but it is crucial to our understanding of the overall impact of MaPP.

As we shall see, our results indicate that MaPP has asymmetric effects on consumption and investment. In particular, we find that households in countries that implement MaPP increase their savings rate by 1.87-3.63 percentage points. This corresponds to a sharp increase of one quarter in savings. Furthermore, we find that MaPP boosts firm investment by a whopping 5.05-6.63 percentage points over time. These results are statistically significant and stand up to several robustness checks.

The rest of the paper is organized as follows. Section 1.2 reviews the existing literature. Section 1.3 provides a detailed explanation of the staggered DiD. Section 1.4 discusses the empirical results and investigates the robustness of our findings. Section 1.5 concludes.

1.2. Literature Review

A vast literature examines the effects of MaPP on financial stability. Most of these papers suggest that MaPP curtails lending (Lim et al., 2011; Dell'Ariccia et al., 2012) and reduces excessive leverage (Claessens et al., 2013). Moreover, MaPP lessens the probability of a crisis (e.g., Kraft and Galac, 2011), especially in housing markets (Crowe et al., 2011; Kuttner and Shim, 2013). This early literature provides compelling evidence that MaPP is an effective tool to manage financial cycles and reduce systemic risk.

However, recent research finds that MaPP may have deleterious effects on growth. Most notably, Angellini et al. (2014) show that banking regulation decreases the steady state level of output. A few general equilibrium models also show that MaPP can be used to correct externalities in aggregate demand (e.g., Farhi and Werning, 2016). These results seem to be consistent with conventional theory on the relationship between credit and spending. If MaPP restricts access to credit, it may force constrained households to reduce consumption (e.g., Hall, 2011). In a scenario of rapid deleveraging, MaPP may even increase precautionary savings, which is likely to depress aggregate demand even further (e.g., Eggertsson and Krugman, 2012; Guierrieri and Lorenzoni, 2017).

This theoretical work finds empirical support in studies that use regressions and time series models to estimate the effects of MaPP on growth (e.g., Lim et al., 2011; Akinci and Olmstead-Rumsey, 2018; Cerutti et al., 2017). This stream of research amassed a remarkable body of evidence on a negative relationship between MaPP and output growth. These studies generally conclude that MaPP should be tightened in boom periods and loosened in bust periods. Yet, these papers offer little explanation on the transmission channels of MaPP, i.e., the way in which MaPP is supposed to have affected output. This point is key to our understanding of the causal effects of MaPP on growth.

Until now, only a few empirical papers have used causal techniques to identify the impact of MaPP on growth. Behncke (2020) uses a simple DiD to estimate the effects of MaPP on lending using data from 25 banks in Switzerland. Her findings show that MaPP constrains lending with no unintended consequences on credit risk. But most papers point to important redistributive effects of MaPP. For example, DeFusco et al. (2020) use a DiD strategy to exploit a policy-induced discontinuity in the DSTI ratio in the US. They report a substantial increase in borrowing costs following the adoption of MaPP with ill effects on the distribution of leverage in the mortgage market. Interestingly, Acharya et al. (2020) also find a similar reallocation of mortgage credit after running a DiD model on loan level data from Ireland.

A major drawback of traditional DiD strategies is that they restrict the analysis to micro-level data from a single country. This is perhaps the simplest way to estimate the treatment effects of MaPP on credit. Although this DiD approach sheds light on the impact of MaPP on credit, it leaves many questions unanswered. For instance, how does MaPP affect spending across countries? How does the length of exposure to MaPP influence consumption and investment over time? By construction, traditional DiD approaches are unable to answer these questions because countries implement MaPP in different time periods.

Overall, there has been, in both theory and empirical work, an obvious push for generality on the effects of MaPP. Yet, it remains unclear how MaPP affects households and firms. The existing work is premised on the assumption that MaPP affects all agents in the same way. But this is unlikely because MaPP imposes different financial constraints on households and firms. A more interesting way to assess the overall impact of MaPP is to disentangle its causal effects on households and firms. It is to these matters that we turn next.

1.3. Methodology

1.3.1. Method

Our methodology is based on the DiD approach with staggered treatment adoption proposed by Callaway and Sant'Anna (2020). Similarly to a standard DiD, this method allows for a causal interpretation and it circumvents the restrictive assumptions of regressions and time series models. But unlike a standard DiD, it enables us to estimate the average treatment effects of MaPP in a setting with multiple countries and variation in treatment timing.

To do so, let us start with some notation. We consider τ periods where $\mathrm{t} = 1, \dots, \tau$ and that $\mathrm{D_t}$ is a binary variable that equals 1 when a country implements MaPP in quarter t and 0 otherwise. We then define $\mathrm{G_g}$ equal to 1 when a country is first treated in quarter g and 0 otherwise. Lastly, we assign C equal to 1 to the countries that never implement MaPP in our sample (i.e., "never-treated") and 0 otherwise. This implies that each country in our sample will have exactly one $\mathrm{G_g}$ or C equal to 1.

The generalized propensity score $\mathrm{p_g(X)}$ is then defined as the probability that a country is treated conditional on having covariates X and belonging to group g or the control group, i.e., $\mathrm{p_g(X) = P(G_g = 1|X, G_g + C = 1)}$. The observed outcome in each period t is estimated as follows:

$$\mathrm{Y_t = D_t Y_t(1) + (1 - D_t) Y_t(0)} \tag{1.1}$$

where $Y_t(1)$ and $Y_t(0)$ are the potential outcomes in time t with and without treatment, respectively.

In contrast to a standard DiD, our main causal parameter of interest is a group-time average treatment effect ($ATT(g,t)$). Simply put, the $ATT(g,t)$ gives us the average treatment effect experienced by group g in time t with "group" being defined as the first period of implementation of MaPP, as below:

$$ATT(g,t) = E[Y_t(1) - Y_t(0)|G_g = 1] \tag{1.2}$$

In our panel data setup, under the assumptions of parallel trends, irreversibility of treatment and covariate overlap and for $2 \leq g \leq t \leq \tau$, the $ATT(g,t)$ for group g in period t can be nonparametrically identified and estimated as below[1]:

$$ATT(g,t) = E\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{E[\frac{p_g(X)C}{1-p_g(X)}]}\right)(Y_t - Y_{g-1})\right] \tag{1.3}$$

Equation (1.3) allows us to assess how the effect of MaPP varies by group and time. It is worth noting that the $ATT(g,t)$ weights up observations from the control group that shares similar characteristics to those in each treated group. This reweighting procedure ensures that the covariates of the treated group and the control group remain balanced.

Next, we aggregate the $ATT(g,t)$ across g and t to interpret the overall effects of MaPP. Given that many, if not most, treated groups will comprise a single country, the easiest way to obtain an "overall" $ATT(g,t)$ is to use a simple average, as follows:

$$\frac{2}{\tau(\tau-1)} \sum_{g=2}^{\tau} \sum_{t=2}^{\tau} \mathbb{1}\{g \leq t\} ATT(g,t) \tag{1.4}$$

Alternatively, we can compute a weighted average of each $ATT(g,t)$ by putting more weight on the $ATT(g,t)$ of groups that are exposed to MaPP for longer, as below:

$$\frac{1}{k} \sum_{g=2}^{\tau} \sum_{t=2}^{\tau} \mathbb{1}\{g \leq t\} ATT(g,t) P(G = g) \tag{1.5}$$

where $k = \sum_{g=2}^{\tau} \sum_{t=2}^{\tau} \mathbb{1}\{g \leq t\} P(G = g)$ so that the weights on the $ATT(g,t)$ sum to 1.

[1] Equation (1.3) identifies the treatment effects under the assumptions of parallel trends, irreversibility of treatment and covariate overlap. The first assumption is tested using the Cramér-von-Mises (CvM) test that fails to reject the parallel trends (Appendix A.3). The second assumption states that a country that adopts MaPP is forever treated, which is consistent with the behaviour of countries in our sample that rarely reverse MaPP. The last assumption simply requires a control group for every treatment period.

In our baseline model, the results are computed using the doubly robust method[2], no covariates and a “not yet treated” control group. Statistical significance is assessed using clustered bootstrapped standard errors at the country level, which also account for the autocorrelation of the data. Of course, making inference based on several $\mathrm{ATT}(g,t)$ can be troublesome. In the following subsections, we explain how the choice and timing of MaPP can bias the estimates of the overall $\mathrm{ATT}(g,t)$. We then describe in detail how we estimate the treatment effect parameters to circumvent these issues. This can be done by computing group-time treatment effects and dynamic effects.

Group-time Treatment Effects

The adoption of MaPP is a choice of each country. Therefore, countries that implement MaPP earlier may also experience the effects of being treated earlier. A caveat of combining the $\mathrm{ATT}(g,t)$ across g and t using a simple average is that we may overweight the effect of early-treated groups with more observations in post-treatment periods. To get around this issue, we compute the $\mathrm{ATT}(g,t)$ specific to each treated group and we average them across all post-treatment periods:

$$\tilde{\theta}_S(g) = \frac{1}{\tau - g + 1} \sum_{t=2}^{\tau} \mathbb{1}\{g \leq t\} \mathrm{ATT}(g,t) \tag{1.6}$$

Equation (1.6) is the time-averaged treatment effect for countries in group g. In simple terms, it is an average of each available $\mathrm{ATT}(g,t)$ in a particular group g across time. The “overall” ATT, θ_S, can then be estimated by aggregating the group-specific treatment effects across groups, as below:

$$\theta_S = \sum_{g=2}^{\tau} \tilde{\theta}_S(g) P(G = g) \tag{1.7}$$

Equation (1.7) is our main measure of the overall impact of MaPP on aggregate demand. Although it may seem similar to equation (1.5), there is an important difference in the weights. While equation (1.5) assigns more weight to groups with a higher number of post-treatment periods, the weights in equation (1.7) depend only on group size. In this way, equation (1.7) does not overweight the effects of earlier-treated groups and provides an unbiased estimate of the effects of MaPP on each treated group g.

[2]The ATT uses OLS regression to compute the difference between the treated and control groups for each observation; these differences are then weighted according to the probability of each observation occurring.

Dynamic Treatment Effects

The effects of MaPP on aggregate demand may also depend on the length of exposure to these policies. One may expect larger effects of MaPP to occur in longer horizons when households and firms have had the time to adjust their behaviour. However, a caveat of parameter (1.5) is that it does not explicitly consider a country's length of exposure to MaPP. To account for this, we begin by averaging the group-time $\mathrm{ATT(g,t)}$ into treatment effects at different lengths of exposure to treatment, as follows:

$$\tilde{\theta}_{\mathrm{D}}(\mathrm{e}) = \sum_{\mathrm{g}=2}^{\tau}\sum_{\mathrm{t}=2}^{\tau} \mathbb{1}\{\mathrm{t}-\mathrm{g}+1=\mathrm{e}\}\mathrm{ATT(g,t)P(G=g|t-g+1=e)} \tag{1.8}$$

where e is the length of exposure to treatment.

A length of exposure equal to 0 estimates the average effect of MaPP across groups in the quarter of implementation of MaPP. To make the point most clearly, suppose that $\mathrm{e}=1$. Then, equation (1.8) estimates a value for the $\mathrm{ATT(g,t)}$ based on group size for $\mathrm{g}=\mathrm{t}=0$. This will be the estimate of the $\mathrm{ATT(g,t)}$ in the first quarter after MaPP adoption. When $\mathrm{e}=2$, equation (1.8) estimates a different value for the $\mathrm{ATT(g,t)}$ based on group size for all groups where $\mathrm{t}-\mathrm{g}=1$. This will be the estimate of the $\mathrm{ATT(g,t)}$ for all the countries exposed to MaPP for 2 quarters. The $\mathrm{ATT(g,t)}$ is computed iteratively in this way for $\mathrm{e}=0,\ldots,40$.

The θ_{D} captures the dynamic evolution of treatment effects by averaging $\tilde{\theta}_{\mathrm{D}}$ over all possible values of e, as below:

$$\theta_{\mathrm{D}} = \frac{1}{\tau-1}\sum_{\mathrm{e}=1}^{\tau-1}\tilde{\theta}_{\mathrm{D}}(\mathrm{e}) \tag{1.9}$$

Equation (1.9) is our main estimate of the dynamic effects of MaPP. Once again, the crucial difference between θ_{D}, θ_{S} and equation (1.5) is in the weights: θ_{D} puts more emphasis on $\mathrm{ATT(g,t)}$ when g is significantly less than t (i.e., when e is large). This allows for groups with longer exposure to MaPP to be weighted more when there is a relatively small number of groups with long periods of exposure. This parameter is particularly suitable to measure how the treatment effects of MaPP evolve over time.

1.3.2. Data

Our empirical setting uses quarterly data on 21 European countries spanning the period 2000:Q1 to 2019:Q4[3]. In our baseline model, the main variables of interest are the household savings rate and firm investment rate. These two measures are often compared and the data is readily available from Eurostat. To provide robustness checks, we also consider other proxies like household consumption to GDP and non-financial corporations' (NFC) gross fixed capital formation (GFCF) to GDP. Summary statistics are presented in Table 1.1.

We assign to the treated group every country that implements prudential rules to reduce banks' exposure to household and firm risks. This includes the loan-to-value (LTV) ratio, debt-service-to-income (DSTI) ratio, and loan restrictions[4]. This data was collected from the IMF iMaPP database (Alam et al., 2019) and updated with information from the ECB Macroprudential Bulletins.

Most countries in our sample end up implementing MaPP at some point in time. This may raise concerns about the size and heterogeneity of our control group. For example, our control group may comprise only low-income or high-income economies with very different characteristics to an average country. As such, we force the control group in the baseline model to include countries that have "not yet" implemented MaPP. This increases the size of the control group at the expense of treatment heterogeneity. The remaining models use alternative specifications for the control group, the estimation method, and the aggregation method.

Although EU countries are fairly homogeneous, there could be covariate-specific trends in aggregate demand across groups. In particular, the literature on the secular drivers of savings suggests that demographics and inequality could influence private spending[5]. To account for these factors, we run alternative specifications of our model including the dependency ratio and GDP per capita. Detailed descriptions of every variable are available in Appendix A.1.

[3]Our initial dataset comprises the 27 member states of the EU plus the United Kingdom. We exclude Cyprus, Malta, Lithuania and Luxembourg due to severe swings in savings and investment. Additionally, Bulgaria, Greece and Romania are removed because data is missing in the pre-treatment or post-treatment periods.

[4]In a similar spirit to Lim et al. (2011), we focus on loan-targeted MaPP.

[5]Other potential drivers of savings may include government debt and unemployment. However, these factors are not explicitly included as control variables in the DiD because they also influence the choice of MaPP and could render our results invalid. That said, GDP per capita should partially capture these effects.

1.4. Results

1.4.1. Baseline Results

Table 1.2 presents the estimates for the impact of MaPP on household savings. The bulk of the results indicates that MaPP leads to a surge in savings. In the baseline model, the group-time treatment effect of MaPP increases savings by 1.94 percentage points. This impact is surprisingly consistent across models ranging from 1.87-2.41 percentage points. To be clear, this estimate for savings is not a small number. The average savings rate in our sample is only 11.33%, which means that MaPP pushes savings up by approximately one quarter.

The vast majority of group-time treatment effects are statistically significant and they stand up to robustness checks that control for demographics and income. They also hold when we restrict the control group to "never treated" countries. When we control for demography and income, the group-time treatment effects are based on the assumption that only countries with similar dependency ratios and GDP per capita would follow a similar trend in savings in the absence of MaPP. These conditional results indicate that MaPP leads to a rise in savings of 2.12-2.41 percentage points. Altogether, both unconditional and conditional results suggest that households increase their savings over and above what they would have in the absence of MaPP.

An interesting question is whether the impact of MaPP on savings is more profound in the short or in the long run. This can be assessed by examining the dynamic effects of MaPP. Our results show that the impact on savings gets stronger as countries are exposed to MaPP for longer. When we consider the length of exposure, the dynamic impact of MaPP on savings ranges from 2.93-3.63 percentage points. This can be visually inspected in Figure 1.1, which depicts the dynamic impact of MaPP on savings under the assumption of unconditional parallel trends. The dynamic effect on savings remains positive across time and becomes stronger as the length of exposure to MaPP increases, especially after year three.

The uncanny finding that MaPP has a lower impact in the short run is not entirely new. A couple of papers using regressions to estimate the effects of MaPP over a four-year window report a severe contraction in credit and output around year three (e.g., Borio and Shim, 2007; Richter et al., 2019). Our estimates now provide a potential explanation for the plunging output: households reduce their consumption and increase savings around year three.

Table 1.3 presents our estimates for the impact of MaPP on firm investment. The group-time treatment effects of MaPP on firm investment are somewhat more modest. Most of our models report a positive and significant impact on investment but a few of the estimates are not statistically significant. This is not unexpected because firms often plan their investments in advance. It may be the case that the effects of MaPP are mostly dynamic.

Not surprisingly, the results for the dynamic effects on investment are far more enlightening. Our estimates show that MaPP increases firm investment by 5.05-6.63 percentage points over time. This corresponds to an increase of more than one quarter in firm investment since the average investment rate in our sample is 23.94%. As before, Figure 1.2 displays the dynamic effects of MaPP on investment. This figure forcibly shows that the short-run effects on investment are relatively mild but they build up significantly with time. The firm investment picks up around year four, which suggests that firms are slower to adjust to MaPP than households. The dynamic effects on investment peak at 6.63 percentage points when we control for differences in income across countries.

In summary, our results suggest that MaPP increases firm investment at the expense of household consumption. The immediate effects of MaPP are relatively modest but they pick up in the long run. If we make a crude comparison between our estimates for savings and investment, firm investment increases twice as much as the decrease in household consumption. These results point to a weaker macroeconomic cost of MaPP than reported in previous papers.

1.4.2. Individual Policy Tools

In this section, we disaggregate the effects of each MaPP tool on aggregate demand. We hope to cast light on the tools that have the greatest impact on private spending. To study individual policy choices, we ensure that the treated countries have not yet implemented another MaPP at the time of treatment. We also examine only countries that implement one household- or firm-targeted policy to isolate the impact of this policy choice.

Tables 1.4 through 1.9 provide the estimations of the impact of LTV ratios, loan restrictions, and DSTI ratios on household savings and firm investment. In Tables 1.4 and 1.7, we can see that the implementation of LTV ratios results in a 3.39 and 3.83 percentage point increase in savings and investment, respectively. When looking at the dynamic effects, we find that implementing an LTV ratio pushes savings and investment up by 4.25 and 8.17 percentage points over time, respectively. The dynamic impact is statistically significant in all cases. In the case of group-time treatment effects, we lose some statistical significance for savings when we condition on GDP per capita. But even in this case, the impact is positive, which is consistent with our main findings.

Tables 1.5 and 1.8 provide the estimates for the impact of loan restrictions on savings and investment, respectively. In short, we find that loan restrictions have little impact on savings and investment. A possible explanation for this result is that loan restrictions are more likely to affect emerging economies than advanced economies. This is because loan restrictions usually target foreign currency lending, certain types of liabilities and excessive leverage (e.g., Cerutti et al., 2017).

Lastly, Tables 1.6 and 1.9 show the impact of the DSTI ratio on savings and investment, respectively. The results indicate that the adoption of a DSTI ratio spurs savings by 7.64 percentage points. Over time, the dynamic effects of the DSTI ratio result in a marked increase in savings of 4.74-8.74 percentage points. Of course, one should interpret these numbers with caution because inference is based on small treated groups. But if our results are more than chance, the DSTI ratio has serious consequences for households. It literally pushes savings up and sends consumption sharply downward. Interestingly, the impact of the DSTI ratio on investment is nearly zero in our baseline model. If we condition on the dependency ratio and the GDP per capita, we obtain mixed results but the impact on investment is always relatively meager. Overall, the DSTI ratio has a greater impact on consumption than on investment.

Our bottom-line result is that measures that directly restrict access to credit – mainly the LTV and the DSTI ratio – have a stronger impact on the spending components of aggregate demand. But their impact is strikingly different: while the LTV ratio affects both household consumption and firm investment, the DSTI ratio has a greater effect on household consumption.

1.4.3. Robustness Checks

Our attempt to establish robustness takes two tacks. First, we test if our results are robust to alternative proxies for the dependent variables. In doing so, we provide reassuring evidence on the validity of our results. Second, we check if our results hold when we restrict the sample to include only countries that never loosen or remove MaPP. This addresses the main limitation of the staggered DiD, which assumes that countries that adopt MaPP will never reverse these policies.

Alternative Dependent Variables

A potential concern with our analysis is that we only look at the effects of MaPP using a single measure of household savings and firm investment. To explicitly address this issue, we rerun the DiD on alternative proxies of spending, particularly on household consumption to GDP and NFC GFCF to GDP.

Tables 1.10 and 1.11 provide the estimations of the DiD using these alternative proxies. We find that household consumption is 0.67 percentage points lower than what it would have been in the absence of MaPP. The dynamic effects on consumption raise this number to 2.20-2.83 percentage points over time. These estimates are in the same ballpark as the ones obtained earlier. Some of these results have less statistical significance but they all point to a negative impact of MaPP on consumption in the short and long run. This ties in with our previous finding that household savings rise sharply in response to MaPP.

The impact on GFCF is also similar to before. Once again, the group-time average treatment effects on investment are close to zero but we do find strong dynamic effects. As the length of exposure to MaPP increases, the impact on GFCF becomes more pronounced. We estimate that the impact on investment can be as high as 3.95-4.98 percentage points. Perhaps more interestingly, we again find that investment only picks up three years after the adoption of MaPP. These results suggest that banks may need some time to adjust to new regulations or that agents may not be as forward-looking as previously thought. The underlying causes of the surge in investment are hard to pinpoint in our analysis, but one thing is clear: MaPP has a lower impact on investment in the short run than in the long run. This result is statistically significant and holds across all our model specifications. This is interesting because MaPP is often implemented in response to a shock. But perhaps this is already too late.

Restricted Sample

A caveat of our staggered DiD is the assumption that once a country is treated, it will remain treated throughout the sample period. This could bias the results if countries reverse MaPP at some point in time. To account for this possibility, we rerun our models using a restricted sample that includes only countries that never loosen or remove MaPP[6].

Tables 1.12 and 1.13 show the estimations of the DiD using the restricted sample. In general, our results continue to hold throughout. They show that, on average, households save between 1.96-2.74 percentage points more than they would have in the absence of MaPP. The dynamic impact suggests an increase of up to 4 percentage points over time, particularly after three years of exposure to MaPP as shown in Figure 1.3. This matches all our previous findings.

[6] Only Denmark, The Netherlands and Poland remove or loosen a prudential rule during our sample period. These countries are excluded from this restricted sample.

Turning to investment, one gratifying result is that our estimates become more statistically significant in the restricted sample. The group-time average treatment effect is now statistically significant in half of our models with an estimated impact of 0.62-1.14 percentage points. The dynamic effects of MaPP indicate that investment increases by 6.78-8.22 percentage points, which is only slightly higher than the impact found in the baseline results. When we plot the dynamic effects in Figure 1.4, we can see that GFCF starts to rise significantly after year four, which is also in line with the pattern of the firm investment rate. This is reassuring despite the fact that we use a different sample.

1.5. Conclusions

In this paper, we have investigated the causal effects of MaPP on aggregate demand. Using a novel DiD approach with staggered adoption, we find that MaPP reduces household consumption in the short and long run while increasing firm investment in the long run. These results clarify a point that is too often overlooked in the literature: consumption and investment are important transmission channels through which MaPP affects growth.

But why does MaPP hinder consumption and facilitate investment? We believe the answer lies in the type of financial constraint imposed by MaPP. When we look at individual MaPP tools, we find that LTV and DSTI ratios have a deleterious impact on household consumption even though the LTV ratio has a positive effect on firm investment. A potential explanation for this is that LTV ratios target mainly home loans. This is likely to foster financial stability, which may lead to a surge in investment at the cost of lower consumption. Finally, we find little evidence that loan restrictions affect aggregate demand, at least, in advanced economies.

Some limitations of our model point to potential research opportunities. First, the staggered DiD assumes that a country becomes forever treated after implementing MaPP. An unintended consequence is that we cannot fully capture the effects of loosening, tightening or removing MaPP. We address this caveat by rerunning our model on a restricted sample of countries that never loosen or remove MaPP. But it would also be interesting to assess how changes in the overall macroprudential stance affect consumption and investment.

Second, our results provide suggestive evidence that some MaPP tools have a disproportionally high impact on aggregate demand. Yet, we are unable to fully disaggregate the effects of individual tools because most of the countries in our sample implement DSTI ratios in conjunction with LTV ratios or loan restrictions. Understanding how the design of MaPP influences aggregate demand remains a potentially fruitful area for research.

In spite of these caveats, our results offer useful policy guidance. An important finding is that MaPP has a weaker macroeconomic cost than previously suggested in the literature. If left unattended, MaPP can have pernicious effects on consumption; but if properly managed, MaPP can also lead to higher investment over time. The overall macroeconomic impact, then, depends on a country's policy objectives. If private consumption is in a free fall, MaPP may aggravate the consequences for households, particularly if countries implement LTV and DSTI ratios. But if private consumption is relatively stable, then MaPP can be an effective tool to restore financial stability and boost investment in the long run.

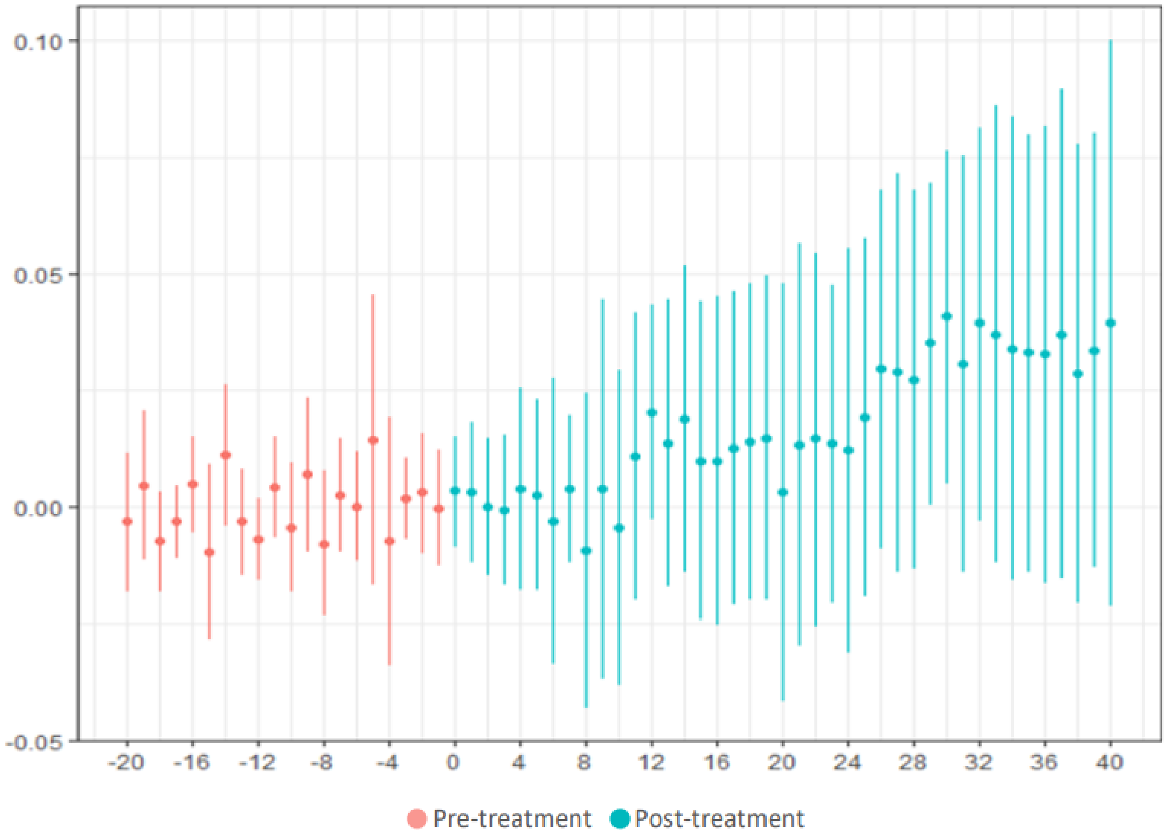
Another important finding is that the effects of MaPP on aggregate demand seem to only gain traction after three years. This finding is interesting because MaPP is usually tightened in response to a crisis but our results suggest that this is already too late. Indeed, MaPP may only send demand downward at the height of the crisis. Instead, our results support the view that policymakers should continuously adjust MaPP in much the same way as monetary policy. But given that MaPP has the ability to drive spending, policymakers should be cautious about using it liberally.

Table 1.1: Summary Statistics

Sample	Obs.	Mean	Std. Dev	Min	Max
HH Savings Rate: Full Sample					
HH Savings Rate	1200	0.11	0.04	0.00	0.20
Dependency Ratio	1200	0.26	0.04	0.15	0.36
GDP per capita	1200	7.34	3.11	1.17	18.58
Firm Investment Rate: Full Sample					
Firm Investment Rate	1200	0.24	0.06	0.09	1.25
Dependency Ratio	1200	0.26	0.04	0.15	0.36
GDP per capita	1200	7.37	3.07	1.05	18.58
HH Savings Rate: Control Group (Full)					
HH Savings Rate	760	0.12	0.04	0.00	0.20
Dependency Ratio	760	0.26	0.04	0.18	0.36
GDP per capita	760	6.49	2.53	1.17	11.23
HH Savings Rate: Not Yet Treated Countries					
HH Savings Rate	360	0.09	0.03	0.00	0.18
Dependency Ratio	360	0.24	0.03	0.18	0.34
GDP per capita	360	4.94	2.66	1.17	9.32
Firm Investment Rate: Control Group (Full)					
Firm Investment Rate	818	0.24	0.05	0.12	0.41
Dependency Ratio	818	0.26	0.04	0.18	0.36
GDP per capita	818	6.51	2.54	1.05	11.23
Firm Investment Rate: Not Yet Treated Countries					
Firm Investment Rate	418	0.26	0.05	0.12	0.41
Dependency Ratio	418	0.25	0.04	0.18	0.34
GDP per capita	418	5.03	2.56	1.05	9.24

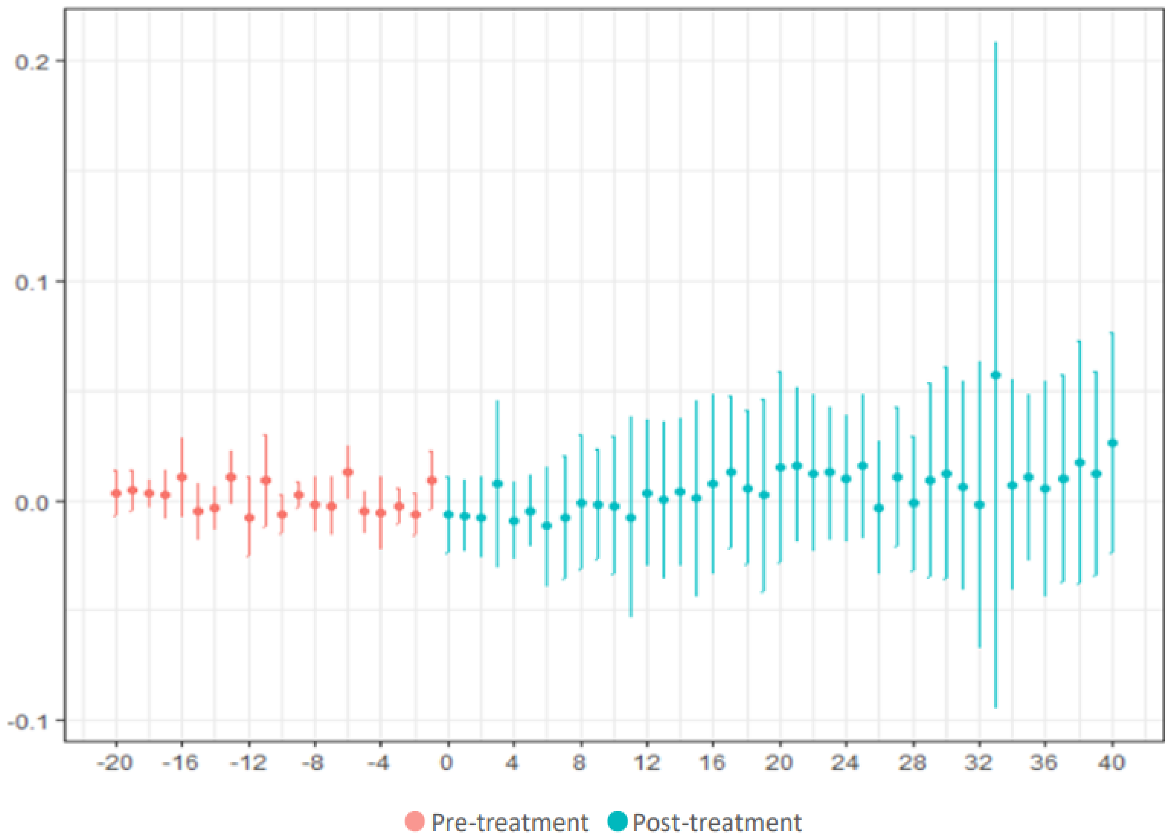
Note: The table reports summary statistics of pre-treatment variables for control and treatment groups. The full control group includes never treated and not yet treated countries. The statistics are further disaggregated into a subset of countries that have not yet adopted MaPP during the sample period.

Figure 1.1: Dynamic Impact of MaPP on Household Savings Rate, 2000-2019



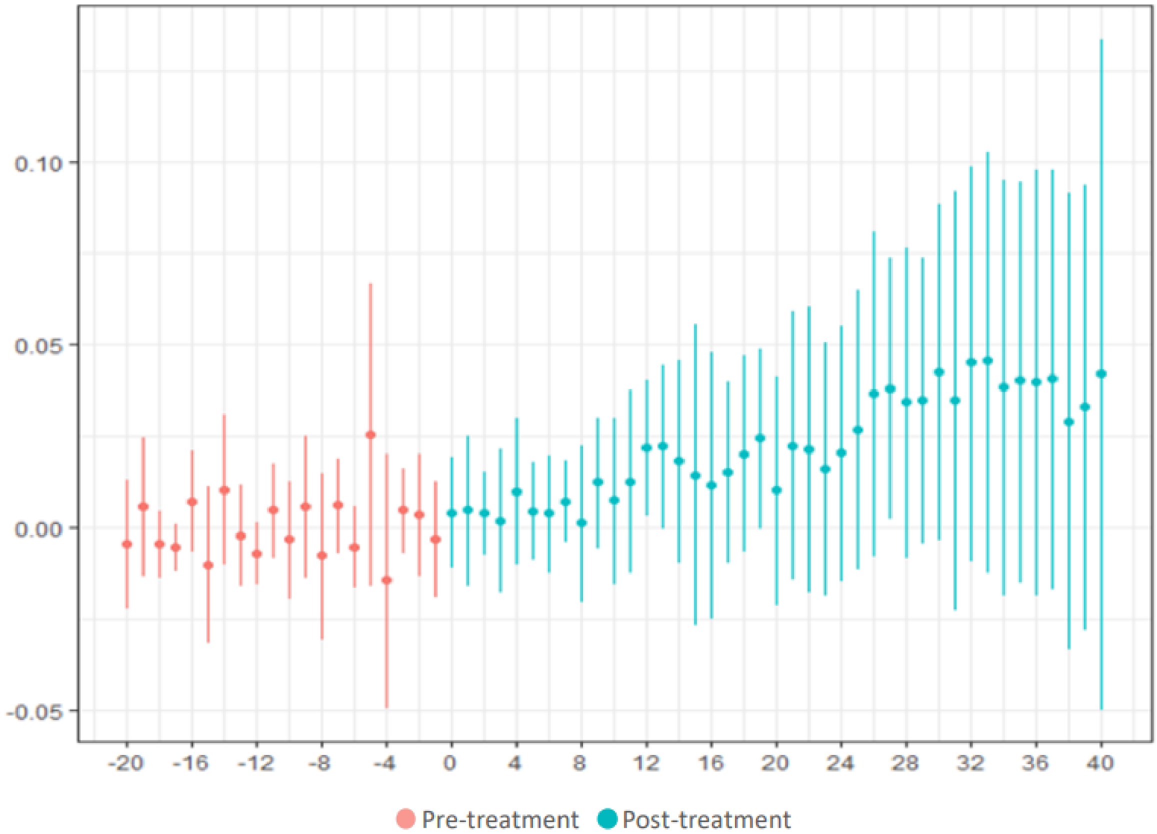
Note: The x-axis is the length of exposure to the treatment. A length of exposure equal to 0 corresponds to the average effect of implementing macroprudential policy across groups in the period they first implement macroprudential policy; equal to -1 corresponds to the period before groups implement macroprudential policy and equal to 1 corresponds to the first period after initial implementation.

Figure 1.2: Dynamic Impact of MaPP on Firm Investment Rate, 2000-2019



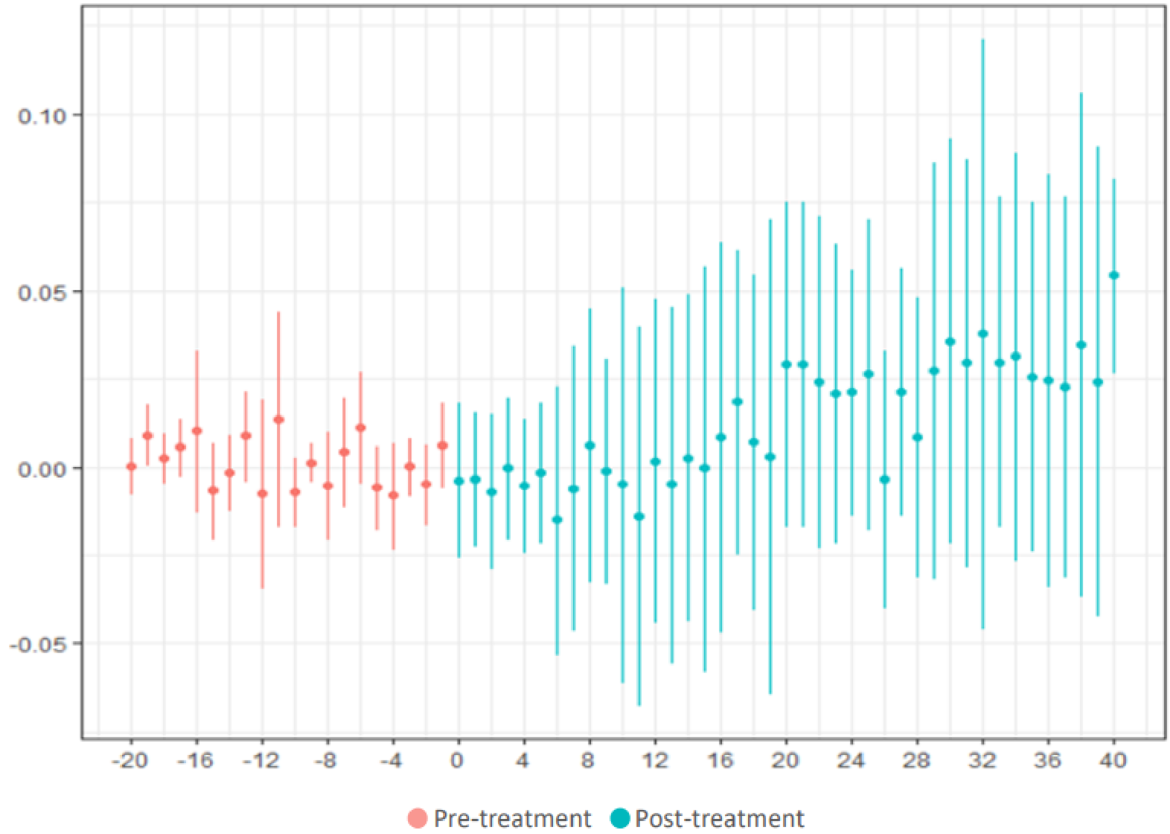
Note: The x-axis is the length of exposure to the treatment. A length of exposure equal to 0 corresponds to the average effect of implementing macroprudential policy across groups in the period they first implement macroprudential policy; equal to -1 corresponds to the period before groups implement macroprudential policy and equal to 1 corresponds to the first period after initial implementation.

Figure 1.3: Dynamic Impact of MaPP on Household Savings Rate, Restricted Sample, 2000-2019



Note: The x-axis is the length of exposure to the treatment. A length of exposure equal to 0 corresponds to the average effect of implementing macroprudential policy across groups in the period they first implement macroprudential policy; equal to -1 corresponds to the period before groups implement macroprudential policy and equal to 1 corresponds to the first period after initial implementation.

Figure 1.4: Dynamic Impact of MaPP on Firm Investment Rate, Restricted Sample, 2000-2019



Note: The x-axis is the length of exposure to the treatment. A length of exposure equal to 0 corresponds to the average effect of implementing macroprudential policy across groups in the period they first implement macroprudential policy; equal to -1 corresponds to the period before groups implement macroprudential policy and equal to 1 corresponds to the first period after initial implementation.

Table 1.2: Impact of MaPP on Household Savings Rate, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0194**	0.0232**	0.0241**	0.0187**
Standard Error	0.0053	0.0060	0.0062	0.0059
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0212**	0.0119	0.0302**	0.0363**
Standard Error	0.0080	0.0085	0.0153	0.0180
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0224	0.0293*	0.0332*	0.0064
Standard Error	0.0171	0.0167	0.0197	0.0221

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on savings across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.3: Impact of MaPP on Firm Investment Rate, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0074**	0.0006	0.0062	0.0068**
Standard Error	0.0030	0.0034	0.0050	0.0021
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0021	0.0165**	0.0591**	0.0505*
Standard Error	0.0031	0.0053	0.0260	0.0274
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0534**	0.0575**	0.0525**	0.0663**
Standard Error	0.0231	0.0239	0.0238	0.0262

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on investment across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on investment over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.4: Impact of MaPP on Household Savings Rate, LTV, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0339**	0.0413**	0.0153	0.0331**
Standard Error	0.0065	0.0050	0.0120	0.0072
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0411**	0.0079	0.0425**	0.0528**
Standard Error	0.0068	0.0163	0.0217	0.0189
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0086	0.0418*	0.0529**	0.0474**
Standard Error	0.0243	0.0221	0.0196	0.0183

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on savings across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.5: Impact of MaPP on Household Savings Rate, Loan Restrictions, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0064	0.0033	0.0243	0.0066
Standard Error	0.0090	0.0178	0.0396	0.0090
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0035	0.0260	0.0055	0.0002
Standard Error	0.0173	0.0360	0.0106	0.0239
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0264	0.0056	0.0004	0.0281
Standard Error	0.0512	0.0108	0.0247	0.0477

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on savings across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.6: Impact of MaPP on Household Savings Rate, DSTI, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0764**	0.0849**	0.0874**	0.0764**
Standard Error	0.0076	0.0169	0.0122	0.0071
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0849**	0.0874**	0.0764**	0.0849**
Standard Error	0.0225	0.0202	0.0076	0.0169
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0874**	0.0764**	0.0849**	0.0474**
Standard Error	0.0122	0.0071	0.0225	0.0202

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on savings across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.7: Impact of MaPP on Firm Investment Rate, LTV, 2000-2019

Model	I	II	III	IV
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0383**	0.0350**	0.0376**	0.0386**
Standard Error	0.0034	0.0041	0.0041	0.0037
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0351**	0.0385**	0.0817**	0.0787**
Standard Error	0.0051	0.0035	0.0243	0.0219
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0828**	0.0820*	0.0787**	0.0835**
Standard Error	0.0281	0.0232	0.0217	0.0285

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on investment across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.8: Impact of MaPP on Firm Investment Rate, Loan Restrictions, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0045	0.0054	0.0026	0.0045
Standard Error	0.0039	0.0048	0.0034	0.0042
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0054	0.0026	0.0045	0.0054
Standard Error	0.0037	0.0035	0.0039	0.0048
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0026	0.0045	0.0054	0.0026
Standard Error	0.0034	0.0042	0.0037	0.0035

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on investment across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on investment over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.9: Impact of MaPP on Firm Investment Rate, DSTI, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0059	-0.0189*	0.0394**	0.0060
Standard Error	0.0052	0.0101	0.0028	0.0046

Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	-0.0192*	0.0389**	0.0088	-0.0146
Standard Error	0.0113	0.0029	0.0093	0.0162

Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0299**	0.0090	-0.0148	0.0294**
Standard Error	0.0131	0.0107	0.0167	0.0138

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on investment across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.10: Impact of MaPP on Household Consumption to GDP, 2000-2019

Model	I	II	III	IV
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	-0.0067*	-0.0084**	-0.0104**	-0.0050
Standard Error	0.0036	0.0033	0.0037	0.0033
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	-0.0053*	-0.0037	-0.0247**	-0.0283**
Standard Error	0.0030	0.0069	0.0108	0.0117
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	-0.0156	-0.0220**	-0.0232**	-0.0037
Standard Error	0.0137	0.0109	0.0110	0.0169

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on consumption to GDP across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on consumption to GDP over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.11: Impact of MaPP on NFC GFCF to GDP, 2000-2019

Model	I	II	III	IV
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0036	0.0003	0.0038	0.0039
Standard Error	0.0025	0.0022	0.0038	0.0027
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0020	0.0103**	0.0434**	0.0395**
Standard Error	0.0022	0.0035	0.0138	0.0176
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0428**	0.0435**	0.0417**	0.0498**
Standard Error	0.0143	0.0175	0.0141	0.0167

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on NFC GFCF to GDP across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on NFC GFCF to GDP over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.12: Impact of MaPP on Household Savings Rate, Restricted Sample, 2000-2019

Model	I	II	III	IV
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0204**	0.0274**	0.0238*	0.0196**
Standard Error	0.0058	0.0065	0.0072	0.0063
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0270**	0.0096	0.0400**	0.0530**
Standard Error	0.0078	0.0068	0.0184	0.0145
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0313**	0.0389**	0.0510**	0.0106
Standard Error	0.0160	0.0183	0.0152	0.0198

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on savings across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Table 1.13: Impact of MaPP on Firm Investment Rate, Restricted Sample, 2000-2019

Model	**I**	**II**	**III**	**IV**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Not Yet Treated	Not Yet Treated	Never Treated
Estimation Method	Doubly Robust	Regression	Regression	Doubly Robust
Aggregation Method	Group	Group	Group	Group
Covariates	-	Dependency Ratio	GDP per capita	-
ATT	0.0112**	0.0091**	0.0062*	0.0096**
Standard Error	0.0026	0.0032	0.0032	0.0015
Model	**V**	**VI**	**VII**	**VIII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Never Treated	Never Treated	Not Yet Treated	Not Yet Treated
Estimation Method	Regression	Regression	Doubly Robust	Regression
Aggregation Method	Group	Group	Dynamic	Dynamic
Covariates	Dependency Ratio	GDP per capita	-	Dependency Ratio
ATT	0.0089**	0.0114*	0.0822**	0.0770**
Standard Error	0.0021	0.0037	0.0237	0.0196
Model	**IX**	**X**	**XI**	**XII**
Period	2000-2019	2000-2019	2000-2019	2000-2019
Treatment Group	Not Yet Treated	Never Treated	Never Treated	Never Treated
Estimation Method	Regression	Doubly Robust	Regression	Regression
Aggregation Method	Dynamic	Dynamic	Dynamic	Dynamic
Covariates	GDP per capita	-	Dependency Ratio	GDP per capita
ATT	0.0678**	0.0776**	0.0748**	0.0793**
Standard Error	0.0251	0.0237	0.0216	0.0257

Note: The table reports the aggregated group treatment effect (ATT(g,t)) parameters estimated as in equation (1.7) to evaluate the impact of MaPP on investment across groups. The aggregated dynamic treatment effect parameters estimated as in equation (1.9) are also reported to examine the impact of MaPP on savings over time. ATT(g,t) is the average treatment effect experienced by group g in time t. Statistical significance is assessed using clustered bootstrapped standard errors at the country level that account for the autocorrelation of the data. "**" represents statistical significance at a 5% level and "*" represents statistical significance at a 10% level.

Chapter 2

The Real Effects of Macroprudential Policy: Evidence from Micro Data

2.1. Introduction

In recent years there has been increasing interest on the effects of macroprudential policy (MaPP). These papers emphasize that MaPP constrains credit with searing consequences on growth. But while this result is intuitively appealing, it offers little help to central banks wishing to steer the economy. The more pertinent question to ask should be: how does MaPP affect different people over time and why? If MaPP has a different impact on the poor than it does on the wealthy, its effects on the real economy may differ greatly across countries. And yet this question has received little attention.

In this paper, I tackle this question from quite a different angle by using individual-level data. I focus on savings and borrowing behavior for two reasons. First, MaPP restricts access to credit. As a result, people can either adjust savings or borrowing to meet the prudential requirements. Second, individual data on savings and borrowing is available for most countries in the World Bank's Global Financial Inclusion (Findex) database. This database also contains information on individual attributes like age, education, gender and income. These attributes are often screened in loan applications but they have been too easily overlooked in the literature.

The empirical design of this paper seeks to address the endogeneity of the MaPP choice. I begin by examining whether MaPP is an important determinant of savings and borrowing using multivariate tests that control for individual and country characteristics. The initial results suggest that an individual subject to MaPP is more likely to save and less likely to borrow. A potential concern about these results is that they may be driven by significant changes in credit and asset prices. I address this issue by matching every person subject to MaPP to a similar person not subject to MaPP. The matching increases the odds of saving by .69 and decreases the odds of borrowing by .21. These estimates could still be biased due to "unobservables", such as cultural differences across countries. To mitigate this concern, I also use central bank independence (CBI) as an instrument for differences in MaPP across countries. As we shall see, the results remain broadly similar.

Next, I examine three factors that may account for differences in the effects of MaPP. I look at the design of the policy tools, the interest rate and the country income level. A number of policy implications emerge. The first is that some tools are better suited to encourage savings, while others are more effective at reducing borrowing. The choice of tool has different consequences for economic growth. The second implication is that changes in the interest rate have stronger effects on savings than on borrowing, at least, when MaPP is tight. The final implication is that MaPP is a more effective tool to push savings up in emerging economies and bring borrowing down in advanced economies. Taken together, these findings show that the effects of MaPP on the real economy depend crucially on the incentives of people to save and borrow.

The contribution of this paper is twofold. First, it identifies the transmission channels of MaPP to the real economy. Previous papers use either general equilibrium models to determine the impact of MaPP on output (e.g., Bastos et al., 2018; Nakatani, 2020) or empirical methods to estimate its effects on private spending (e.g., Lim et al., 2011; Akinci and Olmstead-Rumsey, 2015; Cronin and McQuinn, 2016; Cerutti et al., 2017; Teixeira and Venter, 2023). While these papers are focused on aggregate measures of credit and output, I provide direct empirical evidence on the effects of MaPP on individual behavior. This allows me to examine how differences between people affect the transmission of MaPP within and across countries. Second, this paper adds to the emerging literature on the redistributive effects of MaPP. A handful of papers find that MaPP leads to a reallocation of credit from low to high-income individuals (e.g., Frost, 2018; Acharya et al., 2020; DeFusco et al., 2020). I continue this line of research by documenting the savings and borrowing behavior of individuals across income levels.

The paper proceeds as follows. Section 2.2 describes the data and reports the baseline results for the effects of MaPP on savings and borrowing. Section 2.3 evaluates the robustness of the results using propensity score matching (PSM) and an instrumental variable (IV) approach. Section 2.4 further disaggregates the effects of MaPP according to the policy tool, interest rate, and country income level. Section 2.5 concludes with policy implications.

2.2. Empirical Analysis

2.2.1. Multivariate Tests

I estimate the effects of MaPP on savings and borrowing while controlling for individual and country characteristics, as follows:

$$\mathrm{S}_{\mathrm{i,j,t}} = \alpha + \beta \mathrm{MaPP}'_{\mathrm{i,j,t-1}} + \gamma \mathrm{X}'_{\mathrm{i,j,t}} + \delta_{\mathrm{j}} + \theta_{\mathrm{t}} + \varepsilon_{\mathrm{i,t}} \tag{2.1}$$

$$\mathrm{B}_{\mathrm{i,j,t}} = \alpha + \beta \mathrm{MaPP}'_{\mathrm{i,j,t-1}} + \gamma \mathrm{X}'_{\mathrm{i,j,t}} + \delta_{\mathrm{j}} + \theta_{\mathrm{t}} + \varepsilon_{\mathrm{i,t}} \tag{2.2}$$

where $\mathrm{S}_{\mathrm{i,j,t}}$ and $\mathrm{B}_{\mathrm{i,j,t}}$ are binary variables that equal one when a person i decides to save or borrow in country j at time t, respectively; $\mathrm{MaPP}'_{\mathrm{i,j,t-1}}$ is a vector of binary variables that equals one when a person lives in a country that adopts MaPP in the previous year; $\mathrm{X}'_{\mathrm{i,j,t}}$ is a set of individual attributes that may influence access to credit, which include age, education, gender and income (e.g., Berger et al., 1999; Stein, 2002; Petersen and Rajan, 2002; Ravina, 2019); δ_{j} and θ_{t} are country and year fixed effects to control for country-level differences and time-varying country-specific effects, respectively; and $\varepsilon_{\mathrm{i,t}}$ is the error term. As standard in the literature, the coefficients are estimated using a generalized linear model (GLM) with a logit function. All coefficients are reported in the logit scale[1]. Statistical significance is assessed using standard errors corrected for heteroscedasticity and clustered at the country-year level.

The data is collected from several sources. The binary variables on the decision to save and borrow as well as the individual attributes are taken from Findex. This database is based on surveys conducted in 2011, 2014 and 2017 that cover approximately 450,000 adults age 15 and above in 140 countries. I exclude from the analysis every person that "does not know" or "refuses" to answer the survey[2]. To determine if a person is exposed to MaPP, I cross the

[1]The coefficients are reported as the natural log of the odds. They can be expressed in terms of odds after exponentiating them.

[2]This corresponds to only 2 to 10 percent of the answers depending on the question.

individual data from Findex with the country data on macroprudential policy from Alam et al. (2019)[3]. In line with the literature, I focus on borrower-based measures that directly target individuals, namely the loan-to-value (LTV) ratio and the debt-service-to-income (DSTI) ratio. The final sample consists of 360,567 individuals from 122 countries. Table 2.1 provides descriptive statistics for the full sample. Later on, I break down these statistics into treated and untreated groups, i.e., people exposed to MaPP or not.

2.2.2. Baseline Results

Table 2.2 reports the baseline results for both regressions. The coefficient of MaPP on overall savings is positive and significant at the 1% level. By comparison, MaPP seems to have little effect on formal savings. This modest impact on formal savings is of particular importance in that it shows that MaPP influences the decision to save regardless of whether people save in banks or not. There are two plausible explanations for this. One is that people hold on to money when MaPP is tight. If MaPP sends a signal that banks are in trouble, people may prefer cash to savings. Another possibility is that my estimations understate the effects of MaPP on formal savings. This may happen because MaPP is usually tightened when banks are on the hook for losses. To cast light on how MaPP affects formal savings, one must explicitly control for the endogeneity in the MaPP choice. This is carefully done in Sections 2.3.1 and 2.3.2.

Turning next to borrowing, the MaPP coefficients are negative and significant at the 1% level. Although the effects of MaPP on formal borrowing are relatively strong, its effects on overall borrowing are fairly marginal. These results suggest that people borrow less from financial institutions but not necessarily from informal moneylenders, family and friends. So far, the regression models cannot provide a conclusive answer about whether people are shifting from formal to informal channels of lending. As I am going to argue, this choice depends to a large extent on the policy tool, the interest rate and the country income level. These factors are analyzed in more detail in Section 2.4.

The coefficients of the individual attributes are all significant and empirically plausible. Not surprisingly, a few individual characteristics play a significant role in explaining the behavior of people after the adoption of MaPP. Specifically, higher levels of education and income are associated with a greater propensity to save. Neither age nor gender have much of an effect on savings. At the same time, people with higher income borrow more but women

[3]The data on MaPP adoption is missing for 18 countries, which are removed from the analysis.

and more educated people borrow less. These results point to a potential trade-off between financial stability and financial inclusion.

Overall, the regression models suggest that MaPP is an important determinant of savings and borrowing. This simple analysis also shows that MaPP may shift savings and borrow from formal to informal channels and that a more sophisticated understanding of the effects of MaPP is required. The next section attempts to establish robustness using PSM and an IV model.

2.3. Robustness Checks

2.3.1. PSM

An obvious limitation of regressions is that the MaPP choice may be endogenous. Put differently, countries usually implement MaPP in response to severe swings in credit and asset prices. The ideal experiment would be to compare a person's decision to save or borrow to the same decision had this person not been exposed to MaPP. That is, to estimate the average treatment effect of MaPP on the treated persons (ATT), as follows:

$$\mathrm{ATT} = \mathrm{E}[\mathrm{Y_{i1}}|\mathrm{D_i} = 1] - \mathrm{E}[\mathrm{Y_{i0}}|\mathrm{D_i} = 1] \tag{2.3}$$

where $\mathrm{D_i}$ is a binary treatment variable set to one when a person is in a country that adopts MaPP in the previous period; $\mathrm{Y_{i1}}|\mathrm{D_i} = 1$ is the saving or borrowing decision observed for every person in the country adopting MaPP; and $\mathrm{Y_{i0}}|\mathrm{D_i} = 1$ is the decision that would have been observed had the person not been subject to MaPP.

The problem is that $\mathrm{Y_{i0}}|\mathrm{D_i} = 1$ is never observable. On top of that, it is difficult to find an empirical proxy for the hypothetical decision had the person not been exposed to MaPP. The reason is that MaPP targets the entire population of a country. As a consequence, every person is assigned to the treatment group. Furthermore, the effects of MaPP are a function of individual attributes. Therefore, comparing the sample mean of the treated group with that of the control group may bias the estimates.

My main strategy, then, is to use PSM to construct a control group that mimics a randomized experiment (e.g., Rosenbaum and Rubin, 1983; Imbens, 2004). To each person in a country with MaPP, I assign a person in a country without MaPP with the closest propensity score, where this score is the probability that a person's decision to save or borrow is affected by MaPP conditional on the same set of observable control variables, $\mathrm{X_i}$.

Each person's propensity score is estimated using a simple logistic regression. Under this assumption, equation (2.3) can be rewritten as following:

$$\mathrm{ATT} = \mathrm{E}[\mathrm{Y_{i1}}|\mathrm{D_i} = 1, \mathrm{X_i}] - \mathrm{E}[\mathrm{Y_{i0}}|\mathrm{D_i} = 0, \mathrm{X_i}] \tag{2.4}$$

where $\mathrm{E}[\mathrm{Y_{i0}}|\mathrm{D_i} = 1]$ was replaced with $\mathrm{E}[\mathrm{Y_{i0}}|\mathrm{D_i} = 0, \mathrm{X_i}]$, which is now observable. Subsequently, the ATT can be estimated using the following equation:

$$\mathrm{ATT} = \mathrm{E}[\mathrm{Y_{i1}}|\mathrm{D_i} = 1, \mathrm{p(X_i)}] - \mathrm{E}[\mathrm{Y_{i0}}|\mathrm{D_i} = 0, \mathrm{p(X_i)}] \tag{2.5}$$

I use two different classes of matching methods: nearest-neighbor matching (NNM) and coarsened exact matching (CEM)[4]. The most common form of matching is 1:1 NNM without replacement, which matches each treated unit to the control unit with the closest propensity score. The caveat is that I drop several unmatched control units from the adjusted sample[5]. For robustness, I repeat the ATT estimations using 3:1 NNM without replacement and CEM. The latter is a form of stratum matching that coarsens covariates into six bins and performs exact matching on the coarsened versions[6]. The distributional balance across matching methods is quite similar.

I then perform several tests to check the quality of the matches. Tables 2.3 and 2.4 report the means in the control and treatment groups before and after matching. The tables also report standardized mean differences (SMD), variance ratios, and statistics on the empirical cumulative density functions (eCDF). The values of SMD and eCDF are nearly zero and variance ratios are close to one, which confirms the excellent covariate balance between treated and control groups.

Table 2.5 summarizes the treatment effects of MaPP using PSM. The coefficients are broadly consistent with the GLM models. In fact, they are slightly larger. To interpret the magnitude of the results, I can compute the odds of saving and borrowing. A person subject to MaPP is $(e^{0.522} - 1) \times 100 \approx .69$ more likely to save than a person not exposed to MaPP. The odds of formal saving decrease by .38. Both results are statistically significant at the 1% level. Regardless of the matching method, the odds of a person saving are always significantly higher after the adoption of MaPP. These results are rather encouraging, suggesting as they do that MaPP is an effective tool to influence savings.

[4]The discussion on NNM and CEM is kept to a minimum in the body of the paper. A more detailed discussion is provided in the table footnotes.

[5]For $n > 1$, the matches after the first match are generally worse in terms of closeness to the treated unit. The precision gains usually dissipate after $n = 3$ (e.g., Austin, 2010; Rosenbaum, 2020).

[6]CEM balances the entire joint distribution of covariates and achieves higher matching precision at the expense of higher variance across treatment and control groups.

As for borrowing, the results from PSM point to a much stronger impact than previously suggested decreasing the odds of borrowing after MaPP adoption by .21. Interestingly, there is only a small difference between borrowing and formal borrowing. This indicates that MaPP affects mainly formal channels of lending. On closer examination, though, these generalizations are far less clear. If a country has a high interest rate or an underdeveloped financial system, MaPP may shift borrowing from formal to informal channels. This issue is further explored in Section 2.4.

The point I wish to make here is that the MaPP coefficients are significantly higher for savings than for borrowing. This is crucial because there is a widely held belief that the credit restrictions imposed by MaPP explain all the variability in output. The results presented here suggest another possibility: MaPP triggers a surge in savings. This may happen for two reasons. One is that people may expect tighter credit conditions after the adoption of MaPP. Two is that people are more likely to entrust financial institutions with their money when MaPP is tight. Whatever the reason, one thing is clear: MaPP has stronger effects on savings than on borrowing and most of these savings flow into the financial system.

Up to this point, multivariate tests and PSM confirm that MaPP increases savings and reduces borrowing. More than that, MaPP has stronger effects on formal financial channels. The main limitation of PSM is that it rests on the assumption that people can be matched based on observable characteristics. However, people may differ across unobservable characteristics, such as cultural beliefs. To the extent that cultural differences explain individual behavior, the MaPP coefficients may be biased. The next step, then, is to use an IV approach to mitigate any concerns about estimation bias.

2.3.2. IV Model

The identification challenge is to find an exogenous variation in MaPP, which can be used as an instrument that is uncorrelated with the individual decision to save or borrow. I argue that CBI can be used as an instrument for differences in MaPP across countries. I say this for three reasons. First, a more independent central bank is more likely to implement MaPP, particularly borrower-based measures (e.g., Lim et al., 2011; Masciandaro and Volpicella, 2016). Second, CBI should not affect credit, at least, directly. Lastly, CBI is predetermined regardless of changes in the real economy. I use the CBI index proposed by Romelli (2022). The index ranges between 0 (no independence) and 1 (full independence). The (unreported) results from the first-stage IV regression confirm that CBI is a valid instrument: the MaPP coefficient is highly positive (.679) and significant at the 1% level (t-value = 27.16).

Table 2.6 reports the estimations from the IV model[7]. The odds of saving increase to 1.91 and, more importantly, the odds of formal saving soar to 5.32. In contrast, the odds of borrowing decrease by .62 and for formal borrowing by .76. The coefficients are highly significant and of the expected sign but somewhat high when compared to the estimates from GLM and PSM. The reason is that the IV model estimates local average treatment effects, which correspond to the effects of MaPP on the set of people directly affected by CBI. The IV model is also the only model that estimates a lower MaPP coefficient for overall borrowing than for formal borrowing. This makes sense because independent central banks usually implement MaPP in conjunction with other measures of creditworthiness. Thus, the impact of MaPP on individual behavior should be lower.

Taken together, the results from GLM, PSM and the IV model confirm that people save more and borrow less after the adoption of MaPP. This means that MaPP enhances financial stability by reducing leverage but also by encouraging savings. In what follows, I extend the analysis to look at the effects of individual policy tools, the interest rate, and the country's income level.

2.4. Extensions

2.4.1. Policy Tools

I start by separating the effects of the LTV ratio from the DSTI ratio. To do so, I create a dummy variable that equals one when a country implements an LTV ratio in the previous year and zero otherwise. I create a similar dummy for the DSTI ratio. Then, I repeat the estimations on each of the dummies using GLM and PSM.

Table 2.7 examines the response of people to LTV and DSTI ratios. The LTV ratio increases the odds of saving by .68. Conversely, the DSTI ratio reduces the probability of saving by .45. As expected, both tools have a negative effect on borrowing. For comparison purposes, the LTV and the DSTI ratio drop the odds of borrowing by .18 and .49, respectively. In a way these results are surprising, but not too much. If the DSTI ratio limits debt service payments relative to disposable income, it may eliminate any excessive savings of borrowers. The LTV ratio has exactly the opposite effect on savings. By stipulating a down payment for credit approval, it induces people to save. Both tools may be used to promote financial stability but they have very different implications for growth.

[7]Data on CBI is not available for the following countries: Armenia, Bhutan, El Salvador, Honduras, Israel, Lesotho, Mozambique, Serbia, Sudan and Tajikistan. These countries are dropped from this analysis.

The DSTI ratio might be useful to prevent debt from spiraling out of control. However, it is less effective to ensure liquidity in financial institutions. If the goal is to improve liquidity, the LTV ratio is more appropriate. Sure enough, the effects of MaPP also depend on the interest rate and the country's income level to which I turn next.

2.4.2. Interest Rate

I now examine the behavior of savings and borrowing when countries tighten both macroprudential and monetary policies. I create an additional treatment variable that equals one when a country tightens MaPP and it has an interest rate above the sample median. This identifies the set of countries with a tight overall policy stance.

Table 2.8 displays the response of savings and borrowing when MaPP works together with the interest rate. The comparison with the baseline results is instructive. The sign and magnitude of the coefficients are remarkably similar: people still save more and borrow less when the interest rate is aligned with MaPP. The only difference is that the coefficients for formal savings are now higher in a manner consistent with economic theory. Not surprisingly, people stash their savings in financial institutions when the interest rates are high. On the contrary, the odds of borrowing are marginally lower than the baseline results. This suggests that the interest rate has only modest effects on borrowing when access to credit is already restricted by MaPP.

The bottom line, then, is that MaPP is a more effective tool to prevent excessive leverage, while the interest rate is the most effective tool to influence savings. In many, if not most cases when MaPP is tight, increasing the interest rate will have little impact on borrowing but serious consequences on savings.

2.4.3. Country Income Level

This last section splits the sample into advanced and emerging economies. The group of advanced economies includes all the countries classified as "high-income" by the World Bank. If a country is classified as "middle" or "low" income, it is assigned to the group of emerging economies.

Table 2.9 shows the effects of MaPP disaggregated by advanced and emerging economies. The impact is notably stronger in emerging economies, especially on savings. In particular, MaPP increases the odds of saving by .78. Much of this increase goes into the financial system. By contrast, the odds of borrowing decrease by .10 and can go down by as much as

.25 for formal borrowing. The fact that MaPP hits mostly formal borrowing suggests that people are able to shift from formal to informal channels of lending in emerging economies.

In advanced economies, though, the impact of MaPP on savings is relatively mild. The probability that a person saves increases by only .16. Indeed, the odds are almost zero for formal savings. This means that part of the savings is being kept outside the financial system. This result is supportive of the argument that the interest rate has a more important role on savings when credit is already restricted by MaPP. As for borrowing, the impact of MaPP is hard to overstate. The estimates show that MaPP decreases the chances of borrowing by .30. The effects are similar for formal channels of borrowing.

In sum, the impact of MaPP on advanced and emerging economies is quite different. While MaPP reduces leverage in advanced economies, it promotes savings in emerging economies. People in emerging economies seem to circumvent the prudential requirements by shifting from formal to informal channels of lending. The final section attempts to draw some useful guidance for policy.

2.5. Conclusions

This paper draws three substantive policy conclusions. The first is that people save more and borrow less after the adoption of MaPP. A crucial point is that the effects of MaPP on credit, while apparent, tell only part of the story. The combined evidence presented here suggests that savings are also an important channel through which MaPP affects growth.

The second is that the effects of MaPP depend crucially on the incentives of people to save and borrow. This explains why the effects of MaPP differ greatly across countries. In general, the adoption of MaPP reduces leverage in advanced economies and promotes savings in emerging economies. But these effects also depend on the design of the policy tool. If the goal is to increase savings with little impact on borrowing, then policy tools that establish higher collateral requirements may be more effective. If, instead, the goal is to reduce leverage, then policy tools limiting the amount of debt to income appear to be most useful. The right balance of policies must take into account the existing stock of savings and credit in the economy.

The final conclusion is that a tough interest rate policy is not a surefire guarantee against excessive leverage. My analysis suggests that, when MaPP is too tight, an increase in the interest rate is not helpful to reduce leverage. If nothing else, a higher interest rate will only encourage people to save. This will slow down growth in a limping economy. A simple rule of thumb is that MaPP is a more effective tool to curb credit, while the interest rate is a more appropriate tool to bring liquidity to the financial system.

There are, of course, a number of potential ways to improve this analysis. While I describe the average effects of MaPP using broad samples in a fairly systematic way, I am unable to isolate the effects of some individual country factors that could drive the results. For example, my results suggest that MaPP has a stronger impact on savings than on borrowing and that these effects are especially prevalent in emerging economies. However, I am unable to explain why. A few potential explanations include differences in tax systems, house prices and financial development. The impact of these factors is probably better assessed at the country level or using a relatively small panel of countries.

Also, my analysis is mainly focused on the short-run behavior of people. The results forcibly show that MaPP hinders growth in the short run. Nonetheless, MaPP may support growth in the long run by smoothing the business cycle. Future work could track individuals over time and examine their behavior over longer periods. Ideally, this work would also look at firms. If my story is right in its essentials, the higher savings and lower leverage should foster investment in the long run.

Table 2.1: Summary Statistics

Variable	Obs.	Mean	SD	Min	25th Pctl.	Median	75th Pctl.	Max
Savings								
Overall	358,863	0.490	0.500	0	0	0	1	1
Formal	285,760	0.332	0.471	0	0	0	1	1
Borrowing								
Overall	360,567	0.417	0.493	0	0	0	1	1
Formal	357,596	0.129	0.335	0	0	0	0	1
Person Characteristics								
Age	359,283	42.509	17.803	15	28	40	56	99
Education	358,540	1.877	0.713	1	1	2	2	3
Female	360,567	0.54	0.498	0	0	1	1	1
Income	360,551	3.197	1.421	1	2	3	4	5
Country Characteristics								
GDPpc	337,451	14,805.193	18,806.482	315.657	2,449.958	6,304.193	19,393.007	107,099.048
MaPP	360,567	0.037	0.188	0	0	0	0	1
MaPP-MP	265,467	0.03	0.171	0	0	0	0	1

Note: The table presents summary statistics for the sample. “Overall Savings” and“Overall Borrowing” are dummy variables that equal one if the respondent saved or borrowed in the twelve months after the adoption of MaPP, respectively. “Formal Savings” and “Formal Borrowing” are dummy variables that equal one if the respondent saved or borrowed using an account at a bank or another type of financial institution. “Age” is the respondent’s age. “Education” is the highest completed level of education ranging from 1 (primary education or less) to 3 (tertiary education or more). “Income” is the within-economy household income quintile of each respondent where income includes wages and salaries, remittances from members living elsewhere, and all other possible income sources. “GDP per capita” is the gross domestic product in constant 2015 U.S. dollars divided by midyear population. “MaPP” is a dummy variable coded one when the respondent is in a country that implements an LTV or DSTI ratio in the twelve months prior to the survey. Finally, “MaPP-MP” is also a dummy variable that equals one when “MaPP” is one and the interest rate of the country is above the sample median, which is approximately 3%. All variables are winsorized at the 5% and 95% levels.

Table 2.2: Effects of MaPP on Savings and Borrowing, GLM

	Savings		**Borrowing**	
	Overall	**Formal**	**Overall**	**Formal**
Intercept	-2.621*	-2.907*	-0.551*	-3.102*
	(0.046)	(0.072)	(0.042)	(0.070)
MaPP	0.315*	-0.024	-0.043‡	-0.134*
	(0.026)	(0.030)	(0.025)	(0.037)
Female	-0.148*	-0.189*	-0.182*	-0.208*
	(0.007)	(0.010)	(0.007)	(0.010)
Age	-0.004*	-0.0003*	-0.011*	-0.004*
	(0.000)	(0.000)	(0.000)	(0.000)
Education	0.424*	0.575*	0.160*	0.316*
	(0.007)	(0.008)	(0.006)	(0.008)
Income	0.247*	0.275*	0.010*	0.088*
	(0.003)	(0.004)	(0.003)	(0.004)
Obs.	357,606	284,862	359,255	356,377
Adj. R^2	0.14	0.24	0.05	0.06

Note: The table reports coefficient estimates from the generalized linear model (GLM) with country and year fixed effects. The estimated coefficients are a logit, i.e., the log of the odds. Standard errors are clustered at the country-year level and are shown in parentheses. Standard errors are clustered at the country-year level and are shown in parentheses. "*", "†" and "‡" represent statistical significance at the 1%, 5% and 10% level, respectively.

Table 2.3: Balance of Unmatched Sample

	Means Treated	Means Control	SMD	Var. Ratio	eCDF Mean	eCDF Max
Distance	0.046	0.044	0.207	0.827	0.078	0.148
Female	0.539	0.534	0.009	-	0.005	0.005
Age	44.726	42.420	0.134	0.953	0.028	0.085
Education	1.849	1.889	-0.055	1.077	0.010	0.045
Income	3.162	3.244	-0.057	1.029	0.017	0.032
GDPpc	18,971.972	15,802.474	0.183	0.757	0.151	0.418

Note: The table describes the balance in the sample prior to matching. The treatment is a binary variable, "MaPP", that equals one if a country tightens MaPP in the twelve months prior to a person's decision to save or borrow. The sample is composed of 11,753 person-level treated observations and 253,714 untreated observations. The SMD is the difference in the means of each covariate across groups standardized by the pooled standard deviation. The variance ratio is the ratio of the variance of a covariate in one group to that in the other. The variance ratio is not computed for "Female" because it is a function of the prevalence in each group, which is captured by SMD and eCDF statistics. The mean and maximum differences in the empirical cumulative density functions (eCDFs) between groups are also reported for each covariate.

Table 2.4: Balance of Adjusted Sample Across Matching Methods

	Means Treated	Means Control	SMD	Var. Ratio	eCDF Mean	eCDF Max
1:1 NNM						
Distance	0.046	0.046	0	1	0	0.001
Female	0.539	0.507	0.063	-	0.031	0.031
Age	44.726	46.715	-0.115	0.869	0.023	0.067
Education	1.849	1.874	-0.035	1.011	0.005	0.020
Income	3.162	3.093	0.048	1.025	0.014	0.029
GDPpc	18,971.972	17,879.610	0.063	0.871	0.121	0.339
3:1 NNM						
Distance	0.046	0.046	0	1	0	0.001
Female	0.539	0.522	0.034	-	0.017	0.017
Age	44.726	46.597	-0.108	0.888	0.022	0.059
Education	1.849	1.833	0.021	1.063	0.005	0.018
Income	3.162	3.105	0.040	1.027	0.012	0.026
GDPpc	18,971.972	17,262.888	0.099	0.868	0.119	0.353
CEM						
Distance	0.046	0.046	-0.043	0.782	0.005	0.016
Female	0.539	0.540	-0.003	-	0.002	0.002
Age	44.726	45.686	-0.056	0.911	0.012	0.036
Education	1.849	1.835	0.019	1.075	0.006	0.020
Income	3.162	3.134	0.019	1.021	0.006	0.018
GDPpc	18,971.972	18,669.768	0.018	0.647	0.132	0.358

Note: The table presents the balance for the adjusted sample using 1:1 NNM, 3:1 NNM and CEM. The propensity scores are estimated using logistic regressions. All treated persons were matched to a similar untreated person in the control group. The SMD is the difference in the means of each covariate across groups standardized by the pooled standard deviation. The variance ratio is the ratio of the variance of a covariate in one group to that in the other. The variance ratio is not computed for “Female” because it is a function of the prevalence in each group, which is captured in the mean difference and eCDF statistics. The values of SMD and eCDF statistics are close to zero and the variance ratios are close to one, which suggests an excellent sample balance.

Table 2.5: Treatment Effects of MaPP on Savings and Borrowing, PSM

	Savings		Borrowing	
Panel A: Without Covariates	**Overall**	**Formal**	**Overall**	**Formal**
1:1 NNM	0.465*	0.261*	-0.147*	-0.203*
	(0.028)	(0.027)	(0.027)	(0.039)
3:1 NNM	0.522*	0.319*	-0.177*	-0.188*
	(0.023)	(0.022)	(0.022)	(0.032)
CEM	0.440*	0.314‡	-0.222*	-0.153
	(0.167)	(0.190)	(0.062)	(0.120)
Panel B: With Covariates and Bootstrapping				
1:1 NNM	0.417*	0.221*	-0.157*	-0.207*
	(0.027)	(0.027)	(0.024)	(0.037)
3:1 NNM	0.448*	0.242*	-0.200*	-0.218*
	(0.0244)	(0.022)	(0.020)	(0.030)
CEM	0.398*	0.282*	-0.239*	-0.171*
	(0.020)	(0.020)	(0.0194)	(0.032)

Note: The table reports the average treatment effect of MaPP in the treated persons (ATT) estimated as in equation 2.3. The ATT estimations are based on different matching techniques, particularly 1:1 NNM, 3:1 NNM, and CEM. The MaPP coefficient from the logistic regressions is the log of the odds, i.e., logit. Panel A reports the ATT estimates when omitting variables from the outcome model. Cluster-robust standard errors are reported in parentheses with pair membership as the clustering variable. Panel B reports the ATT estimates with covariates in the outcome model. These covariates include “Female”, “Age”, “Education”, “Income” and “GDPpc”. Including covariates increases precision in the effect estimation, reduces bias from residual imbalance and the effect estimate is “doubly robust”. However, the ATT estimation is more complicated due to the noncollapsibility of the effect measures. To calculate standard errors, I first estimate the outcome model including the covariates, and then use the predictions from the outcome model to determine the contrast of the average potential outcomes under treatment and control. Standard errors are computed using block bootstrap with 100 replications. “*”, “†” and “‡” represent statistical significance at the 1%, 5% and 10% level, respectively.

Table 2.6: Effects of MaPP on Savings and Borrowing, IV Model

	Savings		**Borrowing**	
	Overall	**Formal**	**Overall**	**Formal**
Intercept	-1.580*	-3.392*	-0.086*	-2.916*
	(0.015)	(0.020)	(0.015)	(0.021)
MaPP	0.648*	1.672*	-0.476*	-0.276*
	(0.038)	(0.049)	(0.034)	(0.049)
Female	-0.169*	-0.157*	-0.198*	-0.175*
	(0.007)	(0.009)	(0.007)	(0.010)
Age	0.0003‡	0.010*	-0.011*	0.0001
	(0.000)	(0.000)	(0.000)	(0.000)
Education	0.519*	0.822*	0.173*	0.459*
	(0.005)	(0.007)	(0.005)	(0.007)
Income	0.193*	0.194*	0.007†	0.060*
	(0.003)	(0.003)	(0.003)	(0.004)
R	0.678*	-0.062*	-0.038‡	-0.062‡
	(0.021)	(0.025)	(0.022)	(0.034)

Note: The table reports the results of the IV model with CBI as an instrument. The coefficients are computed using a two-stage estimation in a logistic regression. Standard errors are shown in parentheses. The potential bias of the IV estimator is attenuated using the first-stage residual, R, as an additional regressor in the second-stage model. All other variables correspond to the ones in previous regressions. “*”, “†” and “‡” denote statistical significance at the 1%, 5% and 10% level, respectively.

Table 2.7: Effects of MaPP on Savings and Borrowing, Policy Tools

	LTV				**DSTI**			
	Savings		**Borrowing**		**Savings**		**Borrowing**	
	Overall	**Formal**	**Overall**	**Formal**	**Overall**	**Formal**	**Overall**	**Formal**
GLM	0.345*	-0.054‡	-0.016	-0.137*	-0.385*	-0.296*	-0.721*	-0.164†
	(0.026)	(0.029)	(0.025)	(0.037)	(0.065)	(0.066)	(0.063)	(0.083)
1:1 NNM	0.509*	0.381*	-0.137*	-0.176*	-0.799*	-0.861*	-0.714*	-0.376*
	(0.028)	(0.029)	(0.027)	(0.040)	(0.086)	(0.070)	(0.069)	(0.088)
3:1 NNM	0.518*	0.364*	-0.145*	-0.131*	-0.681*	-0.655*	-0.968*	-0.455*
	(0.024)	(0.023)	(0.227)	(0.034)	(0.065)	(0.055)	(0.060)	(0.075)
CEM	0.467*	0.328*	-0.170*	-0.116†	-0.245	-0.204	-1.050*	-0.378*
	(0.063)	(0.069)	(0.020)	(0.060)	(0.252)	(0.358)	(0.084)	(0.116)

Note: The table reports the LTV and DSTI coefficients on savings and borrowing. The coefficients are the log of the odds, i.e., logit. The results from PSM are based on a simple logistic regression without covariates. Cluster-robust standard errors are reported in parentheses. For the GLM, standard errors are clustered at the country-year level. For PSM, standard errors are clustered by pair membership. "*", "†" and "‡" represent statistical significance at the 1%, 5% and 10% level, respectively.

Table 2.8: Effects of MaPP on Savings and Borrowing, Interest Rate

	Savings		**Borrowing**	
	Overall	**Formal**	**Overall**	**Formal**
GLM	0.448*	0.182*	-0.157*	-0.132*
	(0.031)	(0.034)	(0.030)	(0.045)
1:1 NNM	0.240*	0.203*	-0.320*	-0.228*
	(0.033)	(0.033)	(0.032)	(0.047)
3:1 NNM	0.363*	0.300*	-0.327*	-0.151*
	(0.027)	(0.027)	(0.027)	(0.040)
CEM	0.479*	0.383†	-0.246*	-0.115‡
	(0.094)	(0.202)	(0.059)	(0.067)

Note: The table reports the coefficients for the variable "MaPP-MP", which is a dummy variable coded one when a country implements LTV or DSTI ratios in conjunction with an interest rate above the sample median. The coefficients are the log of the odds, i.e., logit. The results from PSM are based on a simple logistic regression without covariates. Cluster-robust standard errors are reported in parentheses. For the GLM, standard errors are clustered at the country-year level. For PSM, standard errors are clustered by pair membership. "*", "†" and "‡" represent statistical significance at the 1%, 5% and 10% level, respectively.

Table 2.9: Effects of MaPP on Savings and Borrowing, Country Income Level

	Advanced Economies				**Emerging Economies**			
	Savings		**Borrowing**		**Savings**		**Borrowing**	
	Overall	**Formal**	**Overall**	**Formal**	**Overall**	**Formal**	**Overalll**	**Formal**
GLM	-0.031	-0.115‡	-0.389*	-0.032	0.392*	-0.058	-0.140*	-0.187*
	(0.052)	(0.060)	(0.050)	(0.067)	(0.030)	(0.037)	(0.030)	(0.046)
1:1 NNM	0.156*	0.015	-0.313*	-0.194*	0.545*	0.442*	-0.093*	-0.255*
	(0.047)	(0.042)	(0.043)	(0.059)	(0.035)	(0.037)	(0.034)	(0.051)
3:1 NNM	0.123*	-0.037	-0.351*	-0.235*	0.496*	0.629*	-0.075*	-0.224*
	(0.039)	(0.035)	(0.036)	(0.049)	(0.030)	(0.029)	(0.028)	(0.043)
CEM	0.090	-0.081‡	-0.335*	-0.253*	0.575*	0.484‡	-0.111	-0.197
	(0.061)	(0.046)	(0.101)	(0.07)	(0.272)	(0.275)	(0.135	(0.220)

Note: The table reports the ATT of MaPP for advanced and emerging economies. There are 100,129 observations for advanced economies and 165,338 observations for emerging economies. The MaPP coefficient from the logistic regressions is the log of the odds, i.e., logit. The results from PSM are based on a simple logistic regression without covariates. Cluster-robust standard errors are reported in parentheses. For the GLM, standard errors are clustered at the country-year level. For PSM, standard errors are clustered by pair membership. "*", "†" and "‡" represent statistical significance at the 1%, 5% and 10% level, respectively.

Chapter 3

Does Macroprudential Policy Affect Wealth Inequality? Evidence from Synthetic Controls

3.1. Introduction

Following the Great Recession, central banks tightened macroprudential policy (MaPP). A decade later, these policies remain tight to ensure financial stability. But if MaPP restricts credit to a fortunate few, it may have searing consequences on the redistribution of wealth. A couple of questions logically emerge. Does MaPP affect wealth inequality? If so, what are the transmission channels through which MaPP affects wealth?

This paper attempts to answer these questions using a synthetic control approach. Specifically, I use a combination of countries that never implement MaPP to mimic the most relevant characteristics of the countries with MaPP. I then compare the trajectory of wealth inequality in these counterfactuals to the actual evolution of wealth inequality in the treated countries. This allows me to obtain an estimate of the causal effects of MaPP on wealth inequality based on a large sample of 171 countries between 1995-2020.

To be sure, these are important empirical questions that cannot be answered using conventional time-series analyses. I say this for two reasons. First, MaPP is often implemented in response to contemporaneous events. Consequently, conventional analyses struggle to isolate the effects of MaPP from all other possible factors driving wealth inequality. In contrast, synthetic controls minimize the problem of reverse causality and cast light on the causal relationship between MaPP and wealth inequality. Second, a country with MaPP will always be different from any other country without MaPP. As such, a simple comparison between countries with and without MaPP should always be ruled out – for the simple reason that the estimated effects of MaPP may reflect pre-existing differences across countries. Remarkably, synthetic controls ensure that the time-varying response of wealth inequality to unobserved factors will be fairly similar between treated countries and their synthetic versions.

In principle, MaPP may affect wealth inequality through different channels in any one direction. The first channel is aggregate production. There is ample evidence that credit is an important determinant of firm production, which in turn influences the demand for low- and high-skilled workers (e.g., Townsend and Ueda, 2006). There are two possible cases to be considered. If credit is too tight, firms may cut production and reduce the demand for labor. In this first case, wages should go down and wealth inequality may increase. However, tighter credit makes capital more expensive. This may increase labor demand and push wages up. In this second case, wealth inequality may decline as firms substitute capital for labor. It is therefore not clear whether, or to what extent, MaPP affects wealth inequality through aggregate production.

The second possible channel is entrepreneurship. If MaPP makes access to credit more difficult, a talented but poor entrepreneur may be unable to invest in a small business (e.g., Evans and Jovanovic, 1989; Evans and Leighton, 1989; Holtz-Eakin et al., 1994). Evidently, this may aggravate wealth inequality. The difficulty with this argument is that the proportion of entrepreneurs in the economy is relatively small. So, changes in MaPP should have a negligible effect on the redistribution of wealth. On top of that, small, informal businesses have limited access to formal credit and should not be much affected by MaPP. It follows, I believe, as a matter of basic logic that MaPP will most likely hit those entrepreneurs with intermediate levels of wealth. If that's the case, then MaPP may either increase or decrease wealth inequality. It may increase wealth inequality because people with intermediate wealth move away from the top of the distribution, or it may decrease wealth inequality because the people in the middle get closer to the relatively poor. The net effect of these contrasting forces on wealth inequality is ambiguous.

The last channel is human capital accumulation. There is a great deal of evidence that credit can be used to break the historical link between parental wealth and human capital accumulation (e.g., Becker and Tomes, 1979, 1986; Galor and Zeira, 1993). It is perfectly possible that credit helps the poor to acquire education and move up the wealth ladder. If so, then tighter credit due to MaPP may increase wealth inequality. However, I have argued before that MaPP will most likely hit those with intermediate levels of wealth. By the same reasoning, wealth inequality may increase if those with intermediate wealth cannot access a student loan and are pushed away from the top of the distribution; or wealth inequality may decrease because the gap between those in the middle and the poor becomes smaller as more and more people get excluded from student loans. Once again, the net effect of these opposing forces on wealth inequality is far from clear.

Until now, surprisingly little attention was given to the redistributive effects of bank regulation (e.g., Demirgüç-Kunt and Levine, 2009). Only a handful of papers find that bank regulation widens the income distribution (e.g., Galor and Moav, 2004; Beck et al., 2010). But much less is known about the effects of MaPP. Some papers using individual-level data show that MaPP leads to a reallocation of credit from low- to high-income individuals (e.g., Acharya et al., 2020; Behncke, 2020; DeFusco et al., 2020). While these results are interesting and important, they do not tell us much about the way MaPP affects the distribution of wealth. Recently, some papers find cross-country evidence of a positive relationship between MaPP and income inequality (Delis et al., 2014; Frost, 2018; Hasan et al., 2020). But it is also quite possible that MaPP leads to a more stable financial system in the future. This, in turn, could create more economic opportunities for the poor and reduce wealth inequality over time. The point I wish to make here is that all these arguments are fairly speculative. One cannot say as a matter of principle whether MaPP will increase or decrease wealth inequality. It is an empirical question. And the empirical evidence of the effects of MaPP on wealth inequality remains fuzzy at best.

In this paper, I show that, after the adoption of MaPP, wealth concentration in the treated countries increases by 3.4 percentage points in a decade. During the same period, the wealth share going to the top 1% rises steadily at the expense of a lower wealth share held by the bottom 50%. As we shall see, this rise in wealth inequality is more pronounced in countries that implement debt-service-to-income (DSTI) ratios rather than loan-to-value (LTV) ratios. The effects of MaPP are also generally stronger on advanced economies. Taken together, these results support the view that MaPP leads to greater wealth inequality and should be adjusted in much the same way as the interest rate.

The contribution of this paper is threefold. First, it provides evidence on the effects of MaPP on wealth inequality. A limitation of prior work is that it focuses almost exclusively on income inequality. However, MaPP is more likely to affect capital income than wages or earned income. Why? Because MaPP is tightened to prevent large fluctuations in asset prices, particularly in housing (e.g., Vandenbussche et al., 2015; Kelly et al., 2018). If MaPP makes it more difficult for people to get a home loan or profit from increases in asset prices, any measure of income inequality will underestimate the true effects of MaPP. The analysis presented in this paper shows that MaPP has deleterious effects on wealth inequality and that the size of these effects depends crucially on the design of the prudential rule and the country's income level.

Second, an important contribution of this paper is the use of synthetic controls to establish causality from MaPP to wealth inequality. By using synthetic controls, I minimize concerns about reverse causality and omitted variable bias. So far, synthetic controls have only been used to study the impact of a one-time policy in a single region or country (e.g., Card, 1990; Abadie and Gardeazabal, 2003; Abadie et al., 2010). Instead, I use synthetic controls to examine the effects of MaPP in a setting with multiple treated countries and variation in treatment timing.

And this brings me to my last point. Relative to conventional time-series analysis, synthetic controls are a more objective and transparent way to estimate the effects of MaPP. The reasons for this are the following. The first is that synthetic controls provide visual evidence that the control units are capable of reproducing the wealth trajectories in the countries with MaPP had they not implemented these policies. This visual evidence is more compelling than numbers and it is supported by simulation-based uncertainty estimates. The second reason is that synthetic controls reduce discretion in the choice of control units. I simply let a data-driven procedure choose the combination of control units that best matches the characteristics of the countries with MaPP. This choice is independent of the post-treatment evolution of wealth inequality. Since synthetic controls are essentially a pool of weighted control units, I can check and report the relative contribution of each control unit to the counterfactual of wealth inequality in the treated countries.

The remainder of the paper proceeds as follows. Section 3.2 explains the synthetic control approach with staggered adoption. Section 3.3 describes the data. Section 3.4 examines the effects of MaPP on wealth inequality and provides robustness checks. Section 3.5 extends the analysis to different policy tools and country income levels. Section 3.6 concludes.

3.2. Methodology

3.2.1. Identification

This section provides an abridged description of the generalized synthetic control (GSC) method[1]. Suppose Y_{it} is the outcome of interest of country i at time t. Let τ and C denote the set of countries in the treatment and control groups, respectively. The total number of

[1]The interested reader is referred to the original paper by Xu (2017) for additional details.

countries in the sample is $N = N_{tr} + N_{co}$, where N_{tr} and N_{co} are the number of countries in the treated and control groups, respectively. Each country is observed for T periods. Assuming that $T_{0,i}$ is the number of pre-treatment periods for country i, the exposure to treatment is observed for $T - T_{0,i}$ periods. The countries in the control group are never exposed to treatment in the observed time span.

The outcome of interest, Y_{it}, is given by a linear factor model:

$$Y_{it} = \delta_{it}D_{it} + x'_{it}\beta + \lambda'_{i}f_t + \varepsilon_{it} \tag{3.1}$$

where D_{it} is a treatment variable that equals 1 when a country i is treated prior to time t and 0 otherwise; δ_{it} is the heterogeneous treatment effect on country i at time t; x'_{it} is a $(k \times 1)$ vector of observed covariates; $\beta = [\beta_1, \ldots, B_k]'$ is a $(k \times 1)$ vector of unknown parameters; $f_t = [f_{1t}, \ldots, f_{rt}]'$ is a $(r \times 1)$ vector of unobserved common factors; $\lambda_i = [\lambda_i, \ldots, \lambda_{ir}]$ is a $(r \times 1)$ vector of unknown factor loadings and ε_{it} captures the unobserved idiosyncratic shocks of country i at time t. The factor component of the model takes a common linear additive form[2]: $\lambda_i' f_t = \lambda_{i1}' f_{1it} + \lambda_{i2}' f_{2t} + \ldots + \lambda_{ir}' f_{tr}$.

Let $Y_{it}(1)$ and $Y_{it}(0)$ be the potential outcomes of interest for country i at time t when $D_{it} = 1$ and $D_{it} = 0$, respectively. Then, the data-generating process (DGP) for each country can be written as follows:

$$Y_i = D_i \circ \delta_i + X_i\beta + \lambda_i F + \varepsilon_i, i \in 1, 2, \ldots, N_{co}, N_{co}+1, \ldots, N. \tag{3.2}$$

where $Y_i = [Y_{i1}, Y_{i2}, \ldots, Y_{iT}']$; $D_i = [D_{i1}, D_{i2}, \ldots, D_{iT}]'$ and $\delta_i = [\delta_{i1}, \delta_{i2}, \ldots, \delta_{iT}]'$. "$\circ$" means point-wise product; $\varepsilon_i = [\varepsilon_{i1}, \varepsilon_{i2}, \ldots, \varepsilon_{iT}]'$ are $(T \times 1)$ vectors; $X_i = [x_{i1}, x_{i2}, \ldots, x_{iT}]'$ is a $(T \times k)$ matrix; and $F = [f_1, f_2, \ldots, f_T]'$ is a $(T \times r)$ matrix. The control and treated countries are subscripted from 1 to N_{co} and $N_{co} + 1$ to N, respectively.

The outcome of interest for all the countries in the control group is:

$$Y_{co} = X_{co}\beta + F\Lambda_{co}' + \varepsilon_{co} \tag{3.3}$$

in which $Y_{co} = [Y_{i1}, Y_{i2}, \ldots, Y_{N_{co}}]'$ and $\varepsilon_{co} = [\varepsilon_1, \varepsilon_2, \ldots, \varepsilon_{co}]'$ are $(T \times N_{co})$ matrices; X_{co} is a three-dimensional $(T \times N_{co} \times p)$ matrix; and $\Lambda_{co}' = [\lambda_1, \lambda_2, \ldots, \lambda_{N_{co}}]$ is a $(N_{co} \times r)$ matrix. The optimal number of factors, r, is selected based on a cross-validation procedure that minimizes the mean squared prediction error (MSPE).

[2]The term $\lambda_i' f_t$ captures the effects of unobserved factors correlated across countries. This reduces concerns about selection bias in the choice of conditioning factors and allows for MaPP to be endogenous to unobserved time-varying and unit-specific factors.

The causal parameter of interest is the average treatment effect on the treated countries (ATT) at time t, when $t > T_0$:

$$ATT_{t,t>T_0} = \frac{1}{N_{tr}} \sum_{i \in \tau} [Y_{it}(1) - Y_{it}(0)] = \frac{1}{N_{tr}} \sum_{i \in \tau} \delta_{it} \tag{3.4}$$

Note that $Y_{it}(1)$ is observed for the treated countries. The goal here is to estimate the unobserved $\hat{Y}_{it}(0)$ for each treated country in the post-treatment periods. This estimation can be done under fairly standard conditions[3]. It is to this task that I turn next.

3.2.2. Estimation

The GSC estimator of the ATT for country i at time t is given by the difference between the actual outcome and the estimated counterfactual, as below:

$$\delta_{it} = Y_{it}(1) - \hat{Y}_{it}(0) \tag{3.5}$$

where $\hat{Y}_{it}(0)$ is imputed after three steps. In the first step, I obtain $\hat{\beta}$, $\hat{F}$, and $\hat{\Lambda}_{co}$ from an interactive fixed effects (IFE) model using only data from the control group:

$$\begin{gathered}\hat{\beta}, \hat{F}, \hat{\Lambda}_{co} = \underset{\widetilde{\beta}, \widetilde{F}, \widetilde{\Lambda}_{co}}{\arg\min} \sum_{i \in C} (Y_i - X_i \widetilde{\beta} - \widetilde{F}\widetilde{\lambda}_i)'(Y_i - X_i \widetilde{\beta} - \widetilde{F}\widetilde{\lambda}_i) \\ \text{s.t. } \frac{\widetilde{F}'\widetilde{F}}{T} = I_r \text{ and } \hat{\Lambda}'_{co}\hat{\Lambda}_{co} = \text{diagonal}\end{gathered} \tag{3.6}$$

In the second step, I estimate the factor loadings for each treated country. These are the contributions (or "weights") of each control unit in the synthetic control. The loadings minimize the MSPE of the treated outcome in the pre-treatment periods:

$$\begin{aligned}\hat{\lambda} &= \underset{\widetilde{\lambda}_i}{\arg\min} \sum_{i \in C} (Y_i^0 - X_i^0 \hat{\beta} - \hat{F}^0 \widetilde{\lambda}_i)'(Y_i^0 - X_i^0 \hat{\beta} - \hat{F}^0 \widetilde{\lambda}_i) \\ &= (\hat{F}^{0\prime}\hat{F}^0)^{-1}\hat{F}^{0\prime}(Y_i^0 - X_i^0 \hat{\beta}), i \in \tau\end{aligned} \tag{3.7}$$

[3]The estimation requires four additional assumptions. First, the adoption of MaPP must be independent of the error term. Second, there must be weak serial dependence of the error terms, which is confirmed after ruling out the presence of unit roots in the data. Third, standard moment conditions ensure the convergence of the estimator. Lastly, the error terms are assumed to be cross-sectionally independent and homoscedastic since all wealth variables are bounded between 0 and 1 to reduce the variability of the error terms.

where the superscripts “0” denote the pre-treatment periods and $\hat{\beta}$ and $\hat{F}^0$ are taken from the first step. The loadings can be negative or positive. The GSC model uses the factors and outcomes in the pre-treatment period to choose the loadings for the control units and then uses cross-sectional correlations between treated and control units to predict the treated counterfactuals.

In the last step, I obtain the treated counterfactuals based on $\hat{\beta}$, $\hat{F}$, and λ_i:

$$\hat{Y}_{it}(0) = x'_{it}\hat{\beta} + \hat{\lambda}_i'\hat{f}_t, i \in \tau, t > T_0 \tag{3.8}$$

Finally, the $\widehat{ATT}_t$ is estimated as following:

$$\widehat{ATT}_t = \frac{1}{N_{tr}} \sum_{i \in \tau} [Y_{it}(1) - \hat{Y}_{it}(0)] \text{ for } t > T_0 \tag{3.9}$$

Statistical significance is assessed using a parametric bootstrap procedure that estimates the standard errors for each ATT. The prediction error of the IFE model for the treated counterfactuals is computed based on simulated data from the control group. I take one control unit out as a fake treated unit and use the rest of the control units to predict the outcome of the excluded unit. The difference between the predicted and observed outcome is the prediction error of the IFE model. Lastly, the prediction error for the treated counterfactuals is drawn from the empirical distributions of the prediction errors.

3.3. Data

I collect data from 171 countries between 1995-2020. The wealth outcomes are from the World Inequality Database (WID). The outcome variable of interest is the Gini index of wealth concentration. The Gini is a simple way to compare wealth inequality across countries but it masks important changes in certain groups of the wealth distribution. This is why I also look at the wealth share of the top 1%, top 10% and bottom 50% of the distribution. All wealth series are based on the concept of “net personal wealth”, which is defined as the sum of financial and non-financial assets net of financial liabilities held by the household sector. I take the data exactly as published in the WID.

The annual variation in wealth inequality for an average country is close to zero. Given that wealth moves rather slowly over time, the outcome values refer to at least five years $(T_0 + 5)$ after the adoption of MaPP (T_0). This ensures just enough variability on wealth to produce meaningful results[4]. The fact that the distribution of wealth changes slowly over time is particularly suitable for synthetic controls since small interventions like an LTV or DSTI ratio may be indistinguishable from all other shocks when the outcome of interest is very volatile (Abadie, 2021).

I assign to the treatment group every country that implements MaPP during the sample period. I obtain these data from Alam et al. (2019). The treated countries remain in the treatment group as long as they maintain a tight macroprudential stance, i.e., the cumulative number of MaPP tools remains positive in the sample period. All the countries that do not implement MaPP are assigned to the control group. In line with previous studies, I focus on borrower-based policies that have strong redistributive effects. This includes the DSTI and the LTV ratios (e.g., Lim et al., 2011; Frost, 2018; Teixeira and Venter, 2023). Both ratios place explicit limits on debt and restrict the ability of individuals with little collateral to purchase a house or invest in a small business. These individuals have fewer chances to increase their wealth, benefit from a rise in asset prices or have a cushion for bad times.

The set of conditioning factors includes important characteristics of countries that are closely related to wealth inequality. Following the literature, I look at average education, financial development, inflation, population, and real GDP per capita (e.g., Hasan et al., 2020)[5]. This choice of conditioning factors ensures that the synthetic controls can reproduce almost exactly the characteristics of the countries with MaPP. The visual evidence provided in this paper confirms that the synthetic controls are very similar to the treated countries during the pre-treatment period (Abadie, 2021)[6]. The data for the conditioning factors are taken from the World Bank Open Data Catalogue. The tables' footnotes provide a more detailed description of the factors.

Each model uses a cross-validation procedure to choose the optimal number of factors that minimizes the MSPE of the wealth outcome. To improve pre-treatment matching, I restrict the analysis to countries with at least 7 pre-treatment periods (Xu, 2017). All countries with data missing in the post-treatment period are also removed. This reduces the final sample to

[4]To ensure enough variability in the outcome variable, it is common to look at the variable a few years after treatment. For example, Billmeier and Nannicini (2013) examine the effects of economic liberalization on GDP growth five years after the liberalization episode occurred.

[5]I exclude the policy rate because data is missing for a large number of countries. Instead, I use inflation as a close substitute. Moreover, I exclude the tax rate since taxes vary very little per country. Excluding these factors improves the precision of the ATT estimates. The reader should be assured, however, that the results are substantively identical when these factors are included in the models.

[6]One may argue that I should also account for factors that directly affect the distribution of wealth. To provide robustness checks, I repeat the analysis using conditioning factors that are more likely to be related to the distribution of wealth rather than the level of wealth. These results are discussed in section 3.4.2.

114 countries but ensures that the response of wealth inequality to unobserved factors will be fairly similar between treated countries and synthetic controls (Abadie et al., 2010).

Table 3.1 reports summary statistics for the full sample. There are 34 treated countries. The donor pool includes the remaining 80 countries[7]. The distribution of wealth differs drastically by country's income level. Later on, I explore how the results change when I divide the sample into advanced and emerging economies.

3.4. Results

3.4.1. Baseline Results

Figure 3.1 plots the evolution of wealth inequality in countries with MaPP and their synthetic controls. As the figure makes apparent, the synthetic controls closely reproduce the trajectories of wealth in the treated countries prior to the adoption of MaPP. This close fit demonstrates that the synthetic controls are a suitable comparison group to examine the effects of MaPP on wealth inequality in the treated countries.

The estimate of the effects of MaPP is given by the difference between the observed wealth outcome in the treated countries and their synthetic controls. In the initial period, immediately after countries implement MaPP, the Gini index of wealth concentration remains relatively stable[8]. From $T_0 + 7$ onwards, the Gini of the treated countries and their synthetic controls begin to diverge noticeably. While the Gini in the synthetic controls displays a moderate downward trend, the Gini of the treated countries rises steadily. A decade after MaPP adoption, the Gini of the treated countries is estimated to be 3.4 percentage points above that of the synthetic controls. This discrepancy in the Gini trajectories suggests that MaPP has strong effects on wealth concentration.

This finding raises a natural question. Does MaPP affect everyone in the same way? The answer is, obviously, no. The baseline estimates indicate that MaPP expands the wealth share of the top 1% by 5.1 percentage points relative to a synthetic control country during the post-treatment period. The estimates for the wealth share of the top 10% are roughly in the same ballpark. Conversely, the wealth share of the bottom 50% declines by 1 percentage point in the same period. Although this value appears to be small, it implies that the average wealth of the bottom 50% decreases by about one-third in a decade. These results are

[7]For more detail on the sample, see Appendix B.

[8]This is expected given the stability of wealth over time. There is also plenty of evidence that MaPP only affects households a few years after they have had enough time to adjust their behavior (e.g., Borio and Shim, 2007; Richter et al., 2019; Teixeira and Venter, 2023).

consistent with the view that MaPP is more likely to affect people with intermediate levels of wealth. This is a painfully obvious point that has somehow been overlooked by the literature.

To evaluate the robustness of these estimates, I compute standard errors for each ATT using the parametric bootstrap procedure explained earlier. Panel A of Table 3.2 reports cumulative ATTs of MaPP for the full sample and their respective standard errors. It can be clearly seen that almost every estimated ATT is significant at the 1% level. This suggests that the effects of MaPP are unusually large in the treated countries relative to the distribution of the estimates for the synthetic controls.

A potential concern with the analysis above is that some unobserved factors may be driving the wealth outcomes after the adoption of MaPP. Notwithstanding the importance of this criticism, it is unlikely that an unobserved factor would fully explain the differences in wealth inequality between treated countries and synthetic controls. The reason is that I restrict the analysis to treated countries with at least 7 pre-treatment periods. By doing so, I ensure that the response of wealth inequality to unobserved factors will not differ drastically between treated countries and synthetic controls in the post-treatment period (Abadie et al., 2010). It is also worth noting that the only way that synthetic controls are able to reproduce the trajectories of wealth over extended periods of time is if the control units are comparable to the countries with MaPP both in terms of observed and unobserved factors as well as in the effects of those factors on wealth (Abadie et al., 2015). Therefore, the impact of an unobserved factor would have to be quite large to change the results substantively. This possibility is explored in the next section.

Having said this, I wish to emphasize that my estimates are in line with those obtained by Delis et al. (2014) and Frost (2018) for income inequality using regression methods. Delis et al. (2014) suggest that bank regulation increases income concentration by approximately 5 percentage points. Like me, they also find that bank regulation decreases the income share of the bottom 10% with little impact on the income share of the top 10%. Recently, Frost (2018) reports that MaPP adoption is associated with an increase of 3 percentage points in income concentration. The results from my analysis point to a similarly large impact on wealth concentration, which suggests that the effects of MaPP on inequality are more persistent and severe than previously thought.

Overall, these findings support the view that MaPP exacerbates wealth inequality. After the adoption of MaPP, the rich and, for that matter, the upper middle class, become a great deal richer, while the poor become significantly poorer. In the next section, I explore the robustness of these results using alternative model specifications.

3.4.2. Robustness Checks

I first check if the results are robust to changes in the set of conditioning factors. As mentioned in an earlier footnote, some conditioning factors may be related to the level of wealth but not necessarily to the distribution of wealth. This could affect the estimation of the factor loadings and the construction of synthetic controls. It may be argued, for example, that average education is intrinsically related to the level of wealth but not to the distribution of wealth. For instance, many advanced economies have both high levels of education and wealth inequality. A similar case can be made for population, which may or may not be related to the distribution of wealth in a country.

To account for these possibilities, I repeat the analysis using alternative measures of education and population. More specifically, I examine whether the results change when the estimation is performed using government expenditure on education and population growth. Admittedly, these factors are more likely to be related to changes in the distribution of wealth rather than in the level of wealth. The results from these robustness tests are reported in Appendix B.2.

Figure B.3.1 shows that MaPP still leads to a substantial rise in wealth inequality when I use alternative conditioning factors. The pattern of wealth inequality is very similar to the baseline case: there is an increase in the wealth share of the top 1% combined with a decrease in the wealth share of the bottom 50%. The results do not change much because the estimated effects of MaPP on wealth inequality do not depend, at least directly, on the choice of conditioning factors. Perhaps a more interesting question is how the magnitude of the impact of MaPP is influenced by the choice of conditioning factors. The results reveal that MaPP increases wealth inequality by 5.4 percentage points in a decade. These estimates are slightly higher than those found in the previous section and suggest that the baseline results should be taken as a conservative estimate of the true effects of MaPP.

I also test the robustness of the results to other possible factors driving wealth inequality after the adoption of MaPP. These factors include relevant changes in fiscal policy or unconventional monetary policy[9]. As shown in Figures B.3.2 and B.3.3, the results hardly change. After I explicitly control for fiscal policy, wealth concentration as measured by the Gini index increases 3.2 percentage points in a decade. This compares to 3.4 percentage points in the baseline case. This small difference suggests that fiscal policy does not strongly influence

[9]I use government subsidies as a proxy for fiscal policy and forward guidance as a proxy for unconventional monetary policy. The data on government subsidies is readily available on the World Bank Open Data Catalogue, while the data on forward guidance comes from Sutherland (2022) who analyses thirty years of monetary policy statements in eight major central banks.

the impact of MaPP on wealth inequality. Likewise, when I control for unconventional monetary policy, wealth inequality rises by 3.8 percentage points. This is only 0.4 percentage points above the baseline case. A possible explanation for this result is that forward guidance enhances the effects of MaPP when agents anticipate that credit will remain tight in the future.

In summary, the results from these different robustness tests are quite close to the baseline case. The adoption of MaPP always leads to an increase in wealth inequality. This increase ranges from 3.2 to 5.4 percentage points in a decade. Importantly, these results suggest that most of the variation in the effects of MaPP is due to differences in the distribution of wealth across countries and not so much due to omitted variable bias. As we shall see next, the effects of MaPP also vary with the design of the policy tool and the country's income level.

3.5. Extensions

In this section, I investigate whether the effects of MaPP vary with the policy tool and the country's income level. The DSTI ratio restricts credit based on income, while the LTV ratio limits credit based on collateral. These tools may have disparate effects on wealth inequality. Similarly, the response of wealth may vary appreciably across countries with different income levels. I investigate these issues separately below.

3.5.1. Policy Tools

This section separates the effects of the DSTI ratio from the effects of the LTV ratio. I face two challenges here. The first is that some countries implement LTV and DSTI ratios, simultaneously. A way to address this is to divide the sample into countries that implement either one of these tools. The drawback is that I end up dropping all countries that implement both tools at the same time. The second challenge is that most countries only implement a DSTI ratio after the Great Recession. As a result, there are only a few observations available for the DSTI ratio in the post-treatment period. To get around this, I compare the ATTs of both tools from $T_0 + 5$ to $T_0 + 8$. This time span ensures that the synthetic controls have a similar amount of data available for both tools.

Table 3.3 summarizes the estimation results for both policy tools. Figure 3.2 displays the evolution of wealth inequality in countries with DSTI ratios and their synthetic versions. As the figure suggests, the synthetic controls can reproduce the evolution of wealth inequality in the treated countries during the pre-treatment period. The estimates indicate that the adoption of a DSTI ratio increases the Gini index of wealth concentration by 5.7 percentage points in a decade. The cumulative ATTs for the top 1%, top 10%, and bottom 50% are 4.7, 6.4, and -1.9 percentage points, respectively. What is most interesting is that the rise in the wealth share of the top 1% pales in comparison to the one of the top 10%. This suggests that the DSTI ratio benefits mostly the upper middle class at the expense of the lower middle class and the bottom 50%. These results confirm that prudential rules based on income affect mostly individuals with intermediate levels of wealth.

The estimates for the LTV ratio, shown in Figure 3.3, are much weaker. Once again, the synthetic controls are able to replicate the trajectories of wealth in the pre-MaPP years. Surprisingly, the effects of the LTV ratio on wealth inequality are relatively mild. The results indicate that the Gini index of wealth concentration increases a modest 1.1 percentage points in a decade. Just to be sure, these effects are six times smaller than the ones found for the DSTI ratio. Still, the LTV ratio, much like the DSTI ratio, increases the wealth share of the upper middle class and the rich by approximately 3 percentage points. This is in contrast with the wealth share of the bottom 50%, which decreases by 0.1 percentage points. The combined evidence of these figures suggests that the lower middle class is the most affected by LTV ratios. These results could vary with the intensity of the LTV and DSTI ratio. Although I do not explicitly control for this, the effects of the LTV ratio are clearly small.

On the whole, these results indicate that prudential rules based on income have pernicious effects on the distribution of wealth. If central banks are concerned about the redistributive effects of MaPP, as they should be, they should opt for policies that establish higher collateral requirements rather than restricting credit based on income.

3.5.2. Country Income Level

As a final exercise, I examine whether the effects of MaPP vary with the country's income level. There is much evidence that inequality is inversely related to growth in emerging economies and directly related to growth in advanced economies (Barro, 2000). It is only reasonable to expect MaPP to have different effects on advanced and emerging economies. To investigate this, I divide the sample into advanced and emerging economies based on the World Bank's country classification by income level[10].

Figure 3.4 plots the trends in wealth inequality for treated advanced economies as well as their synthetic counterparts. Despite the smaller donor pool, the synthetic controls are

[10]The advanced economies comprise upper-middle and high-income countries, while the emerging economies include lower-middle and low-income countries. This is a simple and tractable way to examine how the effects of MaPP change with the country's income level.

still able to replicate the evolution of wealth inequality in the treated countries during the pre-treatment period. Broadly speaking, I find that MaPP aggravates wealth inequality in advanced economies. After a decade of MaPP, the Gini index of wealth concentration in the treated countries is estimated to be 2.9 percentage points above that of the synthetic controls. During the same period, the wealth share of the top 1% expands by 4.7 percentage points, while that of the bottom 50% shrinks by 0.9 percentage points. These effects are fairly large and persistent.

Turning next to emerging economies, Figure 3.5 shows that the effects of MaPP on wealth inequality are much smaller. To facilitate the comparison, Panels B and C of Table 3.2 disaggregate the estimated effects of MaPP per country income level. The Gini index of wealth concentration increases by 2.1 percentage points above that of the synthetic controls in the post-treatment period. This is about one-third less than the estimate for advanced economies. The wealth share of the top 1% and top 10% increased 2.1 and 2.7 percentage points in a decade, while the wealth share of the bottom 50% decreases a meager 0.2 percentage points. Once again, people with intermediate levels of wealth seem to be most affected by MaPP. These effects are nonetheless small and temporary.

One might ask why the effects of MaPP are more pronounced in advanced economies. Here I will necessarily leave solid ground. A plausible explanation, consistent with the evidence in this paper, is that advanced economies have more people with intermediate levels of wealth. Another possible explanation is that people in emerging economies are more likely to use informal credit and are less affected by MaPP (Teixeira, 2022). It is not too surprising, then, that MaPP has more serious effects on the wealth distribution of advanced economies.

The bottom line is that MaPP increases the wealth share of the relatively rich and makes it more difficult for the poor to climb the wealth ladder. This is true for all countries regardless of their stage of development. Nevertheless, these effects are much stronger on advanced economies. I conclude with a few policy implications.

3.6. Conclusions

There is a widespread belief that MaPP is necessary to prevent financial crises. Surely, MaPP helps maintain financial stability. But at what cost and to whom?

This paper provides evidence of a negative impact of MaPP on wealth inequality. I find that MaPP increases wealth concentration by approximately 3.4 percentage points in a decade. This increase in inequality is explained by an ever-growing share of wealth going to the top of the distribution. As wealth becomes more and more concentrated, the middle class and the relatively poor get trapped in a cycle of less credit and rising prices. This makes it harder for them to accumulate wealth over time. These effects are generally stronger for DSTI ratios, which suggests that prudential rules based on income have stronger redistributive effects. The effects of MaPP are also more pronounced on advanced economies possibly because MaPP restricts credit to a larger number of people with intermediate levels of wealth.

This analysis can be extended in several directions. First, the GSC model does not accommodate treatment reversal. As a consequence, I cannot fully account for the effects of loosening, tightening, or removing MaPP. An important question that remains unanswered is how the strength of the macroprudential stance affects inequality. Second, it would be interesting to separate the effects of MaPP from all other possible factors driving wealth inequality after the adoption of MaPP. The results presented here are robust to changes in fiscal policy and unconventional monetary policy but more work is needed on the way these policies interact with MaPP. Lastly, I assume that all treated countries implement similar prudential rules but this is not necessarily the case. Future work could explore how the limits of the DSTI and LTV ratios affect wealth inequality.

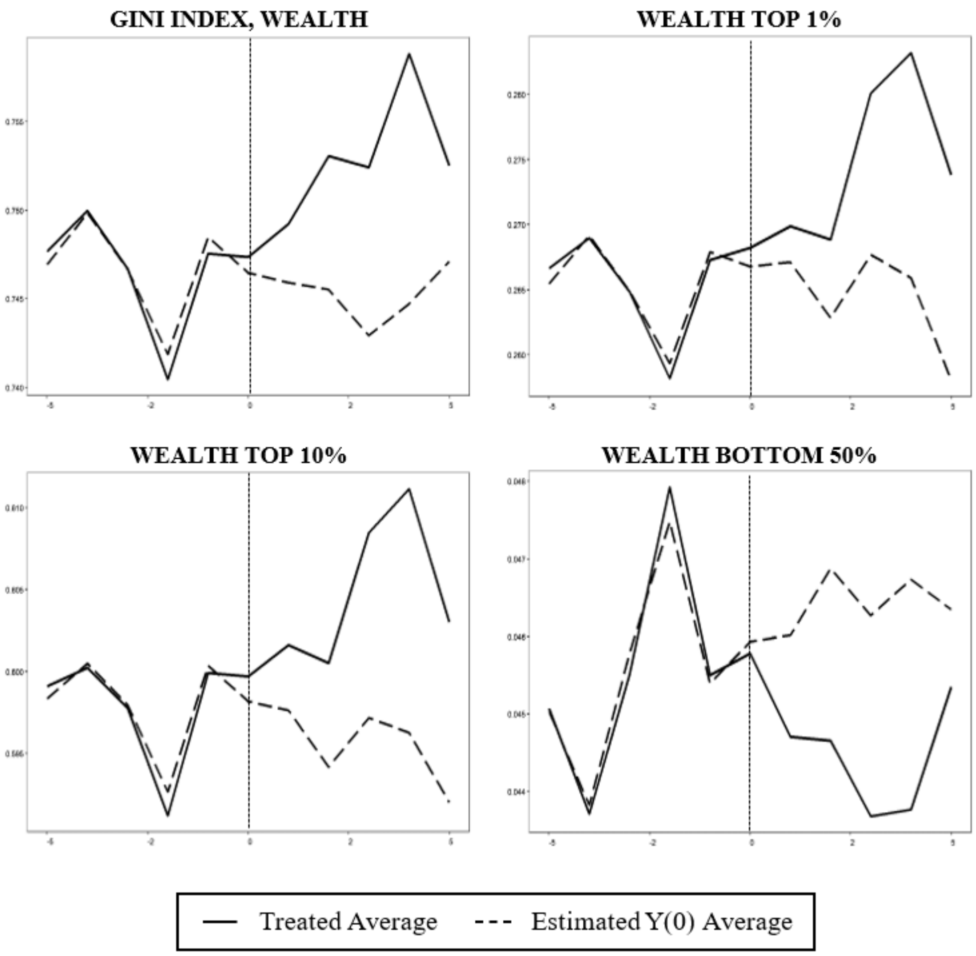
Despite these shortcomings, the core message that follows from this paper is that MaPP improves financial stability at the cost of greater wealth inequality. Does this mean, then, that we should never adopt MaPP? No, of course not. It just means that we should carefully consider the interaction between MaPP and the interest rate. Failing to do so may exacerbate wealth inequality.

Table 3.1: Summary Statistics

Variable	**Obs.**	**Mean**	**SD**	**Min**	**25th Pctl.**	**Median**	**75th Pctl.**	**Max**
Treatment	4,446	0.171	0.376	0	0	0	0	1
Average Education	3,004	0.755	0.328	0.053	0.472	0.843	0.995	1.640
Financial Development	4,290	0.269	0.234	0	0.096	0.185	0.401	1
Forward Guidance	4,446	0.059	0.236	0	0	0	0	1
Gini Index, Wealth	3,863	0.766	0.068	0.472	0.722	0.75	0.807	1
Gov. Expenditure on Education	3,332	4.287	1.827	0	2.92	4.096	5.324	14.059
Gov. Subsidies	2,874	37.761	19.603	0	22.605	35.392	52.065	90.028
Inflation	4,076	0.103	0.828	-0.181	0.018	0.039	0.079	4.145
Population Growth	4,238	0.016	0.020	-0.044	0.005	0.014	0.025	0.191
Real GDP per capita	3,962	10,686	1,402	456	1,313	4,133	12,864	46,273
Total Population	4,368	39,059	14,1	0,075	3,083	8,917	26,233	1411
Wealth Bottom 50%	4,008	0.042	0.025	-0.053	0.028	0.048	0.058	0.166
Wealth Top 1%	4,008	0.299	0.082	0.121	0.241	0.278	0.348	0.571
Wealth Top 10%	4,008	0.629	0.079	0.392	0.574	0.609	0.677	0.891

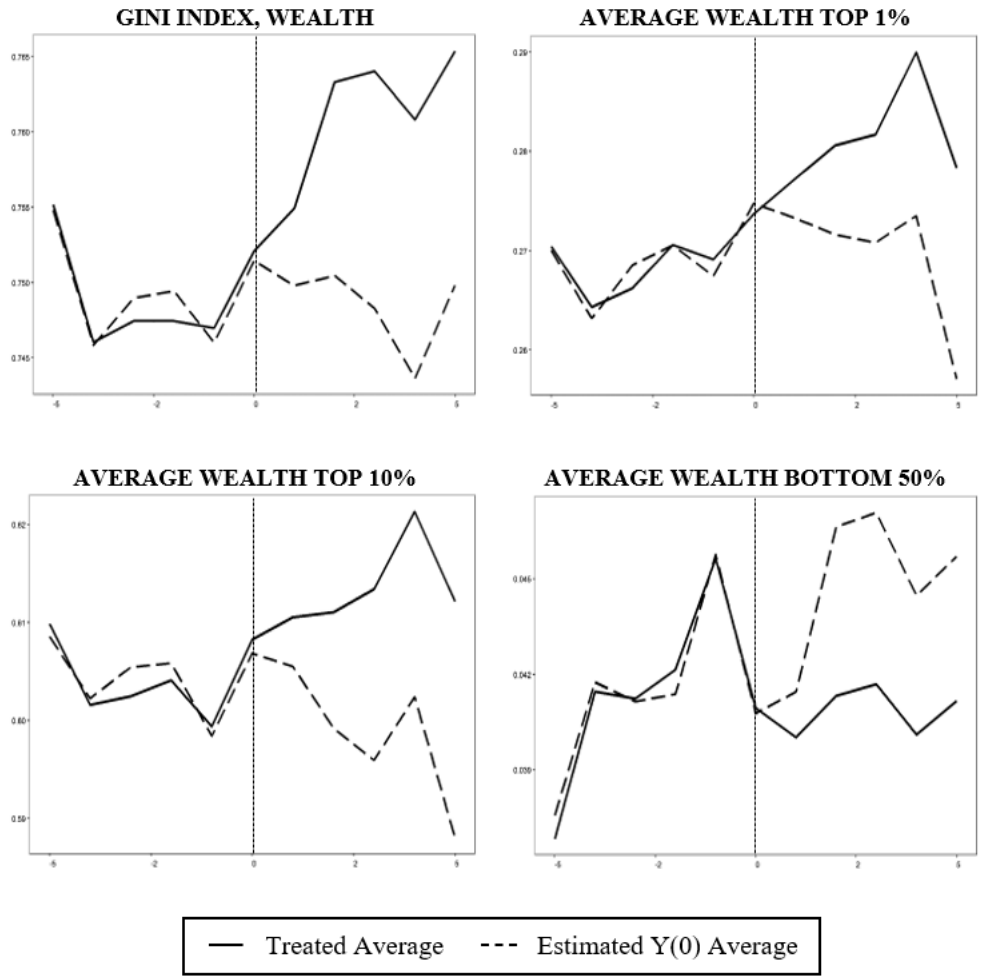
Note: The table presents descriptive statistics for the full sample. "Treatment" is a dummy variable set to one when a country implements an LTV or DSTI ratio. "Average Education" is the gross school enrollment ratio calculated as the number of students enrolled in secondary school, regardless of age, expressed as a percentage of the official school-age population corresponding to the same level of education. "Financial Development" is the index of financial development proposed by Svirydzenka (2016). "Forward Guidance" is a dummy variable that equals one when the central bank of a country uses forward guidance in a given year (Sutherland, 2022). "Gini Index, Wealth" is the Gini index of wealth concentration, where wealth is defined as personal non-financial assets plus personal financial assets minus personal debt. "Gov. Expenditure on Education" is the general government expenditure on education (current, capital, and transfers) as a percentage of GDP. "Gov. Subsidies" is the government expenditure on subsidies, grants, and other social benefits as a percentage of expenses. "Inflation" is the annual growth rate in the consumer price index calculated using the Laspeyres formula. "Population Growth" is the growth rate in the number of residents in a country regardless of their legal status or citizenship. "Real GDP per capita" is gross domestic product divided by midyear population (in thousands, constant 2015 U.S. dollars). "Total Population" is the *de facto* definition of population (in millions). "Wealth Top 1%, Top 10%, and Bottom 50%" are the net personal wealth shares held by the top 1%, top 10%, and bottom 50% groups in each country, respectively.

Figure 3.1: Trends in Wealth Inequality, Baseline Results



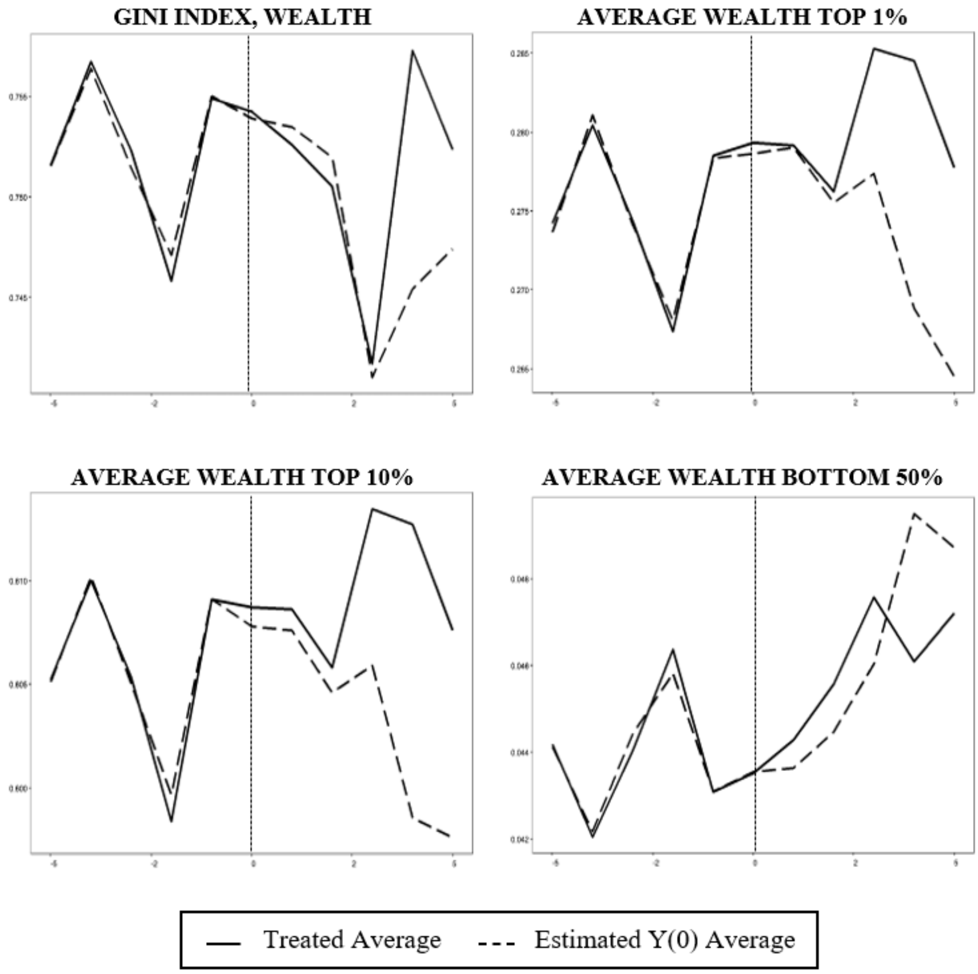
Note: The figure shows the average treatment effect of MaPP on the treated countries (ATT). "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years $(T_0 + 5)$ after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include average education, financial development, inflation, population, and real GDP per capita. The optimal number of factors is selected using cross-validation to minimize the MSPE. Appendix B.2 provides the list of countries and relative weights included in each synthetic control for each treated country.

Figure 3.2: Trends in Wealth Inequality, DSTI Ratio



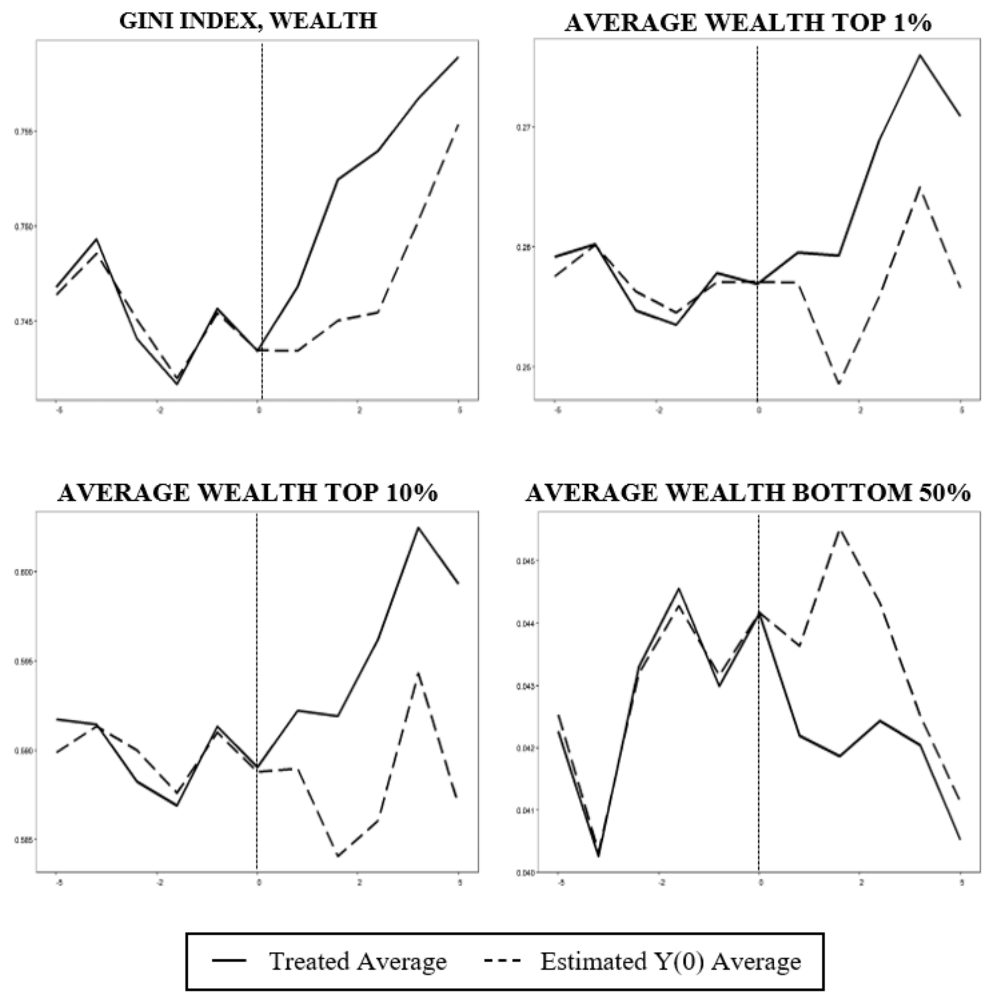
Note: The figure shows the average treatment effect of MaPP on countries that implement a DSTI ratio (ATT). "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years $(T_0 + 5)$ after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include average education, financial development, inflation, population, and real GDP per capita. The optimal number of factors is selected using cross-validation to minimize the MSPE.

Figure 3.3: Trends in Wealth Inequality, LTV Ratio



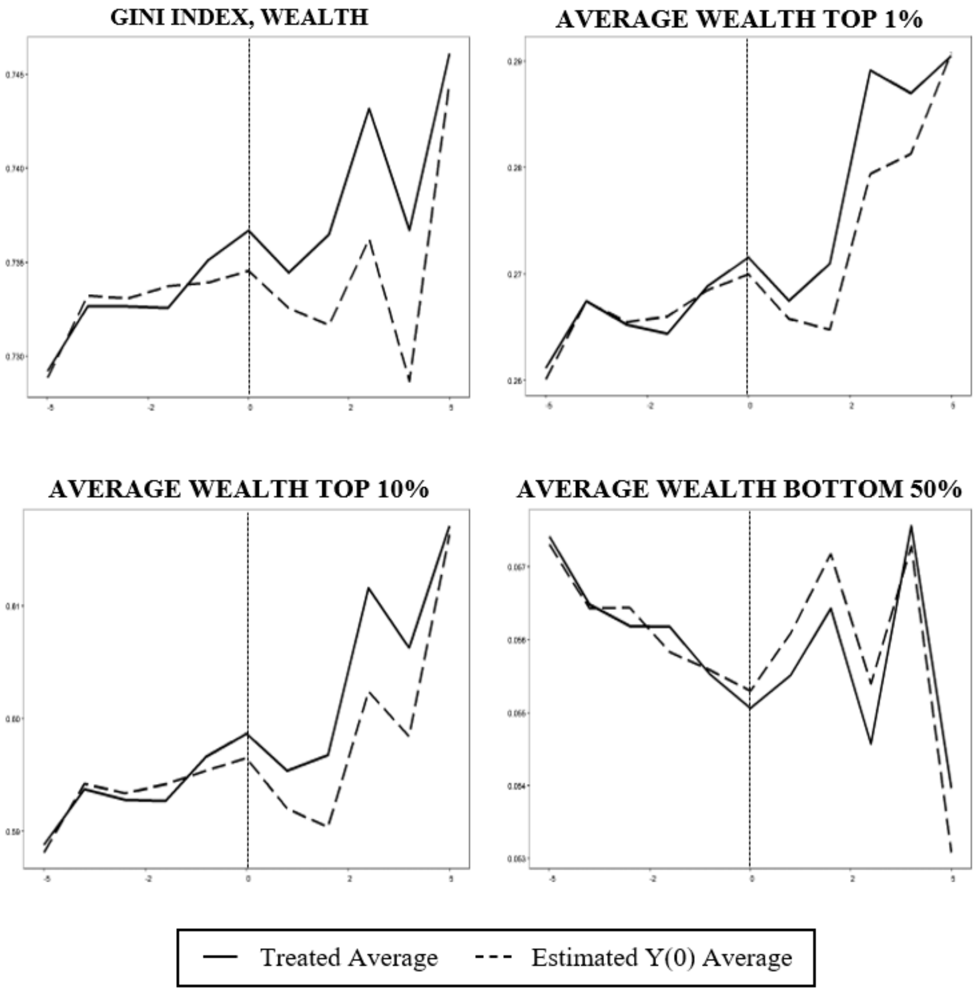
Note: The figure shows the average treatment effect of MaPP on countries that implement an LTV ratio (ATT). “Gini Index, Wealth” is the Gini index of wealth concentration. “Wealth” is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years ($T_0 + 5$) after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include average education, financial development, inflation, population, and real GDP per capita. The optimal number of factors is selected using cross-validation to minimize the MSPE.

Figure 3.4: Trends in Wealth Inequality, Advanced Economies



Note: The figure shows the average treatment effect of MaPP on treated advanced economies (ATT). "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years ($T_0 + 5$) after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include average education, financial development, inflation, population, and real GDP per capita. The optimal number of factors is selected using cross-validation to minimize the MSPE.

Figure 3.5: Trends in Wealth Inequality, Emerging Economies



Note: The figure shows the average treatment effect of MaPP on treated emerging economies (ATT). "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years ($T_0 + 5$) after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include average education, financial development, inflation, population, and real GDP per capita. The optimal number of factors is selected using cross-validation to minimize the MSPE.

Table 3.2: Impact of MaPP on Wealth Inequality Per Country Income Level

	Period	Gini Index	Wealth		
			Top 1%	**Top 10%**	**Bottom 50%**
Panel A: Full Sample	T_0+5	0.001	0.001*	0.002†	0.000†
		(0.001)	(0.001)	(0.001)	(0.000)
	T_0+6	0.004*	0.004*	0.006*	-0.002*
		(0.001)	(0.001)	(0.001)	(0.000)
	T_0+7	0.009*	0.010*	0.012*	-0.004*
		(0.002)	(0.002)	(0.002)	(0.001)
	T_0+8	0.017*	0.022*	0.022*	-0.006*
		(0.002)	(0.003)	(0.002)	(0.001)
	T_0+9	0.028*	0.037*	0.036*	-0.009*
		(0.003)	(0.004)	(0.003)	(0.001)
	T_0+10	0.034*	0.051*	0.045*	-0.010*
		(0.003)	(0.004)	(0.003)	(0.002)
Panel B: Advanced Economies	T_0+5	0.000	0.000	0.000	0.000
		(0.001)	(0.001)	(0.001)	(0.000)
	T_0+6	0.003*	0.002†	0.004*	-0.002*
		(0.001)	(0.001)	(0.001)	(0.000)
	T_0+7	0.011*	0.013*	0.011*	-0.005*
		(0.002)	(0.002)	(0.002)	(0.001)
	T_0+8	0.019*	0.025*	0.021*	-0.007*
		(0.002)	(0.003)	(0.002)	(0.001)
	T_0+9	0.025*	0.035*	0.028*	-0.008*
		(0.003)	(0.004)	(0.003)	(0.001)
	T_0+10	0.029*	0.047*	0.039*	-0.009*
		(0.004)	(0.004)	(0.003)	(0.001)
Panel C: Emerging Economies	T_0+5	0.002*	0.002	0.002†	0.000
		(0.001)	(0.001)	(0.001)	(0.000)
	T_0+6	0.004*	0.003	0.006*	-0.008†
		(0.002)	(0.003)	(0.002)	(0.001)
	T_0+7	0.008*	0.009*	0.011*	-0.002†
		(0.004)	(0.004)	(0.004)	(0.001)
	T_0+8	0.013*	0.015*	0.018*	-0.002*
		(0.005)	(0.005)	(0.005)	(0.002)
	T_0+9	0.018*	0.020*	0.024*	-0.002†
		(0.006)	(0.007)	(0.006)	(0.002)
	T_0+10	0.021*	0.021*	0.027*	-0.002†
		(0.007)	(0.001)	(0.008)	(0.003)

Note: The table presents the cumulative average treatment effects of MaPP on the treated countries (ATT) for each post-treatment period up to ten years after MaPP adoption. Panel A shows the cumulative ATT estimates for the full sample (baseline results). Panel B and C display the cumulative ATT estimates for advanced and emerging economies, respectively. "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. Standard errors are based on 1,000 parametric bootstraps at the country level and are reported in parentheses. "*" and "†" represent statistical significance at the 1% and 5% level, respectively.

Table 3.3: Impact of MaPP on Wealth Inequality Per Policy Tool

	Period	Gini Index	Wealth		
			Top 1%	**Top 10%**	**Bottom 50%**
Panel A: DSTI	T_0+5	0.001	-0.001	0.001	0.0002
		(0.001)	(0.001)	(0.001)	(0.000)
	T_0+6	0.006*	0.003*	0.006†	-0.001
		(0.002)	(0.002)	(0.003)	(0.000)
	T_0+7	0.018*	0.012*	0.018*	-0.006*
		(0.003)	(0.003)	(0.004)	(0.001)
	T_0+8	0.032*	0.021*	0.033*	-0.011*
		(0.004)	(0.004)	(0.005)	(0.002)
	T_0+9	0.044*	0.034*	0.048*	-0.015*
		(0.005)	(0.005)	(0.007)	(0.003)
	T_0+10	0.057*	0.047*	0.064*	-0.019*
		(0.006)	(0.006)	(0.008)	(0.003)
Panel B: LTV	T_0+5	0.000	0.001	0.001	0.000
		(0.000)	(0.001)	(0.001)	(0.000)
	T_0+6	-0.001	0.001	0.002	0.000
		(0.001)	(0.001)	(0.001)	(0.000)
	T_0+7	-0.002	0.002	0.003	0.002
		(0.002)	(0.002)	(0.002)	(0.000)
	T_0+8	-0.001	0.009*	0.010*	0.003†
		(0.002)	(0.003)	(0.002)	(0.001)
	T_0+9	0.007*	0.022*	0.022*	0.001
		(0.003)	(0.04)	(0.003)	(0.002)
	T_0+10	0.011*	0.033*	0.031*	-0.001
		(0.003)	(0.005)	(0.003)	(0.003)

Note: The table presents the cumulative average treatment effects of MaPP on the treated countries (ATT) for each post-treatment period up to ten years after MaPP adoption. Panel A shows the cumulative ATT estimates for the DSTI ratio. Panel B displays the cumulative ATT estimates for the LTV ratio. "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. Standard errors are based on 1,000 parametric bootstraps at the country level and are reported in parentheses. "*" and "†" represent statistical significance at the 1% and 5% level, respectively.

Chapter 4

The Paradox of Banking Prudential Regulation and Sovereign Risk

4.1. Introduction

There is a general consensus that prudential regulation improves financial stability. The reason is intuitively obvious: prudential regulation reduces non-performing loans and minimizes risk in the banking system. Yet, the idea that prudential regulation may also influence sovereign risk seems still to be lacking in the literature. This neglect is somewhat surprising given the implications of sovereign risk for financial stability.

This paper asks whether banking prudential regulation affects sovereign risk. No hard or fast answer can be given to this question, since prudential regulation may either increase or decrease sovereign risk. On the one hand, stricter prudential regulation prevents a deluge of risky loans. This could lead to favorable conditions in sovereign bond markets. If that's the case, prudential regulation may decrease sovereign risk. As a consequence, we expect governments to increase spending and issue additional debt. It is almost paradoxical that prudential regulation may reduce debt in the private sector while increasing debt in the public sector, but it is not implausible. On the other hand, stricter prudential regulation reduces credit availability and increases the cost of borrowing. This may result in firm bankruptcies and defaulting borrowers, which may hinder growth and reduce tax revenue. If so, prudential regulation may increase sovereign risk. In this case, governments may reduce spending and issue less debt. The net effect of these potentially opposing forces remains ambiguous.

We begin by proposing a simple framework that formalizes this trade-off between financial stability and credit availability, and why it matters for sovereign risk. We use this framework to show that there is an optimal level of prudential regulation that minimizes sovereign risk. In general, our results suggest that prudential regulation reduces sovereign risk and enables governments to spend more. At a certain point, though, too much regulation may impede growth and lead to an increase in sovereign risk. The effect of prudential regulation on sovereign risk depends on the level of financial stability, the stock of public debt, and the degree of economic development.

Next, we test this framework using a variety of econometric techniques and proxies for sovereign risk in 120 countries. We find empirical evidence supporting the view of a "paradox" in prudential regulation, whereby regulation reduces sovereign risk and enables governments to accumulate debt over time. Specifically, we find that the impact of an extra prudential rule on bond yields is between -0.021 and -0.045 percentage points in a year. These estimates are very similar when we use different proxies for sovereign risk like bond spreads and credit default swap (CDS) spreads. Consistent with these results, we also find that an extra prudential rule reduces the primary balance by -0.09 to -0.133 percentage points of GDP in the same period. Once again, the estimates remain in the same ballpark when we consider alternative fiscal variables like government expenditure as a percent of GDP or the debt-to-GDP ratio. Using different regression model specifications, we show that the effect of prudential regulation on sovereign risk is non-linear, with diminishing marginal returns beyond a certain level of sovereign debt. The effectiveness of prudential regulation in reducing sovereign risk depends on the level of government debt and the economic development of a country. These results are robust to several estimation methods that account for the endogeneity of prudential regulation. Overall, we come to the paradoxical conclusion that prudential regulation which is designed to prevent excessive leverage in the banking sector may result in too much debt in the public sector.

We explore a couple of explanations for this paradox. The first potential explanation is that prudential regulation constrains credit to the private sector while easing credit to the public sector. The reasoning is straightforward. As prudential rules mount, individuals and firms are pushed out of the credit market. Yet, banks have to maintain or expand profits. There is a possibility, then, that banks make credit cheaper and more easily available to the public sector. A simple way to test this is by examining how prudential regulation affects the share of credit to the public sector in relation to the private sector. For this line of reasoning to be correct, the share of credit to the public sector should increase disproportionately with the adoption of prudential rules. Notwithstanding, we find little evidence that this is the case.

A second possibility is that prudential regulation may improve the overall quality of credit. If so, then sovereign debt ratings should improve and governments should have easier and cheaper access to finance. Furthermore, banks should come to prefer government bonds to other non-earning bank balances because bonds are a reliable source of liquidity and collateral and they receive preferential treatment in regulatory requirements. This increased credit availability could, of course, induce governments to borrow more money at favorable terms. We test this hypothesis by looking at the impact of prudential rules on sovereign debt ratings and government bond issues. Our results provide strong evidence that prudential rules improve sovereign debt ratings and enable governments to issue more debt. These results are consistent with the view that prudential regulation leads to better sovereign debt ratings and increases banks' demand for government bonds (e.g., Afonso et al., 2012; Bonner, 2016).

Our paper builds on and contributes to a number of literatures. First, our findings are closely related to the recent literature examining the effects of financial regulation on banks' incentives to hold sovereign bonds (Acharya and Steffen, 2015; Bonner, 2016; Gropp et al., 2019). Previous papers use bank-level data to look at the effect of financial regulation on banks' balance sheets. Instead, we use a rich dataset to examine the broader effects of regulation on sovereign risk and ultimately on fiscal policy. As far as we know, nobody has yet provided empirical evidence on the impact of prudential regulation on fiscal developments. The results presented in this paper point to a paradox in prudential regulation: central banks may effectively fund government deficits through the implementation of strict prudential rules, especially when these rules do not improve financial stability. This finding is also in line with recent evidence that long-term financing operations reduce the perception of sovereign risk and promote financial stability (Afonso and Jalles, 2019).

Second, our findings add to the burgeoning literature on the real effects of prudential regulation. A few recent papers find that prudential regulation constrains credit and imperils growth, mainly by reducing private spending (e.g., Lim et al., 2011; Cerutti et al., 2017; Teixeira and Venter, 2023). We complement these papers by showing that prudential regulation also affects public spending. Importantly, we provide strong and consistent evidence of a causal effect of prudential regulation on the accumulation of sovereign debt. Our results stand up to a variety of endogeneity tests that include alternative proxies for sovereign risk and government spending, panel data models with fixed effects, two-step Generalized Method of Moment (GMM) estimators, and instrumental variables (IV) to control for differences in prudential regulation across countries.

The paper proceeds as follows. Section 4.2 proposes a simple framework that analyzes the effect of prudential regulation on sovereign risk. Section 4.3 explains the data. Section 4.4 explains the empirical strategy and presents the results. Section 4.5 looks at potential explanations for the effect of prudential regulation on sovereign risk. Section 4.6 concludes.

4.2. Prudential Regulation and Sovereign Risk

In this section, we review the literature to discuss the costs and benefits of prudential regulation with a focus on its effects on financial stability and growth[1]. We then propose a simple theoretical framework to formalize our discussion. This framework is useful to generate testable hypotheses, which we explore in subsequent sections. The hypotheses are tested using different empirical approaches that do not necessarily depend on the assumptions of this framework.

4.2.1. The Costs and Benefits of Prudential Regulation

A large and contentious literature has focused on the benefits of prudential regulation for financial stability. This literature shows that prudential regulation enhances financial stability by reducing the likelihood and severity of financial crises (e.g., Kraft and Galac, 2011). This finding is perhaps not too surprising, since prudential regulation is designed to ensure that financial institutions have limited risk exposure. As a consequence, prudential regulation reduces excessive leverage, the likelihood of bank failures, and the cost of bank runs and bailouts (e.g., Lim et al., 2011; Demirgüç-Kunt and Huizinga, 2010; Claessens et al., 2013). Similarly, prudential regulation often requires financial institutions to implement sound risk management practices, which reduces non-performing loans and other forms of financial distress (e.g., Laeven and Levine, 2009; Dell'Ariccia et al., 2012). These papers provide compelling evidence that prudential regulation improves financial stability, which should reduce systemic risk and lower the yields on government bonds.

While the benefits of prudential regulation have been widely studied, its costs have received much less attention. Theoretical papers suggest that prudential regulation has negative effects on growth by restricting credit and reducing consumption (Hall, 2011; Angellini et al., 2014; Farhi and Werning, 2016). Moreover, during periods of rapid deleveraging, prudential regulation may increase precautionary savings and further depress private spending

[1] A growing body of research analyzes how prudential regulation affects the redistribution of income and wealth. For an in-depth discussion on the redistributive effects of prudential regulation, see Delis et al. (2014), Frost (2018) and Teixeira (2023).

(Eggertsson and Krugman, 2012; Guerrieri and Lorenzoni, 2017). These ideas are supported by a small number of empirical studies showing that prudential regulation impedes growth (e.g., Lim et al., 2011; Cerutti et al., 2017; Akinci and Olmstead-Rumsey, 2018; Teixeira and Venter, 2023). This group of papers suggests that prudential regulation may harm growth, which may increase systemic risk and lead to higher yields on government bonds.

These potential effects of prudential regulation on credit and growth may also have implications for sovereign risk. A few recent papers find that prudential regulation influences banks' demand for government bonds. For example, Acharya and Steffen (2015) find that capital requirements influence banks' incentives to hold sovereign bonds. Similarly, Bonner (2016) shows that liquidity and capital buffers increase banks' exposure to sovereign bonds. Finally, Gropp et al. (2019) find that capital requirements lead to a more stable demand for government bonds, as banks increase their capital ratios by reducing risk-weighted assets.

So far, the literature does not explicitly address the impact of prudential regulation on sovereign risk and government spending. If prudential regulation is too strict, it may lead to a reduction in credit and growth, which could ultimately impact the ability of sovereigns to meet their debt obligations. However, if prudential regulation is too lax, this could also weaken investor confidence in the financial system, which may increase the likelihood of a financial crisis. In both cases, investors may begin to doubt the ability of governments to repay their debts and will demand higher yields on government bonds, making it more difficult for governments to finance their debt. To formalize the preceding discussion, we present a stylized framework in the next section.

4.2.2. A Simple Framework

Consider an economy that is composed of a representative agent, banks, and the government. The representative agent will maximize a lifetime utility function by consuming, saving, and investing in a risk-free asset. The banks provide loans to the agent for consumption and investment purposes, while the government provides public goods and services financed through taxes and debt issuance. Moreover, we assume that banks are subject to prudential regulation and that central banks impose these rules to ensure that banks have sufficient capital to cover potential losses from non-performing loans. The objective of the central bank is to choose an optimal amount of prudential rules, taking into account the trade-off between the benefits of financial stability and the costs of reduced lending.

The government's spending decision is constrained by the level of sovereign risk, which depends on both the government's outstanding debt and the stability of the banking sector.

Specifically, the higher the probability of sovereign default, the higher the (long-term) interest rate the government has to pay to finance its debt, which can crowd out private investment and hinder economic growth. Mathematically, these relationships may be described as follows:

$$r = \phi(D, \theta) \tag{4.1}$$

where r is the interest rate on government debt (i.e., bond yields), D is the level of government debt, and θ is the level of financial stability in the banking sector. For simplicity, we assume that $r > 0$ or governments would have an extra incentive to increase public spending.

The central bank's choice of optimal prudential stance affects the level of financial stability in the banking sector (θ) and determines the interest rate on government debt according to the function $\phi(.)$ above. The specific form of $\phi(.)$ depends on the characteristics of the economy. There are two cases here. If $\phi(.)$ is decreasing in θ, then stricter prudential regulation reduces the probability of default and the government can issue more debt at a lower interest rate. Conversely, if $\phi(.)$ is increasing in θ, a higher level of financial stability increases the interest rate on government debt. This is the case when prudential regulation is too strict, leading to a reduction in banking lending and a contraction in the economy. This could result in a higher probability of sovereign default since the reduction in economic activity leads to lower tax revenue and makes it more difficult to service existing debt, which raises the headline fiscal ratios. Thus, we can derive the following hypothesis:

Hypothesis 1: There exists an optimal level of prudential regulation, θ^*, that minimizes sovereign risk.

Assuming that the government wants to maximize spending subject to a target debt-to-GDP ratio, the government's spending decision can be derived from the flow government budget constraint, as follows:

$$\Delta D = G + rD - T \tag{4.2}$$

where G is primary government spending, T is government revenue, and ΔD is the change in government debt. Government spending determines the level of government debt, D, which in turn influences the interest rate on government debt. Following the literature, we expect higher levels of government spending and debt to reduce the effectiveness of prudential regulation (i.e., $\partial\theta/\partial G < 0$, $\partial\theta/\partial D < 0$). Furthermore, the marginal impact of G and D on the effectiveness of θ should decrease as G and D increase (i.e., $\partial^2\theta/\partial G^2 < 0$, $\partial^2\theta/\partial D^2 < 0$). Equally important, the effect of prudential regulation on government spending is non-linear. It is immediately obvious that $\partial\phi/\partial\theta$ and $\partial\phi/\partial\mathrm{D}$ are not constant for, at least, two reasons. First, the effect of prudential regulation is probably stronger for lower levels of prudential regulation, and weaker for higher levels of prudential regulation. Second, the effect of prudential regulation may also depend on a country's characteristics (ϕ), particularly on its level of financial stability and debt. These characteristics should be captured by the country's level of economic development (Demirgüç-Kunt and Detragiache, 1998). Based on our simple framework and the discussion above, we formulate the following hypotheses:

Hypothesis 2: If prudential regulation improves financial stability, higher levels of prudential regulation should lead to higher government spending. Conversely, if prudential regulation is too strict and worsens financial stability, government spending should remain stable or even decrease over time.

Hypothesis 3: The effectiveness of prudential regulation in reducing sovereign risk is influenced by the level of government debt, with diminishing marginal returns to prudential regulation beyond a certain level of debt.

Hypothesis 4: The relationship between prudential regulation, sovereign risk, and government spending, is mediated by the level of economic development.

Substituting (4.1) into the above equation, we obtain:

$$\Delta\mathrm{D} = \mathrm{G} - \mathrm{T} + \phi(\mathrm{D}, \theta)\mathrm{D} \tag{4.3}$$

Rearranging the equation, the government's primary spending decision can be expressed as follows:

$$\mathrm{G} = \Delta\mathrm{D} + \mathrm{T} - \phi(\mathrm{D}, \theta)\mathrm{D} \tag{4.4}$$

In summary, equation (4.4) shows that the government's spending decision is influenced by both the level of financial stability in the banking sector (θ) and the level of government debt (D). If prudential regulation improves financial stability, the government can increase spending without increasing taxes. However, if prudential regulation is too stringent, it may reduce bank lending, which could limit economic growth and, in turn, reduce tax revenue. This simple framework shows that central banks should strike a balance between financial stability and credit availability to promote growth and also consider, to some extent, that governments must maintain a stable debt-to-GDP ratio to ensure fiscal sustainability (see, for instance, Afonso and Jalles, 2017). Of course, a theory is only vindicable if its hypotheses are empirically valid. It is to these matters that we turn next.

4.3. Data

The data used in this paper is collected from multiple sources. Our primary variables of interest are sovereign risk and government spending. To assess sovereign risk, we analyze bond yields, bond spreads (relative to the US. Bond rate) and CDS spreads. This data comes from Datastream financial database. To examine government spending, we look at the primary balance, government expenditure as a percent of GDP, and debt-to-GDP ratio. These data are taken directly from the IMF Global Debt Database. As a supplementary analysis, we investigate the impact of prudential regulation on sovereign debt ratings and government bond issues. The sovereign debt ratings are a simple average of the rating given by the three major credit rating agencies, namely Moody's, Fitch, and Standard & Poor's. Finally, we calculate the growth rate in government bond net issues. The data is publicly available at the Bank of International Settlements (BIS).

We build a rating database with sovereign debt ratings attributed by the three main rating agencies: Moody's, Fitch Ratings, and Standard & Poor's. The data on sovereign debt ratings are collected from Bloomberg. The rating of a particular year is the rating attributed on the 31st of December. The ratings are grouped into 21 categories. The few observations in our sample with a rating below B- are given a value of two, while AAA observations receive a value of 21 (see the full numerical scale in Appendix C.2). The sovereign debt rating is calculated as a simple average of the ratings attributed by the three main rating agencies. Of course, not all countries have a rating attributed by the three agencies. In such cases, we calculate the average rating based on the ratings given by two agencies or rely on the single rating provided by the only agency available.

We measure the level of prudential regulation in each country as the number of prudential policies in effect every quarter. The data on prudential regulation is taken from Alam et al.(2019), who provide a comprehensive dataset of prudential policies implemented worldwide. We do not differentiate between borrower-based and capital-based measures nor do we take into account the strictness of the prudential rules. However, we do consider reversals in prudential policy. For instance, if a country tightens two prudential policies but loosens one previous policy, the overall change in the prudential stance for the quarter will be one. On average, each country in our sample has 2.373 prudential rules in effect each quarter. However, the standard deviation is 4.092 rules, which suggests that the level of prudential regulation varies considerably across countries. This measure is a simple and tractable way to compare the level of prudential regulation across countries over time.

In addition, we use several control variables to account for potential factors driving sovereign risk. These variables control for GDP growth, domestic credit, public debt, inflation, financial crises, and financial openness. Some model specifications use a standard fiscal reaction function that also includes the primary balance as a control variable. These variables are obtained from the IMF International Financial Statistics (IFS) database. Furthermore, we use data from other sources to supplement our analysis. For example, we rely on the CBI index proposed by Romelli (2022) to measure central bank independence based on a wide range of central bank characteristics in 154 countries. The CBI index ranges between 0 (no independence) and 1 (full independence). Finally, we use a rich and novel dataset constructed by Sutherland (2022) to identify the periods in which central banks use forward guidance. A more detailed description of the variables is provided in Appendix C.1.

Table 4.1 presents descriptive statistics for all the variables used in our analyses. Our main sample consists of a panel of 120 countries spanning the period 1990-2020. Figure 4.1 depicts the dynamics of sovereign risk in countries that implement prudential regulation at year 0 relative to other countries that do not implement prudential regulation at that time. The figure shows that the adoption of prudential regulation is, on average, preceded by a temporary peak in sovereign risk. The pattern in this figure suggests that prudential regulation reduces sovereign risk, but it also highlights the need to carefully address endogeneity in our analysis. The next section explains how we estimate the relationship between prudential regulation, sovereign risk and government spending in a way that mitigates endogeneity concerns.

4.4. Estimation and Results

4.4.1. How Does Prudential Regulation Affect Sovereign Risk?

If prudential regulation reduces sovereign risk, it may be the case that governments will increase spending and issue more debt at favorable terms. To test this prediction, we estimate a dynamic panel using different proxies for sovereign risk in the dependent variable, as shown below:

$$\mathrm{SR}_{\mathrm{i,t}} = \beta_0 + \beta_1 \mathrm{SR}_{\mathrm{i,t-4}} + \beta_2 \mathrm{PR}_{\mathrm{i,t-4}} + \beta \mathrm{X}_{\mathrm{i,t}} + \alpha_{\mathrm{i}} + \varepsilon_{\mathrm{i,t}} \tag{4.5}$$

where $SR_{i,t}$ is a measure of sovereign risk for country i in period t, $SR_{i,t-4}$ is the dependent variable lagged four quarters to capture the persistence of government spending over time, $PR_{i,t-4}$ is the level of prudential regulation for country i in period $t-4$, $X_{i,t}$ is a vector of country-specific characteristics that influence sovereign risk in that period, α_i are country fixed effects, and $\varepsilon_{i,t}$ is the error term. We lag the variable on prudential regulation to account for possible delayed effects on sovereign risk, since banks and households are slow to adjust their behavior to regulation (e.g., Cerutti et al, 2017; Teixeira and Venter, 2023)[2]. We consider three proxies for sovereign risk: government bond yields, government bond spreads, and sovereign CDS spreads. The model is estimated using pooled ordinary least squares (OLS), a within estimator, and a two-step GMM estimator. The standard errors are corrected for heteroskedasticity and serial correlation at the country level.

Table 4.2 reports the estimated effect of prudential regulation on sovereign risk. In all cases, the effect of prudential regulation on sovereign risk, shown in the first row, is negative and significant at the 1% level. The within estimates indicate that an extra prudential rule reduces bond yields, bond spreads and CDS spreads by -0.045, -0.025, and -0.021 percentage points, respectively. All coefficients are highly significant. The within estimates, however, may be biased because we included a lagged dependent variable (LDV) in the models. This variable accounts for the persistence of sovereign risk over time and improves the fit of the model. Nevertheless, an LDV may lead to "Nickell bias" when correlated with country-specific effects. This bias is expected to be small because our panel is fairly long and the number of periods is quite large.

Still, we repeat our estimations using a two-step GMM estimator. The only difference is that we treat the dependent variable and the variable on prudential regulation as endogenous and instrument them with lags. The first step of the GMM estimator uses the lagged values of the dependent variable and prudential regulation to estimate the relationship between these variables. The residuals from this first-step regression represent the part of the variation in sovereign risk that is not explained by the instrumental variables. The second step uses the residuals from the first-step regression to construct an estimator for the covariance matrix of the error term, which is then used to calculate the efficient GMM estimator. The GMM estimator ensures that the moment conditions are orthogonal to the first-step residuals and addresses potential endogeneity and measurement error in the explanatory variables.

The GMM estimates, shown in columns 4-6 of Table 4.2, are consistent estimates of the effect of prudential regulation on sovereign risk even when an LDV is included in the

[2] The reader should, however, be assured that the results are qualitatively similar when we do not lag this variable or we lag all the variables in the model.

model. As we anticipated, the GMM estimates are remarkably similar to the within estimates, thus confirming our baseline findings. The only difference is that the GMM estimator points to a greater impact of prudential regulation on CDS spreads, resulting in a reduction of approximately -0.040 percentage points in a year. Perhaps more interestingly, the GMM estimates indicate that bond yields fall faster than bond spreads. This is interesting because it means that the spread between the country's bond rate and the U.S. Treasury bond rate is narrowing. Put differently, prudential regulation increases the relative safety of a nation's government bonds when compared to a benchmark rate.

The estimated coefficients for the LDV are always between 0 and 1, which provides additional evidence that the GMM estimates are stable and consistent over time[3]. For transparency's sake, we report the number of countries and instruments in every model. This is especially useful to check whether the models suffer from the "too-many-instruments" problem (Alvarez and Arellano, 2003). The number of moment conditions in the GMM estimator is of the order of T. For large values of T, too many instruments can lead to an asymptotic bias of order $1/N$. We address this issue by computing the GMM estimates based on the last quarter of each year across countries. By doing so, we ensure that the number of instruments is smaller than the number of countries in each model. The validity of the GMM estimates is tested in the bottom rows of columns 4-6. Specifically, we report the *p*-value of a test for serial correlation in the first-differences residuals. The *p*-values for this test range from 0.325 to 0.977, which indicates that we cannot reject the assumption of no serial correlation. The *p*-values for the Hansen overidentification test range from 0.220 to 0.394, showing no evidence of model misspecification. Overall, our models are highly robust and they reveal a negative relationship between prudential regulation and sovereign risk.

To test for the existence of an optimal level of prudential regulation, as posited in our first hypothesis, we repeat the estimations using a quadratic term for prudential regulation. The sign of the quadratic term allows us to determine whether the relationship between prudential regulation and sovereign risk is concave or convex. Table 4.3 presents the results of these estimations, indicating that the coefficients of the linear terms are always negative, while the coefficients of the quadratic terms are consistently positive with statistical significance across all specifications. This suggests that the relationship between prudential regulation and sovereign risk is non-linear and likely convex. Initially, prudential regulation has a substantial impact on lowering sovereign risk, but this effect gradually diminishes as the level of prudential regulation increases. Beyond a certain level of prudential regulation, the

[3]The effect of the LDV on its current values should be positive and bounded between 0 and 1 to ensure dynamic stability.

negative effect may become positive, meaning that further increases in prudential regulation can increase sovereign risk. The quadratic term allows for the possibility that the relationship between prudential regulation and sovereign risk is not linear and suggests that there is an optimal level of prudential regulation that minimizes sovereign risk. These findings are consistent with our framework above and support our first hypothesis.

Overall, our results provide strong and consistent evidence that higher levels of prudential regulation are associated with lower sovereign risk. That said, we also find that too much regulation may jeopardize growth and lead to higher sovereign risk. This finding is one of our basic results that will reappear in the discussions below. Next, we turn to the impact of prudential regulation on government spending.

4.4.2. If Prudential Regulation Reduces Sovereign Risk, Does It Affect Government Spending?

Our second hypothesis suggests that if prudential regulation reduces sovereign risk, then the government may have the opportunity to increase spending and issue debt at more favorable terms. To test this prediction, we estimate a dynamic panel similar to the one above but with different proxies for fiscal developments in the dependent variable, as below:

$$\mathrm{GS}_{\mathrm{i,t}} = \beta_0 + \beta_1 \mathrm{GS}_{\mathrm{i,t-4}} + \beta_2 \mathrm{PR}_{\mathrm{i,t-4}} + \beta \mathrm{X}_{\mathrm{i,t-4}} + \alpha_{\mathrm{i}} + \varepsilon_{\mathrm{i,t}} \tag{4.6}$$

where $\mathrm{GS}_{\mathrm{i,t}}$ is a measure of government spending for country i in period t, $\mathrm{GS}_{\mathrm{i,t-4}}$ is the dependent variable lagged four quarters to capture the persistence of government spending over time, $\mathrm{PR}_{\mathrm{i,t-4}}$ is the level of prudential regulation for country i in period $\mathrm{t}-4$, $\mathrm{X}_{\mathrm{i,t-4}}$ is a vector of country-specific characteristics that influence government spending in period $\mathrm{t}-4$, α_{i} are country fixed effects, and $\varepsilon_{\mathrm{i,t}}$ is the error term. Note that we lag all our variables four quarters because a country-specific shock is not expected to have an immediate impact on government spending. If prudential regulation becomes tighter or inflation rises more than expected, the government will take some time to decide how to allocate tax revenue in the next government's budget. This is in sharp contrast with bond yields and CDS spreads that should respond almost immediately to events in the economy. As before, we consider three alternative proxies for fiscal developments: primary balance, government expenditure as a percent of GDP, and debt-to-GDP ratio. The model is estimated using the within estimator and a two-step GMM estimator.

Table 4.4 presents the estimated effect of prudential regulation on government spending. The columns are structured in a manner similar to those in Table 4.2. In all model specifications, we find that prudential regulation reduces the primary balance, increases government expenditure, and leads to an accumulation of debt over time. The within estimates show that an extra prudential rule lowers the primary balance by approximately -0.114 percentage points in a year. This result is consistent with a 0.075 percentage points hike in government expenditure and a consequent 0.148 percentage point rise in the debt-to-GDP ratio. To be sure, these coefficients are quite large. The average country in our sample has 2.3 prudential rules in effect every quarter; the evidence provided here suggests that prudential regulation causes the debt-to-GDP ratio of an average country to rise by at least 0.3 percentage points in a year. Our results also suggest that prudential regulation facilitates an increase in government spending only in situations where it enhances financial stability. To check this, we look at the coefficients of the financial crisis, which serve as a direct measure of financial stability. Our analysis reveals consistently positive and highly significant coefficients for both prudential regulation and financial crisis, except for the case of the primary balance which has a negative coefficient. These results lend support to our second hypothesis that prudential regulation leads to higher government spending only when it contributes to improving financial stability.

The estimates from GMM closely resemble the within estimates both in terms of size and magnitude. The results are also highly significant. However, some of the GMM estimates must be interpreted with caution for two reasons. First, the number of instruments exceeds, albeit by a very small margin, the number of countries when the dependent variable is the primary balance or the debt-to-GDP ratio. There is a chance that some of the coefficients and standard errors are biased. Second, when the dependent variable is government expenditure as a percent of GDP, the model does not suffer from the "too-many-instruments" problem but the p-value for the AR2 test indicates that there is some degree of serial correlation of the idiosyncratic error term. Despite these issues, the p-values for the Hansen tests suggest that our models are reasonably well specified. The point estimates from GMM are also similar to within estimates, which is quite reassuring.

Taken together, our results suggest that prudential regulation leads to an increase in government spending, consistent with our previous findings on sovereign risk. However, the magnitude of this effect may depend on the level of government debt. To explore this relationship further, we investigate whether the effect of prudential regulation on sovereign risk is influenced by the level of sovereign debt.

4.4.3. Is the Effect of Prudential Regulation on Sovereign Risk Influenced by the Level of Sovereign Debt?

Our third hypothesis predicts that the effectiveness of prudential regulation in reducing sovereign risk is influenced by the level of government debt, with diminishing marginal returns to prudential regulation beyond a certain level of debt. To test this, we include an interaction term between the level of prudential regulation and sovereign debt in each country:

$$\begin{aligned} \mathrm{SR}_{\mathrm{i,t}} = \beta_0 + \beta_1 \mathrm{SR}_{\mathrm{i,t-4}} + \beta_2 \mathrm{PR} * \mathrm{Debt}_{\mathrm{i,t-4}} \\ + \beta_3 \mathrm{PR}_{\mathrm{i,t-4}} + \beta_4 \mathrm{Debt}_{\mathrm{i,t-4}} + \beta_{\mathrm{i}} \mathrm{X}_{\mathrm{i,t-4}} + \alpha_{\mathrm{i}} + \varepsilon_{\mathrm{i,t}} \end{aligned} \tag{4.7}$$

where $\mathrm{SR}_{\mathrm{i,t}}$ is a measure of sovereign risk for country i in period t, $\mathrm{SR}_{\mathrm{i,t-4}}$ is the dependent variable lagged four quarters to capture the persistence of sovereign risk over time, $\mathrm{PR} * \mathrm{Debt}_{\mathrm{i,t-4}}$ is an interaction term between prudential regulation and debt level for each country i in period $\mathrm{t}-4$, $\mathrm{PR}_{\mathrm{i,t-4}}$ is the level of prudential regulation for country i in period $\mathrm{t}-4$, $\mathrm{Debt}_{\mathrm{i,t-4}}$ is a dummy variable that equals 1 when the country debt level in the period is above the median debt level of the country during the sample period, $\mathrm{X}_{\mathrm{i,t-4}}$ is a vector of country-specific characteristics that influence sovereign risk in period $\mathrm{t}-4$, α_{i} are country fixed effects, and $\varepsilon_{\mathrm{i,t}}$ is the error term. We replicate the previous analysis using the same estimators and proxies for sovereign risk.

Table 4.5 presents the results for the interaction term between prudential regulation and sovereign debt in our models. The coefficients reported in the first row capture the change in the impact of prudential regulation on sovereign risk as government debt increases. In almost all model specifications, the coefficients are negative and statistically significant, ranging from -0.065 to -0.366 percentage points. Moving across the columns, the magnitudes of the coefficients become stronger. These results indicate that prudential regulation is often implemented in a way that is not effective. The benefits of prudential regulation may be realized up to a certain level of debt, after which increasing regulation has only negative effects on sovereign risk. These findings are consistent with our third hypothesis that the effect of prudential regulation on sovereign risk depends on the level of government debt.

In summary, our findings suggest that, as government debt levels increase, the effectiveness of prudential regulation in reducing sovereign risk diminishes. Put simply, prudential regulation is most effective in reducing sovereign risk when government debt levels are relatively low. These results reinforce our previous conclusion that prudential regulation exhibits diminishing marginal returns beyond a certain point of government debt.

4.4.4. Is the Effect of Prudential Regulation on Sovereign Risk Mediated by the Level of Economic Development?

Our framework also suggests that a country's level of economic development could influence the effectiveness of prudential regulation in reducing sovereign risk. To test this issue more formally, we include an interaction term between prudential regulation and the level of economic development, as below:

$$\begin{aligned} SR_{i,t} = & \beta_0 + \beta_1 SR_{i,t-4} + \beta_2 PR * ED_{i,t-4} \\ & + \beta_3 PR_{i,t-4} + \beta_4 ED_{i,t-4} + \beta_i X_{i,t-4} + \alpha_i + \varepsilon_{i,t} \end{aligned} \tag{4.8}$$

where $SR_{i,t}$ is a measure of sovereign risk for country i in period t, $SR_{i,t-4}$ is the dependent variable lagged four quarters to capture the persistence of sovereign risk over time, $PR * ED_{i,t-4}$ is an interaction term between prudential regulation and the level of economic development of country i in period $t-4$, $PR_{i,t-4}$ is the level of prudential regulation for country i in period $t-4$, $ED_{i,t-4}$ is a dummy variable that equals 1 when a country is classified as an upper-middle or high-income economy according to the World Bank's country classification by income level and zero otherwise, $X_{i,t-4}$ is a vector of country-specific characteristics that influence sovereign risk in period $t-4$, α_i are country fixed effects, and $\varepsilon_{i,t}$ is the error term. Once again, we repeat the analysis using the same estimators and proxies for sovereign risk as before.

Table 4.6 presents the results. The coefficient of the interaction term is negative and highly significant in all models, suggesting that the effectiveness of prudential regulation in reducing sovereign risk varies with the level of economic development. Specifically, our results indicate that prudential regulation is more effective in reducing sovereign risk in countries with lower levels of economic development. This effect is not only significant, but it is also sizable. The coefficients for the interaction term range from -0.169 percentage points for bond spreads to -0.390 percentage points for CDS spreads. In general, the estimated coefficients are higher for CDS spreads. One possible explanation for this is that CDS spreads are generally more sensitive to changes in sovereign risk than bond yields. Alternatively, CDS markets are typically less liquid than bond markets, which could amplify the impact of changes in sovereign risk on CDS spreads. Further research would be necessary to determine the exact reasons why CDS spreads are more responsive to changes in prudential regulation in countries with varying degrees of development. Regardless, our findings are robust to various proxies of sovereign risk and estimation methods.

Our findings are also consistent with a small body of literature that has investigated the effects of prudential regulation using cross-country data. A few papers argue that prudential regulation has a stronger effect on emerging economies with underdeveloped financial systems (e.g., Cerutti et al., 2017; Teixeira, 2022). While these papers do not specifically address sovereign risk, they suggest that the impact of prudential regulation is tied to the level of development of the financial system. In a related study, Dieckmann and Plank (2012) have shown that financial instability can increase sovereign risk by causing government interventions that transfer risk from the private to the public sector. Interestingly, the authors observe that the transfer of risk is higher in countries with greater exposure to the financial system, particularly to the subprime mortgage sector. Consistent with these ideas, our findings suggest that prudential regulation has a more substantial impact on sovereign risk in emerging economies, where financial systems tend to be less developed and stable over time. This is important because most of these countries are also subject to significant fiscal constraints. Section 4.5 explores the reasons why prudential regulation reduces sovereign risk, particularly in emerging economies.

4.4.5. Controlling for Potential Endogeneity

There are two potential concerns with the analysis above. First, it is possible that sovereign risk or government spending determines the stringency of prudential regulation rather than the other way around. If sovereign risk is high, the central bank may impose stricter prudential regulations to reduce the risk of default. Second, some omitted country characteristics may be correlated with prudential regulation. For instance, political instability or the quality of the legal system may affect the efficacy of prudential regulation. These potential sources of endogeneity may bias the estimates. This bias, however, is likely to be small because the within estimates control for time-invariant unobservable factors and the GMM estimates control for both observable and unobservable factors.

That said, we repeat the estimations using a two-stage least squares (2SLS) regression as an additional robustness check. The 2SLS regression accounts for the endogeneity between prudential regulation and sovereign risk by using an IV that is correlated with prudential regulation but not directly correlated with sovereign risk or government spending. In the first stage, the IV is used to estimate the effect of prudential regulation on the endogenous variable (either sovereign risk or government spending). The IV is correlated with prudential regulation but is not correlated with the error term in this first-stage regression. This allows us to isolate the exogenous variation in prudential regulation and obtain an estimate that is not biased by endogeneity. In the second stage, the predicted values of prudential regulation from the first stage are used as the explanatory variable in the regression for sovereign risk or government spending. The 2SLS regression estimates the causal effect of prudential regulation adjusted for potential endogeneity.

We use two instruments. The first instrument is central bank independence (CBI). An independent central bank is more likely to have the power to enforce prudential regulation without political interference (e.g., Klomp and Haan, 2009; Valencia and Ueda, 2012; Doumpos et al., 2015). At the same time, CBI is exogenous to banks' behavior and should not affect sovereign risk. Moreover, CBI changes over time and across countries, providing the necessary variation for isolating the causal effect of financial stability on sovereign risk. We rely on the CBI index proposed by Romelli (2022), which covers a wide range of central bank characteristics in 154 countries. The results from the first-stage IV regression confirm that CBI is a valid instrument: the prudential regulation coefficient is highly correlated (2.624) and significant at the 1% level (*t*-value: 4.210).

Table 4.7 presents the estimates of the effect of prudential regulation on sovereign risk using CBI as an instrument. We confirm that prudential regulation reduces sovereign risk. In particular, the estimated effect of an extra prudential rule on bond yields, bond spreads and CDS spreads is -0.806, -0.360, and -0.140 percentage points, respectively. These results are extremely close to the within estimates and the GMM estimates obtained before. In fact, the coefficients become slightly more negative when we instrument prudential regulation with CBI. This makes sense because more independent central banks are also more likely to manage monetary policy and maintain financial stability in the country. Additionally, more independent central banks signal to investors a stronger commitment to financial stability, which could further reduce sovereign risk (e.g., Klomp and Haan, 2009). These estimates largely corroborate the findings in our previous analysis, providing further evidence that our estimates are highly robust.

However, the use of CBI as an instrument is unlikely to isolate the effect of prudential regulation on government spending. This is because CBI is inversely related to government spending, as independent central banks are less likely to finance government deficits. To further examine the relationship between prudential regulation and government spending, we use forward guidance as an alternative instrument. We contend that central banks using forward guidance enforce stricter prudential rules, but forward guidance should not affect, at least directly, the level of government spending. The results from the first-stage IV regression show that forward guidance is a valid instrument: the coefficient for prudential regulation is positively correlated (2.219) and significant at the 1% level (*t*-value: 8.219).

Table 4.8 presents estimates of the effect of prudential regulation on government spending using forward guidance as an instrument. Once more, we find our results substantially unchanged: an extra prudential rule decreases the primary balance by -0.239 percentage points while increasing government expenditure and debt-to-GDP by 0.126 and 0.387 percentage points, respectively. These results point to a stronger impact of prudential regulation on government spending than previously suggested. This makes sense because the use of forward guidance isolates the exogenous variation in prudential regulation that is not affected by changes in government spending. Even this measure, though, is not immune to bias, as forward guidance could be used to stabilize market expectations during times of fiscal instability, potentially affecting public spending[4]. These results indicate that our previous estimates are on the cautious side and that the true effect of prudential regulation on government spending may be even larger.

Through various model specifications, our results consistently reveal that prudential regulation reduces sovereign risk and increases government spending, rather than the other way around. The resulting financial stability enables governments to take on more debt over time. The next question is why prudential regulation affects sovereign risk.

4.5. Why Prudential Regulation Reduces Sovereign Risk?

Having determined that prudential regulation reduces sovereign risk and increases government spending, we now examine why this occurs. We consider two hypotheses. The first hypothesis is that prudential regulation constrains credit to the private sector but eases credit to the public sector. The simplest way to test this is to check whether the share of credit to the public sector relative to the private sector increases disproportionately with the adoption of prudential rules.

Table 4.9 presents the estimates of the effect of prudential regulation on the public-to-private credit ratio. The first column reports within estimates. The fourth column repeats the estimation using the two-step GMM that mitigates endogeneity concerns. In both cases, we find little evidence that prudential rules lead to an increase in the public-to-private credit ratio. On the contrary, our results point to a marginal decrease in the public-to-private credit ratio by the end of the year. The estimated coefficients range from -0.001 to -0.002 percentage points in a year, which suggests that prudential regulation has more serious consequences for private credit than public credit.

[4] In this case, forward guidance could influence public spending. Nonetheless, governments usually need time to adjust their fiscal policy. Additionally, we control for periods of financial instability in the 2SLS regressions.

A second hypothesis is that prudential regulation improves the overall quality of credit, which reduces sovereign risk and makes government bonds relatively more attractive. This can encourage governments to increase spending and issue more debt at favorable terms. To check for this, we examine the impact of prudential rules on sovereign debt ratings and government bond net issues. If prudential rules reduce sovereign risk, they should improve sovereign debt ratings. As a consequence, governments should issue more debt than they redeem in those years.

The second and third columns of Table 4.9 provide strong evidence supporting this hypothesis. Our results suggest that prudential regulation increases sovereign credit ratings and allows governments to issue more bonds. This effect is both sizable and significant. We find that the introduction of an extra prudential rule has a positive impact on the sovereign rating of an average country, increasing it by an average of 0.014 units on a 0-21 scale. This improvement in the rating is associated with a growth of 0.135-0.199 percentage points in the net issuance of government bonds. Our findings suggest that prudential regulation enhances the overall credit quality and facilitates the issuance of government debt at more favorable terms.

4.6. Conclusion

This paper uncovers a paradoxical relationship between prudential regulation, sovereign risk, and government spending. While prudential regulation may lead to a reduction in private debt, it also appears to increase public debt. We provide strong and consistent evidence that prudential regulation reduces sovereign risk and enables governments to increase spending over time. This is because prudential regulation improves a country's overall credit quality, thereby enabling governments to access debt markets on more favorable terms. The effects of prudential regulation on sovereign risk are generally stronger in countries with lower levels of debt and economic development.

These findings have important implications for policymakers and central banks. On the one hand, prudential regulation helps to reduce the risk of financial instability by limiting excessive lending to the private sector. On the other hand, it may lead to an increase in public sector debt, which can have negative consequences for fiscal sustainability and economic growth. As such, policymakers need to strike a balance between promoting financial stability and ensuring sustainable public finances. Our findings also show that prudential regulation can be an effective tool to reduce sovereign risk and enable governments to increase spending, particularly in countries with relatively low levels of government debt or economic development.

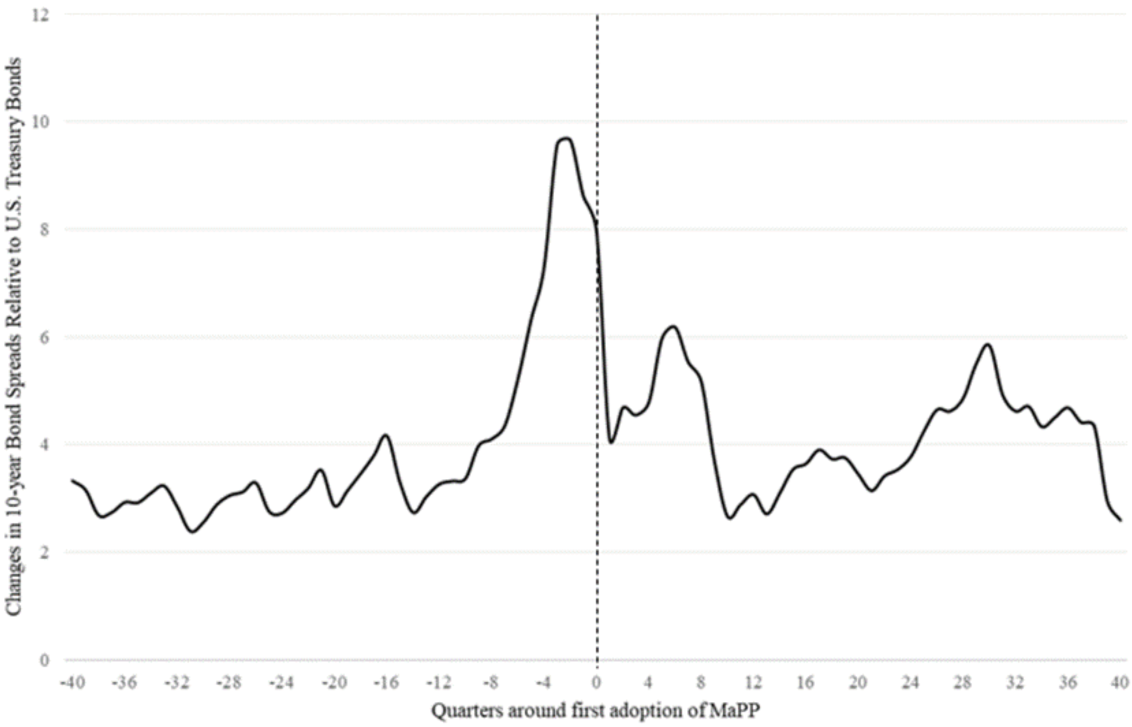
Despite the significant insights gained from our analysis, several questions remain unanswered. For instance, what is the optimal level of prudential regulation? While we provide evidence that a certain level of prudential regulation can minimize sovereign risk, we also show that a one-size-fits-all approach is not feasible. The optimal level of prudential regulation will vary across countries and over time, depending on a range of factors, including institutional factors, the structure of the financial system, and macroeconomic events. More research is needed on the way these factors influence the impact of prudential regulation on sovereign risk. Another key question is how prudential rules interact with monetary and fiscal policy to reduce sovereign risk. Central banks need guidance on how best to balance these various policy tools, and yet there is a lack of research in this area. Finally, future research could benefit from a more granular analysis of the impact of individual prudential tools on sovereign risk. For example, how do capital- and borrower-based measures affect sovereign risk? A more nuanced analysis is needed to better understand how different prudential rules affect sovereign risk both across and within countries. Answering these questions could throw more light on the paradox of prudential regulation.

Table 4.1: Summary Statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
PR	15,004	2.373	4.092	0	32
Crisis	15,004	0.215	0.411	0	1
Bond Yield	8,703	7.015	5.036	0.620	19.700
Bond Spread	8,703	3.004	4.665	-2.117	15.137
CBI	8,684	0.596	0.195	0.142	0.929
CDS	3,059	188.735	144.461	25.610	530
Credit-to-GDP	10,152	57.185	48.196	0.491	304.575
Debt-to-GDP	10,152	55.993	44.644	5.627	155.079
Forward Guidance	15,004	0.078	0.267	0	1
GDP Growth	10,838	1.181	4.821	-9.053	11.935
Gov. Expenditure	12,424	31.191	11.862	10.601	57.274
Gov. Bonds Net Issues	6,761	-2.107	24.449	-183.429	95.364
Inflation	14,205	1.553	2.143	-0.949	8.004
Openness	11,552	0.623	1.582	-1.927	2.311
Primary Balance	9,580	0.625	4.537	-9.243	19.613
Public-Private Credit	3,403	0.559	0.390	0.041	1.929
Sovereign Credit Ratings	9,477	14.318	5.120	2	21

Note: This table presents summary statistics for the main variables used in our analysis. "PR" is an overall cumulative macroprudential policy index that sums all cumulative prudential rules in a country in every quarter. "PR Demand-side" are measures targeted at borrowers, particularly LTV and DSTI ratios. "PR Supply-side" are measures targeted at financial institutions, such as capital requirements, countercyclical and conservation buffers, liquidity requirements, and other limits to credit growth. "Crisis" is an indicator variable that equals one whenever a country experiences a financial crisis in a given quarter based on the dataset of Laeven and Valencia (2018). "Bond Yield" is the 10-year government bond yield of a country in a given quarter. "Bond Spread" is the 10-year bond yield of a country relative to the U.S. Treasury bond rate. "CBI" is the index of central bank independence proposed by Romelli (2022). "CDS" is the 10-year sovereign credit default swap (CDS) spread of a country in a given quarter. "Credit-to-GDP" is domestic credit to the private sector as a percentage of GDP. "Debt-to-GDP" is gross public debt as a percent of GDP. "GDP Growth" is the growth rate of GDP adjusted for inflation. "Gov. Expenditure" is total government expenditure as a percent of GDP. "Forward Guidance" is a dummy variable that equals one whenever the central bank of a country uses forward guidance in a given year (Sutherland, 2022). "Gov. Bonds Net Issues" is the growth rate in the net issue of government bonds (gross issue – redemptions). "Inflation" is the growth rate in the consumer price index using the Laspeyres formula. "Openness" is the Chinn-Ito financial openness index. "Primary Balance" is primary net lending minus borrowing as a percent of GDP. "Public-Private Credit" is the ratio of credit to the general government over domestic credit to the private sector. "Sovereign Credit Ratings" is a simple average of the sovereign credit ratings of the main credit rating agencies (Fitch, Moody's, and S&P500). All continuous variables are winsorized at the 1st and 99th percentile.

Figure 4.1: Sovereign Risk Before and After the Adoption of Prudential Regulation



Note: This figure plots the evolution of sovereign risk as measured by the average 10-year bond spread in each country relative to the U.S. Treasury bond before and after the adoption of PR. The vertical axis plots the 4-quarter ahead bond spreads to account for the potential delayed effects of PR on sovereign risk. The horizontal axis corresponds to the time (in quarters) relative to the implementation of PR. To facilitate visualization, the x-axis is restricted to ten years prior to and after the adoption of PR.

Table 4.2: Effect of Prudential Regulation on Sovereign Risk

	Within Estimates			GMM Estimates		
	Bond Yield	Bond Spread	CDS	Bond Yield	Bond Spread	CDS
	(1)	(2)	(3)	(4)	(5)	(6)
PR	-0.045***	-0.025**	-2.139***	-0.073***	-0.027***	-4.024***
	(0.014)	(0.012)	(0.601)	(0.006)	(0.006)	(0.254)
LDV	0.752***	0.633***	0.352***	0.727***	0.682***	0.545***
	(0.031)	(0.037)	(0.035)	(0.006)	(0.006)	(0.014)
GDP Growth	-0.017***	-0.026***	-0.371*	-0.005	-0.024***	1.308***
	(0.006)	(0.006)	(0.199)	(0.004)	(0.005)	(0.246)
Inflation	0.198***	0.164***	7.319**	0.304***	0.266***	19.630***
	(0.033)	(0.033)	(3.269)	(0.014)	(0.017)	(0.772)
Crisis	0.468***	0.570***	25.837	0.621***	0.728***	39.070***
	(0.154)	(0.176)	(20.313)	(0.044)	(0.046)	(2.185)
Openness	-0.039	-0.071	-19.212	-0.115***	-0.112***	-14.249***
	(0.094)	(0.099)	(14.467)	(0.031)	(0.023)	(1.493)
Credit-to-GDP	-0.003	0.003	1.150***	-0.007***	-0.008***	-0.329***
	(0.003)	(0.003)	(0.368)	(0.001)	(0.001)	(0.032)
Debt-to-GDP	-0.005	0.003	0.206	-0.001***	0.000	0.506***
	(0.003)	(0.004)	(0.368)	(0.000)	(0.001)	(0.038)
Intercept	1.632***	0.334	26.663	1.853***	1.029***	79.096***
	(0.458)	(0.413)	(49.234)	(0.121)	(0.092)	(3.813)
Observations	4,798	4,798	2,274	1,170	1,170	514
Adjusted R^2	0.893	0.862	0.334			
Countries	79	79	50	77	77	50
Instruments				66	65	47
AR2 test *p*-value				0.977	0.325	0.577
Hansen test *p*-value				0.220	0.394	0.275

Note: This table reports the estimated effects of prudential regulation (PR) on sovereign risk as measured by government bond yields (%), government bond spreads relative to the U.S. Treasury bond rate (%) and sovereign credit default swaps (CDS) spreads (in basis points). Cols.1-3 present results from the within estimator, which controls for country fixed effects. Cols.4-6 present results from the two-step system GMM (Arellano and Bover, 1995). The estimates from GMM are based on the last quarter of each year to ensure that the number of instruments is smaller than the number of countries. The variable on PR is lagged four quarters to account for delayed effects on sovereign risk. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that an increase in sovereign risk is a consequence of past risk levels or country-specific risk perceptions. The AR2 row reports the *p*-value for the test of serial correlation in the residuals. Additionally, we provide the number of instruments in the two-step GMM and the *p*-value of the Hansen test of overidentification. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Table 4.3: Optimal Level of Prudential Regulation on Sovereign Risk

	Within Estimates			GMM Estimates		
	Bond Yield	Bond Spread	CDS	Bond Yield	Bond Spread	CDS
	(1)	(2)	(3)	(4)	(5)	(6)
PR	-0.135***	-0.086***	-4.904***	-0.228***	-0.177***	-8.015***
	(0.027)	(0.023)	(1.562)	(0.003)	(0.002)	(0.478)
PR^2	0.005***	0.004***	0.179**	0.011***	0.011***	0.385***
	(0.001)	(0.001)	(0.087)	(0.000)	(0.000)	(0.026)
LDV	0.690***	0.588***	0.276***	0.644***	0.600***	0.626***
	(0.027)	(0.025)	(0.037)	(0.002)	(0.001)	(0.011)
GDP Growth	-0.015*	-0.020***	-0.371	0.004**	-0.015***	0.042
	(0.008)	(0.007)	(0.232)	(0.002)	(0.001)	(0.381)
Inflation	0.248***	0.222***	5.553	0.355***	0.354***	22.528***
	(0.055)	(0.059)	(4.240)	(0.005)	(0.001)	(0.433)
Crisis	0.679***	0.753***	62.643*	0.890***	0.886***	61.139***
	(0.241)	(0.283)	(32.276)	(0.009)	(0.014)	(1.001)
Openness	0.012	-0.027	-26.650	-0.177***	-0.175***	-7.571***
	(0.117)	(0.118)	(21.486)	(0.005)	(0.005)	(1.325)
Credit-to-GDP	-0.003	0.003	1.207***	-0.009***	-0.011***	-0.238***
	(0.003)	(0.003)	(0.375)	(0.000)	(0.000)	(0.040)
Debt-to-GDP	-0.003	0.007*	0.088	0.000	0.002***	0.370***
	(0.003)	(0.004)	(0.468)	(0.001)	(0.001)	(0.046)
Intercept	1.932***	0.132	53.092	2.639***	1.552***	57.221***
	(0.456)	(0.432)	(60.957)	(0.050)	(0.045)	(4.463)
Observations	4,798	4,798	2,147	1,17	1,17	514
Adjusted R^2	0.867	0.822	0.247			
Countries	79	79	50	77	77	50
Instruments				95	94	76
AR2 test p-value				0.808	0.845	0.677
Hansen test p-value				0.834	0.908	0.980

Note: This table reports estimates for the impact of the quadratic term of prudential regulation (PR) on sovereign risk to determine whether there is an optimal level of prudential regulation that minimizes sovereign risk. Cols.1-3 present results from the within estimator, which controls for country fixed effects. Cols.4-6 present results from the two-step system GMM (Arellano and Bover, 1995). The estimates from GMM are based on the last quarter of each year to ensure that the number of instruments is smaller than the number of countries. The variable on PR is lagged four quarters to account for delayed effects on sovereign risk. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that an increase in sovereign risk is a consequence of past risk levels or country-specific risk perceptions. The AR2 row reports the *p*-value for the test of serial correlation in the residuals. Additionally, we provide the number of instruments in the two-step GMM and the *p*-value of the Hansen test of overidentification. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Table 4.4: Effect of Prudential Regulation on Government Spending

	Within Estimates			GMM Estimates		
	Primary Balance	Gov. Expenditure	Debt-to-GDP	Primary Balance	Gov. Expenditure	Debt-to-GDP
	(1)	(2)	(3)	(4)	(5)	(6)
PR	-0.114***	0.075**	0.148**	-0.133***	0.107***	0.104***
	(0.029)	(0.027)	(0.065)	(0.010)	(0.014)	(0.016)
LDV	0.558***	0.826***	0.928***	0.708***	0.807***	0.986***
	(0.039)	(0.021)	(0.018)	(0.011)	(0.016)	(0.007)
GDP Growth	2.084***	-0.957	0.231	0.101***	-0.059***	-0.028
	(0.507)	(0.652)	(2.552)	(0.012)	(0.018)	(0.017)
Inflation	-0.019	-0.036	-0.033	0.027	-0.004	-0.283***
	(0.042)	(0.038)	(0.141)	(0.021)	(0.021)	(0.028)
Crisis	-0.898**	0.347	1.323	-0.611***	-0.400***	0.079
	(0.425)	(0.333)	(1.047)	(0.112)	(0.105)	(0.091)
Openness	-0.443***	0.085	0.596*	-0.216***	0.707***	0.121**
	(0.117)	(0.106)	(0.349)	(0.027)	(0.065)	(0.040)
Credit-to-GDP	-0.685	0.634	3.400**	0.003***	0.012***	0.011***
	(0.431)	(0.548)	(1.663)	(0.001)	(0.002)	(0.002)
Debt-to-GDP	0.011**	-0.017***		0.008***	0.008**	
	(0.005)	(0.005)		(0.001)	(0.003)	
Prim. Balance			-0.465***			-0.304***
			(0.093)			(0.009)
Intercept	0.986	6.048***	1.103	-0.257*	4.712***	0.832**
	(0.544)	(0.820)	(1.528)	(0.125)	(0.367)	(0.309)
Observations	4,682	6,365	6,296	1,144	1,553	1,148
Adjusted R^2	0.321	0.648	0.859			
Countries	63	80	63	64	80	64
Instruments				67	67	67
AR2 test p-value				0.931	0.033	0.412
Hansen test p-value				0.407	0.118	0.331

Note: This table reports estimates for the impact of the quadratic term of prudential regulation (PR) on sovereign risk to determine whether there is an optimal level of prudential regulation that minimizes sovereign risk. Cols.1-3 present results from the within estimator, which controls for country fixed effects. Cols.4-6 present results from the two-step system GMM (Arellano and Bover, 1995). The estimates from GMM are based on the last quarter of each year to ensure that the number of instruments is smaller than the number of countries. The variable on PR is lagged four quarters to account for delayed effects on sovereign risk. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that an increase in sovereign risk is a consequence of past risk levels or country-specific risk perceptions. The AR2 row reports the *p*-value for the test of serial correlation in the residuals. Additionally, we provide the number of instruments in the two-step GMM and the *p*-value of the Hansen test of overidentification. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Table 4.5: Effect of Prudential Regulation on Sovereign Risk, Level of Sovereign Debt

	Within Estimates			GMM Estimates		
	Bond Yield	Bond Spread	CDS	Bond Yield	Bond Spread	CDS
	(1)	(2)	(3)	(4)	(5)	(6)
PRxDebt	-0.065**	-0.063*	-2.359	-0.366***	-0.327***	-9.877***
	(0.027)	(0.033)	(2.022)	(0.018)	(0.020)	(0.309)
PR	-0.017	0.019	-0.449	0.251***	0.267***	6.009***
	(0.022)	(0.028)	(1.743)	(0.022)	(0.018)	(0.440)
Debt	0.081	-0.089	-38.212***	0.799***	0.681***	26.613***
	(0.201)	(0.214)	(12.428)	(0.058)	(0.065)	(2.477)
LDV	0.695***	0.590***	0.273***	0.633***	0.606***	0.617***
	(0.028)	(0.025)	(0.037)	(0.007)	(0.004)	(0.006)
GDP Growth	-0.015**	-0.020***	-0.371	-0.002	-0.023***	-0.554**
	(0.008)	(0.007)	(0.227)	(0.004)	(0.004)	(0.280)
Inflation	0.249***	0.224***	5.121	0.341***	0.330***	21.389***
	(0.055)	(0.059)	(4.165)	(0.011)	(0.010)	(0.883)
Crisis	0.676***	0.752***	63.260*	0.864***	0.860***	61.137***
	(0.239)	(0.280)	(32.726)	(0.061)	(0.074)	(3.197)
Openness	0.029	-0.006	-23.133	-0.172***	-0.098***	-7.496***
	(0.116)	(0.117)	(21.939)	(0.031)	(0.026)	(1.161)
Credit-to-GDP	-0.004	0.003	1.224***	-0.010***	-0.010***	-0.265***
	(0.003)	(0.003)	(0.389)	(0.001)	(0.001)	(0.052)
Debt-to-GDP	-0.003	0.008**	0.299	0.000	0.002**	0.447***
	(0.003)	(0.004)	(0.494)	(0.001)	(0.001)	(0.038)
Intercept	1.762***	0.088	66.094	1.968***	0.679***	28.687***
	(0.472)	(0.447)	(58.448)	(0.127)	(0.097)	(5.126)
Observations	4,798	4,798	2,147	1,170	1,170	514
Adjusted R^2	0.867	0.822	0.245			
Countries	79	79	50	77	77	50
Instruments				68	67	49
AR2 test *p*-value				0.801	0.860	0.767
Hansen test *p*-value				0.140	0.433	0.226

Note: This table reports the estimates for the impact of an interaction term between prudential regulation (PR) and debt levels on the different proxies for sovereign risk. The variable Debt is a dummy that equals 1 when the country's debt level in the period is above the median debt level of the country during the sample period. Cols.1-3 present results from the within estimator, which controls for country fixed effects. Cols.4-6 present results from a two-step system GMM (Arellano and Bover, 1995). The estimates from GMM are based on the last quarter of each year to ensure that the number of instruments is smaller than the number of countries. The variable on PR is lagged four quarters to account for delayed effects on sovereign risk. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that an increase in sovereign risk is a consequence of past risk levels or country-specific risk perceptions. The AR2 row reports the *p*-value for the test of serial correlation in the residuals. Additionally, we provide the number of instruments in the two-step GMM and the *p*-value of the Hansen test of overidentification. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Table 4.6: Effect of Prudential Regulation on Sovereign Risk, Level of Economic Development

	Within Estimates			GMM Estimates		
	Bond Yield	Bond Spread	CDS	Bond Yield	Bond Spread	CDS
	(1)	(2)	(3)	(4)	(5)	(6)
PRxED	-0.062**	-0.077***	-0.841	-0.148***	-0.169***	-3.900***
	(0.029)	(0.026)	(1.794)	(0.019)	(0.019)	(0.745)
PR	-0.021	0.027	-1.757	0.028	0.098***	0.704
	(0.023)	(0.021)	(1.552)	(0.018)	(0.019)	(0.708)
LDV	0.696***	0.590***	0.280***	0.627***	0.558***	0.627***
	(0.028)	(0.025)	(0.036)	(0.004)	(0.005)	(0.005)
GDP Growth	-0.015*	-0.020***	-0.379	0.004	-0.015***	-0.143
	(0.008)	(0.007)	(0.233)	(0.004)	(0.005)	(0.620)
Inflation	0.251***	0.225***	5.633	0.326***	0.299***	21.911***
	(0.055)	(0.058)	(4.228)	(0.017)	(0.010)	(1.131)
Crisis	0.670***	0.744***	62.599*	0.963***	0.976***	61.973***
	(0.240)	(0.281)	(32.389)	(0.035)	(0.031)	(2.780)
Openness	0.023	-0.016	-26.263	-0.080***	-0.081***	-5.075***
	(0.117)	(0.116)	(21.507)	(0.019)	(0.023)	(1.568)
Credit-to-GDP	-0.003	0.003	1.228***	-0.010***	-0.010***	-0.201***
	(0.003)	(0.003)	(0.381)	(0.001)	(0.001)	(0.041)
Debt-to-GDP	-0.003	0.008*	0.117	0.002***	0.004***	0.436***
	(0.003)	(0.004)	(0.466)	(0.001)	(0.001)	(0.040)
Intercept	1.773***	0.012	44.815	2.353***	1.157***	40.774***
	(0.458)	(0.429)	(60.592)	(0.092)	(0.100)	(4.560)
Observations	4,798	4,798	2,147	1,170	1,170	514
Adjusted R^2	0.870	0.827	0.249			
Countries	79	79	50	77	77	50
Instruments				67	66	48
AR2 test *p*-value				0.852	0.732	0.732
Hansen test *p*-value				0.451	0.743	0.342

Note: This table reports the estimates for the impact of the interaction term between prudential regulation (PR) and economic development (ED) on the proxies of sovereign risk. Economic development is measured using a dummy variable that equals one whenever a country is classified as an upper-middle or high-income economy according to the World Bank. In the interest of space, we only report estimates for the most robust model specifications. Cols.1-3 present results from the within estimator, which controls for country fixed effects. Cols.4-6 present results from a two-step system GMM (Arellano and Bover, 1995). The estimates from GMM are based on the last quarter of each year to ensure that the number of instruments is smaller than the number of countries. The variable on PR is lagged four quarters to account for delayed effects on sovereign risk. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that an increase in sovereign risk is a consequence of past risk levels or country-specific risk perceptions. The AR2 row reports the *p*-value for the test of serial correlation in the residuals. Additionally, we provide the number of instruments in the two-step GMM and the *p*-value of the Hansen test of overidentification. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Table 4.7: IV Estimates of the Effect of Prudential Regulation on Sovereign Risk (Robustness)

	Bond Yield	Bond Spread	CDS
	(1)	(2)	(3)
		A. 2SLS with Fixed Effects	
PR	-0.806***	-0.360***	-13.530
	(0.249)	(0.109)	(8.951)
LDV	0.535***	0.560***	0.227***
	(0.060)	(0.020)	(0.048)
GDP Growth	-0.007	-0.014**	-0.635
	(0.009)	(0.007)	(0.487)
Inflation	0.144***	0.103***	4.445*
	(0.036)	(0.027)	(2.619)
Crisis	0.515***	0.623***	9.890
	(0.172)	(0.129)	(10.988)
Openness	0.102	-0.009	-18.324**
	(0.094)	(0.068)	(7.451)
Credit-to-GDP	0.012**	0.014***	2.351***
	(0.005)	(0.003)	(0.237)
Debt-to-GDP	0.003	0.009***	0.799***
	(0.003)	(0.002)	(0.249)
Intercept	2.268***	-0.495**	-71.270***
	(0.352)	(0.225)	(23.031)
		B. First-stage Estimates	
CBI	2.624***	4.293***	22.586***
	(0.624)	(-0.626)	(6.301)
LDV	-0.220***	-0.114***	-0.004***
	(0.015)	(0.019)	(0.001)
GDP Growth	0.004	0.007	0.004
	(0.008)	(0.019)	(0.017)
Inflation	-0.011	-0.042	-0.160***
	(0.034)	(0.034)	(0.076)
Crisis	-0.225	-0.435***	0.061
	(0.157)	(0.160)	(0.375)
Openness	0.059	-0.057	0.292
	(0.087)	(0.089)	(0.235)
Credit-to-GDP	0.017***	0.022***	0.0188***
	(0.002)	(0.002)	(0.006)
Debt-to-GDP	0.007***	0.009***	0.009***
	(0.002)	(0.002)	(0.008)
Intercept	-0.590	-3.190***	-13.996***
	(0.432)	(0.396)	(4.486)
F-*statistic*	62.26	39.09	6.82
Observations	4,556	4,377	4,572
Adjusted R^2	0.538	0.960	0.960

Note: This table reports the IV estimates of the effects of prudential regulation (PR) on sovereign risk. Panel A presents 2SLS with country fixed effects instrumenting PR with central bank independence (CBI). Panel B presents the first-stage estimates and the excluded instruments F-*statistic*. The variable on PR is lagged four quarters to account for delayed effects on sovereign risk. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that an increase in sovereign risk is a consequence of past risk levels or country-specific risk perceptions. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Table 4.8: IV Estimates of the Effect of Prudential Regulation on Government Spending (Robustness)

	Primary Balance (1)	Gov. Expenditure (2)	Debt-to-GDP (3)
		A. 2SLS with Fixed Effects	
PR	-0.239***	0.126**	0.387**
	(0.065)	(0.057)	(0.170)
LDV	0.575***	0.829***	0.931***
	(0.014)	(0.008)	(0.007)
GDP Growth	0.004	-0.007	-0.011
	(0.007)	(0.005)	(0.018)
Inflation	0.025	-0.059***	-0.177***
	(0.019)	(0.015)	(0.051)
Crisis	-1.237***	0.493***	1.764***
	(0.193)	(0.145)	(0.506)
Openness	-0.424***	0.070	0.421**
	(0.075)	(0.055)	(0.197)
Credit-to-GDP	-0.007***	0.007***	0.036***
	(0.003)	(0.002)	(0.007)
Debt-to-GDP	0.008***	-0.015***	
	(0.002)	(0.002)	
Primary Balance			
			(0.037)
Intercept	1.540***	5.790***	0.804
	(0.248)	(0.277)	(0.657)
		B. First-stage Estimates	
Forward Guidance	2.219***	2.336***	2.219***
	(0.270)	(0.152)	(0.173)
LDV	0.042***	0.014	0.013***
	(0.016)	(0.009)	(0.003)
GDP Growth	0.007	0.002	0.007
	(0.008)	(0.006)	(0.008)
Inflation	0.148***	0.119***	0.148***
	(0.021)	(0.015)	(0.021)
Crisis	-0.850***	-0.462***	-0.854***
	(0.218)	(0.160)	(0.212)
Openness	0.349***	-0.120***	0.346***
	(0.087)	(0.063)	(0.085)
Credit-to-GDP	0.028***	0.027***	0.029***
	(0.002)	(0.002)	(0.002)
Debt-to-GDP	0.013***	0.002	
	(0.003)	(0.002)	
Primary Balance			0.041***
			(0.016)
Intercept	-1.651***	-0.472**	-1.669***
	(0.270)	(0.317)	(0.300)
F-*statistic*	58.09	68.47	58.79
Observations	4,556	4,377	4,572
Adjusted R^2	0.538	0.960	0.960

Note: This table reports the IV estimates of the effects of prudential regulation (PR) on government spending. Panel A presents 2SLS with country fixed effects instrumenting PR with forward guidance. Panel B presents the first-stage estimates and the excluded instruments F-statistic. All regressors are lagged four quarters to account for delayed effects on government spending. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that an increase in government spending is a consequence of recent changes in fiscal policy or country-specific factors that affect government spending. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Table 4.9: Potential Explanations for the Effect of Prudential Regulation on Sovereign Risk

	Within Estimates			GMM Estimates		
	Public-Private Credit	Sovereign Credit Ratings	Gov. Bonds Net Issues	Public-Private Credit	Sovereign Credit Ratings	Gov. Bonds Net Issues
	(1)	(2)	(3)	(4)	(5)	(6)
PR	-0.001**	0.014**	0.199	-0.002***	0.003***	0.135***
	(0.000)	(0.007)	(0.159)	(0.000)	(0.001)	(0.020)
LDV	0.822***	0.829***	0.076*	0.802***	1.014***	0.028***
	(0.015)	(0.019)	(0.044)	(0.011)	(0.003)	(0.001)
GDP Growth	0.000	0.004***	0.039	-0.003***	0.001	0.069***
	(0.000)	(0.001)	(0.051)	(0.001)	(0.001)	(0.013)
Inflation	-0.002	-0.084**	-0.314	0.001	-0.083***	0.317***
	(0.001)	(0.032)	(0.197)	(0.001)	(0.006)	(0.051)
Crisis	-0.021**	-0.279*	1.910	-0.009***	-0.147***	0.960***
	(0.009)	(0.145)	(1.672)	(0.003)	(0.011)	(0.143)
Debt-to-GDP	0.001***	-0.013***	-0.005	0.001***	-0.002***	-0.001
	(0.000)	(0.003)	(0.012)	(0.000)	(0.000)	(0.002)
Primary Balance	-0.003***	0.039***	0.104	-0.004***	0.029***	-0.038***
	(0.001)	(0.010)	(0.101)	(0.001)	(0.002)	(0.010)
Reserves	0.013	0.190	0.421	0.006	-0.015	1.428
	(0.019)	(0.411)	(2.708)	(0.009)	(0.074)	(0.905)
Intercept	0.010	3.505***	-1.880*	0.032***	-0.162**	-1.609***
	(0.014)	(0.423)	(1.080)	(0.004)	(0.072)	(0.089)
Observations	2,668	4,513	3,778	669	1,100	946
Adjusted R^2	0.981	0.967	0.01			
Countries	32	58	47	32	58	45
Instruments				67	65	67
AR2 test p-value				0.429	0.116	0.131
Hansen test p-value				1.000	0.861	1.000

Note: This table reports estimates of the effects of prudential regulation (PR) on potential transmission channels into sovereign risk and government spending. Col. 1 compares credit given to the government to credit to the private sector. Col. 3 looks at the average sovereign credit rating. Col. 3 examines the growth rate in the net issue of government bonds. Cols.4-6 repeats the analysis using the two-step system GMM (Arellano and Bover, 1995). The estimates from GMM are based on the last quarter of each year to ensure that the number of instruments is smaller than the number of countries. The AR2 row reports the p-value for the test of serial correlation in the residuals. Additionally, we provide the number of instruments and the p-value of the Hansen test of overidentification. The variable on PR is lagged four quarters to account for delayed effects. We include a lagged dependent variable (LDV) that lags four quarters behind the current period to mitigate the possibility that some channels may be more important than others in some countries. Standard errors reported in parentheses are clustered at the country level. "***", "**" and "*" represents statistical significance at 1%, 5% and 10% levels.

Concluding Remarks

This thesis began by showing that MaPP reduces household consumption and increases firm investment, particularly in the long run (Chapter 1). This happens because people save more and borrow less after the adoption of MaPP (Chapter 2). This, in turn, leads to an ever-growing share of wealth going to the top of the distribution, which hurts the relatively poor (Chapter 3). Paradoxically, prudential regulation prevents excessive leverage in the banking sector, but can also increase debt in the public sector. The reason for this is that prudential regulation improves the nation's credit rating and its borrowing conditions in sovereign bond markets (Chapter 4). Taken together, these findings support the contention that MaPP should be used sparingly and adjusted in much the same way as the interest rate.

Having said this, I do not wish to convey the impression that central banks should not adopt MaPP. Nor do I wish to imply that MaPP imposes the same kind of restrictions on aggregate demand as monetary policy does. The point I wish to make here is that the effects of macroprudential and monetary policy are almost certainly different, and this is precisely why we must choose the mix best suited to the prescribed end. Just as generals are said to be always fighting the wrong war, central banks have been accused of fighting with the wrong tools. The problem, I suspect, is not so much political as it is conceptual. A number of practical issues remain puzzling. How can prudential rules be designed to prevent a crisis without hammering the economy? Which prudential rules are better suited to a particular economy? And, most importantly, how should these rules interact with the interest rate? I have tried to treat these issues somewhat exhaustively in this thesis by using a combination of rich data and causal methods.

The essential question of this thesis boils down to this: can central banks foster financial stability without compromising growth? The answer is no. However, they can ensure that prudential rules have a limited impact on growth. The evidence presented here suggests that the effects of MaPP depend crucially on the design of the prudential rule. I find that prudential rules based on income have stronger effects on aggregate demand and, ultimately, on the redistribution of wealth. If central banks are concerned about the redistributive effects of MaPP, as they should be, they must implement prudential rules based on collateral. That said, I show that MaPP can be an effective tool to reduce sovereign risk and enable governments to increase spending, particularly in countries with relatively low levels of government debt and economic development.

And this brings me to my last point. I have shown that the effects of MaPP vary with the country's income level. In general, I find that MaPP has stronger effects on borrowing in advanced economies and on savings in emerging economies. I also find that the effects of MaPP on sovereign risk are stronger in emerging economies. Obviously, these generalizations may not always stand up under careful examination. A better rule of thumb is that MaPP should only be used when people save too little or are insufficiently prudent and parsimonious.

In closing, I would like to point out that MaPP is found to be a more effective tool to curb credit than the interest rate. The results presented in this thesis show that increasing the interest rate when MaPP is already too tight has only negligible effects on the economy. Or, to put the matter most crudely, MaPP may be as important as monetary policy in steering the economy. What is sure and inescapable is that, one way or another, MaPP will foster financial stability at the cost of lower growth, higher inequality, and more government debt.

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Supplementary Materials for Chapter 1

A.1. Data Description

Variable	Type	Source	Details
Dependency Ratio	Control	Eurostat	Ratio between the number of persons aged 65 and over (age when they are generally economically inactive) and the number of persons aged between 15 and 64. This indicator is published annually, and it was assumed constant for all quarters within the year.
DSTI ratio	Policy	IMF iMaPP	Limits to the debt-service-to-income ratio and the loan-to-income ratio, which restrict the size of debt services or debt relative to income. They include those targeted at housing loans, consumer loans, and commercial real estate loans. Index cumulated to a quarterly frequency.
Firm Investment Rate	Dependent	Eurostat	Gross fixed capital formation (P51) divided by gross value added (B1G) of NFC. Seasonally and calendar adjusted. Quarterly data.
GDP per capita	Control	Eurostat	Gross domestic product at market prices. Million euros. Seasonally and calendar adjusted. Divided by total population. Total population is published annually, and it was assumed constant for all quarters within the year. Quarterly data.
HH Consumption to GDP	Dependent	Eurostat	Private consumption expenditure consists of expenditure incurred for the direct satisfaction of individual or collective needs by private households or non-profit institutions serving households. Seasonally and calendar adjusted. Quarterly data.
HH Loan Restrictions	Policy	IMF iMaPP	Household loan restrictions include mainly loan limits and may be conditioned on loan characteristics like the maturity, the size, the type of interest rate and the LTV ratio. Index cumulated to a quarterly frequency.
Household (HH) Savings Rate	Dependent	Eurostat	Gross saving (B8G) divided by gross disposable income adjusted for changes in pension entitlements (B6G + D8net); Seasonally and calendar adjusted. Quarterly data.
LTV ratio	Policy	IMF iMaPP	Limits to the loan-to-value ratios, including those mostly targeted at housing loans, but also those targeted at automobile loans, and commercial real estate loans. Index cumulated to a quarterly frequency.
NFC GFCF to GDP	Dependent	Eurostat	GFCF consists of resident producers' acquisitions, less disposals of fixed assets plus certain additions to the value of non-produced assets realised by productive activity, such as improvements to land. Seasonally and calendar adjusted. Quarterly data.
NFC Loan Restrictions	Policy	IMF iMaPP	Firm loan restrictions include mainly loan limits and may be conditioned on loan characteristics like the maturity, the size, the type of interest rate and the LTV ratio. Index cumulated to a quarterly frequency.

A.2. Unit Root Tests

Maddala and Wu (1999) test			
	Obs.	**Statistic**	**P-Value**
HH Savings Rate	1200	193.751	0.000
HH Consumption to GDP	1600	129.887	0.000
Firm Investment Rate	1200	236.652	0.000
NFC GFCF to GDP	1280	212.983	0.000
Dependency Ratio	1600	26.644	0.948
GDP per capita	1600	9.896	0.000

Pesaran (2007) test			
	Obs.	**Statistic**	**P-Value**
HH Savings Rate	1200	-6.821	0.000
HH Consumption to GDP	1600	-7.454	0.000
Firm Investment Rate	1200	-8.984	0.000
NFC GFCF to GDP	1280	-8.542	0.000
Dependency Ratio	1600	-0.502	0.308
GDP per capita	1600	-2.335	0.000

Note: The table presents the First generation Maddala and Wu (1999) test and second generation Pesaran (2007) test for panel unit roots results based on H_0: All panels contain unit roots and H_a: At least one panel is stationary. The results of an Inverse Chi-squared test are presented above with both the test statistic and the p-value being displayed. The presence of a unit root is always rejected because the p-value is less than 0.1 except for the case of the dependency ratio.

A.3. Cramér-von-Mises Tests

	HH Savings Rate		
Covariates	**No Covariates**	**Dependency Ratio**	**GDP per capita**
CvM Test Statistic	0.0673	0.0451	0.0560
CvM Critical Value	0.3253	0.3501	0.4912
CvM P-Value	0.8400	0.9760	0.9210

	Firm Investment Rate		
Covariates	**No Covariates**	**Dependency Ratio**	**GDP per capita**
CvM Test Statistic	0.1372	0.1397	0.1586
CvM Critical Value	0.4527	0.7462	1.0857
CvM P-Value	0.8190	0.9640	0.9460

Note: The tables present the CvM test for the presence of (un)conditional parallel pre-trends based on H_0: (Un)conditional parallel pre-trends hold and H_a: (Un)conditional parallel pre-trends do not hold. The results of the Wald-type test are presented in the above table with both the test statistic and the p-value being displayed. Note that we always fail to reject the presence of parallel trends as the p-value is greater than 0.10.

A.4. Control and Treated Groups

Variable	Treated Group	"Never" Treated Control Group
HH Savings Rate	Czech Republic	Austria
	Denmark	Belgium
	Finland	France
	Ireland	Germany
	Netherlands	Italy
	Poland	
	Portugal	
	Sweden	
	United Kingdom	
HH Consumption to GDP	Croatia	Austria
	Czech Republic	Belgium
	Denmark	France
	Estonia	Germany
	Finland	Italy
	Hungary	
	Latvia	
	Netherlands	
	Poland	
	Portugal	
	Slovakia	
	Slovenia	
	Sweden	
	United Kingdom	
Firm Investment Rate	Czech Republic	Austria
	Denmark	Belgium
	Estonia	Germany
	Finland	Italy
	France	United Kingdom
	Netherlands	
	Poland	
	Portugal	
	Sweden	
NFC GFCF to GDP	Czech Republic	Austria
	Denmark	Belgium
	Estonia	Germany
	Finland	Italy
	France	United Kingdom
	Hungary	
	Netherlands	
	Poland	
	Portugal	
	Sweden	

Note: List of countries in the control and treated groups for the DiD estimations on household consumption and firm investment. A country is assigned to the treatment group if it implements MaPP at some point in time in the sample period. A country is assigned to the "never treated" control group if it "never" implements MaPP in the sample period. An important point to note is that the control group in our main models will also include countries that have "not yet" implemented MaPP at the time of implementation of MaPP for every group g.

A.5. MaPP Adoption

Country	Date of implementation	Policy Implemented
Croatia	2006-Q4	LTV
Czech Republic	2015-Q2	LTV, Household Loan Restrictions
Denmark	2003-Q2	Household Loan Restrictions, NFC Loan Restrictions
Estonia	2015-Q1	LTV, Household Loan Restrictions, DSTI
Finland	2010-Q1	LTV
France	2018-Q3	NFC Loan Restrictions
Hungary	2010-Q1	LTV, Household Loan Restrictions, DSTI
Ireland	2001-Q4	LTV
Latvia	2007-Q1	NFC Loan Restrictions
Netherlands	2007-Q1	DSTI
Poland	2006-Q4	Household Loan Restrictions
Portugal	2018-Q3	LTV, Household Loan Restrictions, DSTI
Slovakia	2014-Q4	LTV
Slovenia	2016-Q3	LTV, DSTI
Sweden	2004-Q3	LTV
United Kingdom	2009-Q1	Household Loan Restrictions

Note: Date of first implementation of MaPP for every country in our sample and brief description of the policy.

Supplementary Materials for Chapter 3

B.1. Sample Details

Country	Treatment Status
Afghanistan	Missing Data
Albania	Control Unit
Algeria	Treated since 2007
Angola	Control Unit
Argentina	Missing Data
Armenia	Control Unit
Australia	Missing Data
Austria	Control Unit
Azerbaijan	Missing Data
Bahamas	Missing Data
Bahrain	Treated since 2012
Bangladesh	Missing Data
Belarus	Control Unit
Belgium	Control Unit
Belize	Missing Data
Benin	Control Unit
Bhutan	Treated since 2014
Bolivia	Control Unit
Bosnia and Herzegovina	Missing Data
Botswana	Control Unit
Brazil	Treated since 2013
Bulgaria	Control Unit
Burkina Faso	Control Unit
Burundi	Control Unit
Cabo Verde	Control Unit
Cambodia	Control Unit
Cameroon	Control Unit

Country	Treatment Status
Canada	Treated since 2012
Central African Republic	Control Unit
Chad	Control Unit
Chile	Control Unit
China	Missing Data
Colombia	Missing Data
Comoros	Missing Data
Congo	Missing Data
Costa Rica	Treated since 2005
Cote d'Ivoire	Control Unit
Croatia	Treated since 2006
Cuba	Missing Data
Cyprus	Treated since 2003
Czech Republic	Treated since 2015
Denmark	Missing Data
Djibouti	Missing Data
Dominican Republic	Control Unit
DR Congo	Missing Data
Ecuador	Missing Data
Egypt	Missing Data
El Salvador	Control Unit
Equatorial Guinea	Control Unit
Eritrea	Missing Data
Estonia	Treated since 2015
Ethiopia	Control Unit
Finland	Treated since 2010
France	Control Unit
Gabon	Missing Data
Gambia	Missing Data
Georgia	Control Unit
Germany	Control Unit
Ghana	Control Unit
Greece	Treated since 2005
Guatemala	Control Unit
Guinea	Control Unit
Guinea-Bissau	Control Unit
Guyana	Control Unit
Haiti	Missing Data
Honduras	Control Unit
Hungary	Treated since 2010
Iceland	Control Unit
India	Treated since 2010
Indonesia	Treated since 2012
Iran	Missing Data
Iraq	Control Unit

Country	Treatment Status
Ireland	Missing Data
Israel	Treated since 2012
Italy	Control Unit
Jamaica	Control Unit
Japan	Missing Data
Jordan	Treated since 2008
Kazakhstan	Treated since 2013
Kenya	Control Unit
Korea	Missing Data
Kuwait	Missing Data
Kyrgyzstan	Missing Data
Lao PDR	Control Unit
Latvia	Treated since 2007
Lebanon	Missing Data
Lesotho	Control Unit
Liberia	Missing Data
Libya	Missing Data
Lithuania	Treated since 2011
Luxembourg	Missing Data
Macao	Missing Data
Madagascar	Control Unit
Malawi	Control Unit
Maldives	Control Unit
Mali	Control Unit
Malta	Control Unit
Mauritania	Control Unit
Mauritius	Treated since 2014
Mexico	Control Unit
Moldova	Missing Data
Mongolia	Treated since 2008
Montenegro	Missing Data
Morocco	Control Unit
Mozambique	Control Unit
Myanmar	Control Unit
Namibia	Missing Data
Nepal	Treated since 2009
Netherlands	Treated since 2007
New Zealand	Treated since 2013
Nicaragua	Control Unit
Niger	Control Unit
Nigeria	Control Unit
North Macedonia	Control Unit
Norway	Treated since 2010
Oman	Missing Data
Pakistan	Missing Data

Country	**Treatment Status**
Palestine	Missing Data
Panama	Control Unit
Papua New Guinea	Control Unit
Paraguay	Control Unit
Peru	Control Unit
Philippines	Control Unit
Poland	Treated since 2010
Portugal	Missing Data
Qatar	Control Unit
Romania	Treated since 2004
Russian Federation	Control Unit
Rwanda	Control Unit
Sao Tome and Principe	Control Unit
Saudi Arabia	Treated since 2014
Senegal	Control Unit
Serbia	Missing Data
Seychelles	Control Unit
Sierra Leone	Control Unit
Singapore	Missing Data
Slovakia	Missing Data
Slovenia	Control Unit
Somalia	Missing Data
South Africa	Control Unit
South Sudan	Control Unit
Spain	Control Unit
Sri Lanka	Treated since 2015
Sudan	Control Unit
Suriname	Control Unit
Swaziland	Missing Data
Sweden	Treated since 2004
Switzerland	Control Unit
Syrian Arab Republic	Control Unit
Taiwan	Missing Data
Tajikistan	Control Unit
Tanzania	Missing Data
Thailand	Missing Data
Timor-Leste	Control Unit
Togo	Control Unit
Trinidad and Tobago	Control Unit
Tunisia	Control Unit
Turkey	Treated since 2011
Turkmenistan	Missing Data
Uganda	Missing Data
Ukraine	Control Unit
United Arab Emirates	Treated since 2011

Country	Treatment Status
United Kingdom	Treated since 2014
Uruguay	Control Unit
USA	Missing Data
Uzbekistan	Missing Data
Venezuela	Missing Data
Vietnam	Missing Data
Yemen	Missing Data
Zambia	Missing Data
Zimbabwe	Missing Data

Note: The table reports the treatment status of each country in the sample. "Treated countries" implement either LTV or DSTI ratios during the sample period. "Control unit" is a country that does not implement MaPP during the sample period. "Missing data" refers to countries that have at least one factor with less than 7 observations in the pre-treatment period or data missing in the post-treatment period.

B.2. List of Synthetic Controls

This appendix reports the implicit weights of every control unit for each synthetic control in the baseline analysis. A few things to notice. First, the weights take into account the similarity of the factors but also of the outcome of interest. This means that the weights vary with the wealth outcome. Second, contrary to standard synthetic controls, the loadings (or “weights”) can be positive or negative and they are not restricted to be in [0,1]. The list of country codes is presented after the figures.

Synthetic Controls, Gini Index, Wealth

UNTREATED COUNTRIES (ROWS) / TREATED COUNTRIES (COLUMNS)	AE	BH	BR	BT	CA	CR	CY	CZ	DZ	FI	GB	GR	HR	HU	ID	IL	IN	JO	KZ	LK	LT	LV	MN	MU	NL	NO	NP	NZ	PL	RO	SA	SE	TR
AL	-0,04	-0,04	0,07	0,03	-0,02	0,00	0,01	0,19	0,01	0,09	0,15	0,01	0,03	0,01	0,08	-0,04	0,02	-0,01	-0,05	0,04	0,03	0,01	0,01	-0,04	-0,08	0,03	0,00	0,07	0,02	0,03	0,09	0,09	-0,03
AM	0,04	0,04	-0,06	-0,01	0,02	0,01	-0,04	-0,17	0,00	-0,05	-0,12	-0,02	-0,01	-0,01	-0,04	0,02	0,04	0,02	0,04	0,04	0,00	0,01	0,00	0,08	0,05	-0,09	0,01	-0,06	-0,01	-0,02	-0,10	-0,05	-0,02
AO	-0,34	-0,21	0,46	0,33	-0,08	0,02	-0,10	0,92	0,08	0,36	0,97	0,06	0,14	0,01	0,45	-0,35	0,27	-0,07	-0,33	0,52	0,15	0,08	0,02	-0,17	-0,41	-0,05	0,05	0,40	0,04	0,05	0,45	0,46	0,36
AT	0,09	0,05	-0,02	0,01	0,04	0,02	-0,03	-0,29	-0,01	-0,03	-0,28	-0,03	-0,02	0,01	-0,16	0,20	-0,08	0,03	0,05	-0,02	-0,04	0,00	0,00	0,07	0,07	0,01	-0,01	-0,11	-0,01	-0,01	-0,11	-0,08	0,06
BE	-0,06	-0,08	0,13	-0,02	-0,05	-0,03	0,12	0,37	0,00	0,09	0,24	0,03	0,02	0,02	0,08	-0,04	-0,06	-0,04	-0,07	-0,16	0,00	-0,02	0,00	-0,15	-0,09	0,20	-0,02	0,12	0,03	0,05	0,20	0,10	-0,02
BF	0,27	0,52	-0,99	-0,14	0,22	0,02	-0,43	-2,21	-0,11	-0,89	-1,50	-0,14	-0,24	-0,13	-0,80	-0,02	-0,18	0,16	0,53	0,34	-0,22	-0,04	-0,14	0,55	0,84	-0,70	0,06	-0,82	-0,27	-0,48	-1,09	-0,87	0,67
BG	-0,11	-0,09	0,12	0,05	-0,04	-0,01	0,03	0,40	0,02	0,19	0,31	0,04	0,05	0,02	0,16	-0,11	-0,03	-0,04	-0,11	0,08	0,05	0,01	0,01	-0,14	-0,17	0,10	0,00	0,14	0,04	0,05	0,20	0,17	-0,06
BI	-0,09	-0,05	0,09	0,05	-0,03	-0,01	0,00	0,25	0,01	0,06	0,26	0,02	0,03	0,00	0,12	-0,12	0,06	-0,02	-0,07	0,07	0,03	0,01	0,00	-0,06	-0,08	0,01	0,01	0,10	0,00	0,01	0,12	0,10	0,08
BJ	-0,42	-0,27	0,20	-0,11	-0,22	-0,16	0,34	1,50	-0,02	-0,02	1,36	0,17	0,07	-0,03	0,63	-0,88	0,25	-0,19	-0,21	-0,39	0,08	-0,08	-0,04	-0,47	-0,21	0,31	0,00	0,54	0,00	0,02	0,68	0,30	0,01
BO	0,28	0,22	-0,30	-0,06	0,14	0,06	-0,15	-1,07	-0,03	-0,15	-0,97	-0,10	-0,09	0,00	-0,46	0,45	-0,23	0,11	0,22	0,06	-0,09	0,01	0,00	0,27	0,27	-0,16	-0,01	-0,41	-0,03	-0,07	-0,50	-0,32	-0,04
BW	-0,01	-0,02	0,08	0,03	0,00	0,01	0,00	0,08	0,01	0,08	0,03	0,00	0,02	0,01	0,01	0,06	-0,03	0,00	-0,04	0,02	0,01	0,01	0,01	-0,03	-0,06	0,05	0,00	0,03	0,02	0,03	0,05	0,06	0,00
BY	0,01	0,00	-0,03	-0,03	0,00	0,00	0,02	0,01	0,00	0,00	-0,01	0,00	0,00	0,00	0,01	0,00	0,00	0,00	0,01	-0,04	0,00	0,00	0,00	0,00	0,00	0,01	0,00	0,00	0,01	0,01	0,00	-0,01	-0,07
CF	-0,02	-0,23	0,14	-0,42	-0,20	-0,15	0,62	1,22	-0,04	-0,22	0,81	0,11	-0,02	-0,02	0,47	-0,32	0,38	-0,14	0,02	-1,23	0,00	-0,14	0,02	-0,28	-0,01	0,45	-0,08	0,42	0,07	0,19	0,49	0,07	-0,78
CH	-0,15	-0,13	0,13	-0,04	-0,09	-0,06	0,16	0,66	0,00	0,05	0,55	0,07	0,04	0,00	0,26	-0,28	0,09	-0,08	-0,10	-0,18	0,04	-0,03	0,00	-0,20	-0,12	0,16	-0,01	0,24	0,02	0,05	0,30	0,15	-0,07
CI	0,10	0,11	0,07	0,31	0,15	0,14	-0,39	-0,76	0,05	0,33	-0,64	-0,10	0,04	0,05	-0,33	0,51	-0,28	0,11	-0,05	0,77	0,02	0,12	0,03	0,21	-0,11	-0,21	0,03	-0,26	0,02	-0,01	-0,29	0,05	0,30
CL	0,02	0,00	0,04	0,02	0,01	0,01	-0,01	-0,03	0,01	-0,01	-0,02	-0,01	0,00	0,00	-0,01	0,06	0,05	0,01	0,00	0,00	0,00	0,01	0,00	0,04	0,00	-0,02	0,00	0,00	0,00	0,01	-0,02	0,00	0,03
CM	-0,14	-0,04	-0,05	-0,01	-0,04	-0,04	0,01	0,27	-0,01	-0,05	0,34	0,04	0,02	-0,02	0,17	-0,35	0,08	-0,04	-0,04	0,06	0,03	-0,01	-0,02	-0,08	-0,02	-0,04	0,02	0,10	-0,03	-0,05	0,11	0,05	0,08
CV	0,51	0,55	-0,72	0,12	0,36	0,19	-0,68	-2,73	-0,03	-0,33	-2,15	-0,25	-0,17	-0,03	-1,04	0,83	-0,40	0,29	0,44	0,87	-0,16	0,10	-0,03	0,77	0,58	-0,76	0,06	-0,99	-0,14	-0,29	-1,27	-0,69	0,43
DE	0,00	-0,04	0,05	-0,05	-0,03	-0,02	0,10	0,21	0,00	0,06	0,09	0,02	0,01	0,02	0,05	0,02	-0,04	-0,02	-0,02	-0,17	0,00	-0,02	0,01	-0,08	-0,05	0,14	-0,02	0,06	0,03	0,05	0,10	0,05	-0,15
DO	0,28	0,23	-0,29	-0,02	0,14	0,07	-0,19	-1,13	-0,02	-0,16	-0,99	-0,10	-0,09	0,00	-0,48	0,46	-0,21	0,12	0,21	0,13	-0,09	0,02	0,00	0,30	0,27	-0,20	0,00	-0,42	-0,04	-0,08	-0,52	-0,33	0,04
EE	0,06	0,05	-0,02	0,06	0,05	0,04	-0,11	-0,29	0,01	0,02	-0,22	-0,03	0,00	0,00	-0,09	0,14	0,00	0,04	0,02	0,17	0,00	0,03	0,01	0,11	0,02	-0,12	0,01	-0,09	0,00	-0,01	-0,14	-0,03	0,05
ES	-0,22	-0,01	-0,26	-0,14	-0,09	-0,10	0,08	0,34	-0,05	-0,37	0,53	0,08	-0,03	-0,08	0,25	-0,75	0,23	-0,08	0,05	-0,13	0,01	-0,06	-0,07	-0,11	0,16	-0,12	0,03	0,12	-0,10	-0,16	0,09	-0,09	0,16
ET	-0,04	-0,05	0,07	0,02	-0,02	0,00	0,01	0,20	0,02	0,06	0,18	0,01	0,02	0,00	0,11	-0,05	0,08	-0,01	-0,05	0,02	0,04	0,01	0,01	-0,02	-0,07	0,00	0,00	0,08	0,02	0,03	0,08	0,08	-0,06
FR	-0,09	-0,04	0,02	-0,04	-0,05	-0,05	0,09	0,26	-0,02	-0,11	0,27	0,04	-0,01	-0,02	0,09	-0,22	0,05	-0,04	-0,02	-0,16	-0,02	-0,03	-0,03	-0,10	0,03	0,08	0,00	0,10	-0,03	-0,04	0,13	0,00	0,14
GE	-0,05	-0,03	-0,01	0,03	0,00	0,02	-0,05	0,12	0,02	0,06	0,17	0,01	0,03	0,00	0,15	-0,13	0,14	0,00	-0,03	0,17	0,07	0,03	0,02	0,04	-0,07	-0,14	0,02	0,06	0,01	0,02	0,02	0,08	-0,12
GH	-0,04	-0,03	0,05	0,06	0,00	0,02	-0,03	0,12	0,02	0,19	0,07	0,01	0,04	0,03	0,05	0,01	-0,10	-0,01	-0,07	0,17	0,04	0,02	0,02	-0,06	-0,12	0,04	0,01	0,03	0,03	0,04	0,07	0,11	-0,06
GN	-0,04	0,05	-0,03	0,08	0,03	0,00	-0,10	-0,23	-0,02	-0,01	-0,14	-0,01	-0,01	0,00	-0,16	0,01	-0,19	0,01	-0,01	0,18	-0,05	0,00	-0,04	-0,03	0,04	0,01	0,01	-0,09	-0,05	-0,09	-0,05	-0,05	0,39
GQ	-0,01	-0,01	-0,04	-0,02	0,00	0,01	0,00	0,08	0,01	0,07	0,05	0,01	0,02	0,01	0,07	-0,05	0,00	0,00	-0,01	0,06	0,04	0,01	0,01	-0,01	-0,05	-0,02	0,01	0,02	0,02	0,03	0,02	0,04	-0,18
GT	0,25	0,20	-0,29	-0,06	0,12	0,05	-0,14	-0,99	-0,03	-0,16	-0,89	-0,09	-0,08	0,00	-0,43	0,40	-0,22	0,10	0,21	0,05	-0,09	0,00	-0,01	0,24	0,26	-0,14	-0,01	-0,38	-0,04	-0,07	-0,46	-0,31	0,00
GW	-0,26	-0,68	1,13	-0,32	-0,44	-0,22	1,11	3,19	0,03	0,18	2,24	0,24	0,12	0,04	1,09	-0,34	0,74	-0,30	-0,40	-1,94	0,09	-0,17	0,10	-0,73	-0,54	1,12	-0,16	1,20	0,22	0,52	1,48	0,68	-0,91
GY	0,22	0,14	-0,28	-0,12	0,09	0,03	-0,05	-0,69	-0,03	-0,03	-0,72	-0,06	-0,05	0,02	-0,31	0,32	-0,27	0,07	0,16	-0,02	-0,05	-0,01	0,01	0,13	0,15	-0,03	-0,02	-0,30	0,01	0,00	-0,32	-0,21	-0,28
HN	0,21	0,14	-0,21	-0,12	0,06	0,01	-0,02	-0,64	-0,03	-0,36	-0,50	-0,06	-0,10	-0,04	-0,22	0,18	0,15	0,07	0,21	-0,22	-0,08	-0,02	-0,01	0,27	0,30	-0,20	-0,01	-0,21	-0,06	-0,07	-0,35	-0,31	-0,03
IQ	0,01	0,00	0,03	0,03	0,01	0,01	-0,03	-0,01	0,01	0,03	0,00	-0,01	0,01	0,00	0,01	0,04	0,04	0,01	-0,01	0,06	0,01	0,01	0,01	0,03	-0,02	-0,04	0,00	0,01	0,01	0,01	-0,01	0,02	-0,01
IS	-0,03	-0,04	0,00	-0,06	-0,03	-0,03	0,09	0,22	-0,01	0,07	0,11	0,03	0,01	0,02	0,06	-0,06	-0,10	-0,03	-0,02	-0,12	0,01	-0,02	0,00	-0,12	-0,05	0,15	-0,01	0,05	0,03	0,04	0,12	0,05	-0,15
IT	-0,07	-0,21	0,61	0,30	-0,04	0,09	-0,05	0,72	0,12	0,42	0,61	0,00	0,13	0,04	0,38	0,21	0,46	0,01	-0,27	0,27	0,16	0,11	0,10	0,07	-0,42	-0,08	0,01	0,36	0,12	0,22	0,32	0,44	-0,10
JM	0,33	0,28	-0,53	-0,16	0,15	0,06	-0,16	-1,27	-0,04	-0,26	-1,12	-0,10	-0,11	-0,01	-0,47	0,35	-0,21	0,12	0,32	0,06	-0,09	0,00	-0,01	0,34	0,37	-0,28	0,00	-0,50	-0,05	-0,10	-0,64	-0,43	-0,25
KE	-0,03	-0,04	0,11	0,09	0,02	0,03	-0,02	0,13	0,02	0,48	-0,11	0,01	0,07	0,09	-0,08	0,23	-0,50	-0,01	-0,14	0,24	0,02	0,03	0,03	-0,22	-0,26	0,30	-0,01	-0,02	0,09	0,10	0,16	0,21	-0,11
KH	0,00	-0,01	0,05	0,03	0,00	0,01	-0,01	0,01	0,01	-0,01	0,03	-0,01	0,00	0,00	0,01	0,03	0,06	0,00	-0,01	0,00	0,00	0,01	0,00	0,03	-0,01	-0,02	0,00	0,02	0,00	0,00	0,01	0,01	0,04
LA	-0,03	-0,06	0,12	0,00	-0,03	-0,01	0,07	0,27	0,01	0,07	0,18	0,02	0,02	0,01	0,09	0,00	0,03	-0,02	-0,05	-0,09	0,02	0,00	0,01	-0,07	-0,08	0,09	-0,01	0,10	0,03	0,05	0,13	0,09	-0,07
LS	0,65	0,42	-0,37	-0,06	0,25	0,13	-0,29	-2,17	-0,04	-0,68	-1,80	-0,23	-0,23	-0,07	-0,88	1,01	0,12	0,25	0,49	-0,16	-0,23	0,01	0,00	0,81	0,69	-0,53	-0,02	-0,73	-0,13	-0,16	-1,06	-0,75	0,22
MA	0,07	0,01	0,02	-0,01	0,02	0,02	0,01	-0,07	0,01	0,05	-0,13	-0,01	0,00	0,01	-0,04	0,14	-0,03	0,02	0,01	-0,04	0,00	0,01	0,02	0,03	-0,01	0,02	-0,01	-0,03	0,02	0,04	-0,03	0,00	-0,12
MG	0,02	-0,03	0,10	-0,03	-0,02	-0,02	0,10	0,13	-0,01	-0,02	0,04	0,01	-0,01	0,00	-0,02	0,08	-0,01	-0,02	-0,01	-0,23	-0,03	-0,03	0,00	-0,06	0,01	0,14	-0,02	0,05	0,01	0,02	0,08	0,00	0,02
MK	0,02	0,00	0,05	0,03	0,01	0,02	-0,02	-0,02	0,01	0,03	-0,03	-0,01	0,01	0,01	-0,01	0,08	0,02	0,01	-0,01	0,03	0,01	0,01	0,01	0,03	-0,02	-0,01	0,00	0,00	0,01	0,02	-0,01	0,02	0,00
ML	0,00	0,05	-0,13	0,01	0,03	0,01	-0,08	-0,21	0,00	0,00	-0,14	-0,01	0,00	0,00	-0,06	-0,04	-0,08	0,02	0,03	0,18	0,00	0,01	-0,01	0,03	0,03	-0,09	0,02	-0,09	-0,01	-0,04	-0,10	-0,04	0,02
MM	-0,03	-0,04	0,10	0,04	-0,01	0,01	0,00	0,15	0,01	0,09	0,11	0,01	0,02	0,01	0,06	0,02	0,03	-0,01	-0,05	0,04	0,02	0,01	0,01	-0,03	-0,08	0,03	0,00	0,06	0,02	0,03	0,08	0,08	-0,01
MR	0,00	0,03	0,04	0,21	0,08	0,09	-0,25	-0,28	0,05	0,33	-0,22	-0,04	0,06	0,04	-0,08	0,20	-0,16	0,05	-0,08	0,60	0,06	0,09	0,03	0,08	-0,17	-0,14	0,03	-0,10	0,04	0,02	-0,10	0,13	0,06
MT	-0,40	-0,29	0,28	-0,01	-0,19	-0,10	0,27	1,52	0,02	0,34	1,26	0,16	0,13	0,03	0,61	-0,66	0,04	-0,17	-0,29	-0,12	0,13	-0,03	0,00	-0,52	-0,41	0,38	0,00	0,52	0,08	0,12	0,72	0,46	-0,19
MV	-0,40	-0,09	0,10	0,23	-0,06	-0,04	-0,14	0,52	0,01	0,24	0,64	0,08	0,09	0,01	0,18	-0,54	-0,23	-0,09	-0,24	0,58	0,05	0,03	-0,06	-0,33	-0,23	0,08	0,05	0,18	-0,04	-0,12	0,33	0,26	0,66
MW	-0,20	-0,08	0,07	-0,18	-0,16	-0,18	0,33	0,62	-0,08	-0,57	0,68	0,09	-0,08	-0,09	0,14	-0,55	0,25	-0,12	0,03	-0,78	-0,13	-0,14	-0,10	-0,24	0,25	0,26	-0,03	0,25	-0,13	-0,16	0,30	-0,15	0,56
MX	0,05	-0,12	0,20	-0,07	-0,05	0,01	0,17	0,51	0,04	0,08	0,32	0,02	0,03	0,01	0,27	0,06	0,31	-0,02	-0,04	-0,31	0,07	0,00	0,06	0,01	-0,13	0,05	-0,02	0,21	0,08	0,17	0,18	0,14	-0,51
MZ	-0,07	0,01	-0,11	0,01	0,00	0,00	-0,06	0,02	0,00	0,07	0,06	0,01	0,02	0,00	0,06	-0,17	-0,05	-0,01	-0,02	0,21	0,04	0,02	0,00	-0,03	-0,04	-0,07	0,02	-0,01	0,00	-0,02	0,00	0,04	-0,05
NE	0,07	0,20	-0,10	0,49	0,25	0,22	-0,70	-1,19	0,08	0,71	-0,98	-0,13	0,10	0,10	-0,45	0,59	-0,64	0,16	-0,11	1,56	0,09	0,21	0,05	0,23	-0,27	-0,38	0,08	-0,45	0,05	-0,03	-0,46	0,15	0,31
NG	0,10	0,03	-0,01	-0,07	0,01	-0,01	0,06	-0,18	-0,02	-0,16	-0,18	-0,02	-0,05	-0,01	-0,11	0,14	0,05	0,02	0,08	-0,25	-0,06	-0,03	0,00	0,07	0,13	0,02	-0,02	-0,06	-0,02	-0,01	-0,09	-0,12	0,01
NI	0,09	0,07	-0,23	-0,18	0,01	-0,04	0,09	-0,25	-0,05	-0,11	-0,33	0,00	-0,05	0,01	-0,17	0,05	-0,24	0,00	0,12	-0,26	-0,07	-0,05	-0,02	-0,05	0,15	0,13	-0,03	-0,15	-0,01	-0,03	-0,11	-0,17	-0,17
PA	0,26	0,23	-0,41	-0,16	0,11	0,02	-0,07	-1,02	-0,05	-0,29	-0,92	-0,08	-0,11	-0,02	-0,44	0,29	-0,23	0,08	0,27	-0,11	-0,12	-0,03	-0,02	0,22	0,35	-0,12	-0,01	-0,41	-0,06	-0,10	-0,49	-0,40	-0,08
PE	-0,20	-0,17	0,44	0,20	-0,06	-0,02	0,07	0,72	0,03	0,51	0,45	0,06	0,11	0,08	0,09	0,08	-0,37	-0,08	-0,29	0,13	0,02	0,02	0,01	-0,38	-0,38	0,47	-0,02	0,24	0,08	0,11	0,48	0,38	0,26
PG	0,03	-0,02	0,08	0,02	0,01	0,02	0,01	0,04	0,02	0,10	-0,02	-0,01	0,02	0,02	0,01	0,13	0,00	0,01	-0,03	0,00	0,02	0,01	0,02	0,01	-0,07	0,03	-0,01	0,02	0,03	0,06	0,03	0,06	-0,11
PH	0,00	0,11	-0,27	-0,09	0,01	-0,04	-0,03	-0,37	-0,05	-0,34	-0,17	-0,01	-0,07	-0,05	-0,13	-0,20	0,01	0,01	0,14	-0,07	-0,07	-0,04	-0,06	0,07	0,25	-0,13	0,01	-0,14	-0,10	-0,16	-0,19	-0,24	0,23
PY	0,16	0,10	-0,11	-0,06	0,06	0,01	-0,01	-0,51	-0,03	-0,04	-0,55	-0,04	-0,05	0,02	-0,31	0,33	-0,26	0,04	0,10	-0,11	-0,09	-0,02	0,00	0,06	0,13	0,09	-0,03	-0,22	-0,01	-0,02	-0,20	-0,17	0,00
QA	-0,03	-0,05	0,15	0,05	-0,03	0,00	0,03	0,21	0,01	-0,04	0,22	0,01	0,01	-0,01	0,10	-0,03	0,18	-0,01	-0,05	-0,06	0,01	0,00	0,00	0,01	-0,03	-0,01	0,00	0,11	0,00	0,01	0,09	0,05	0,11
QT	0,25	-0,05	0,22	-0,17	-0,04	-0,02	0,29	0,11	-0,01	-0,29	-0,07	-0,03	-0,07	-0,02	-0,02	0,38	0,38	0,02	0,11	-0,84	-0,08	-0,06	0,04	0,15	0,16	0,14	-0,07	0,09	0,02	0,12	0,01	-0,13	-0,30
RU	-0,36	-0,40	0,67	0,05	-0,23	-0,10	0,40	1,87	0,04	0,41	1,44	0,15	0,15	0,05	0,65	-0,38	0,17	-0,18	-0,39	-0,42	0,11	-0,03	0,03	-0,55	-0,51	0,59	-0,04	0,69	0,12	0,23	0,93	0,58	-0,12
RW	0,30	0,16	-0,05	0,12	0,15	0,13	-0,25	-0,98	0,03	0,04	-0,88	-0,12	-0,04	0,02	-0,38	0,66	-0,06	0,14	0,12	0,30	-0,03	0,07	0,04	0,37	0,10	-0,26	0,00	-0,33	0,01	0,02	-0,46	-0,16	-0,03
SC	-0,02	0,03	-0,05	0,02	0,01	-0,01	-0,04	-0,13	-0,01	-0,09	-0,04	0,00	-0,02	-0,02	-0,05	-0,06	0,00	0,00	0,02	0,05	-0,02	0,00	-0,02	0,02	0,07	-0,05	0,01	-0,04	-0,04	-0,06	-0,05	-0,06	0,18
SD	-0,04	-0,02	0,03	0,03	-0,01	0,00	-0,01	0,09	0,01	0,03	0,10	0,01	0,01	0,00	0,05	-0,06	0,02	-0,01	-0,03	0,06	0,02	0,01	0,00	-0,02	-0,03	-0,01	0,01	0,04	0,00	0,00	0,04	0,04	0,05
SI	-0,35	-0,22	0,13	0,00	-0,14	-0,07	0,14	1,20	0,03	0,26	1,07	0,13	0,12	0,01	0,59	-0,68	0,14	-0,13	-0,23	0,09	0,16	0,00	0,01	-0,35	-0,34	0,13	0,03	0,42	0,06	0,08	0,52	0,38	-0,23
SL	-0,03	0,13	-0,30	-0,01	0,04	-0,01	-0,14	-0,47	-0,03	-0,21	-0,24	-0,01	-0,05	-0,04	-0,15	-0,20	-0,10	0,02	0,10	0,22	-0,04	-0,01	-0,06	0,07	0,19	-0,19	0,03	-0,19	-0,09	-0,17	-0,23	-0,19	0,29
SN	-0,11	0,00	-0,01	-0,03	-0,05	-0,07	0,07	0,13	-0,04	-0,19	0,19	0,03	-0,03	-0,03	-0,02	-0,23	-0,05	-0,05	0,00	-0,16	-0,07	-0,05	-0,06	-0,13	0,10	0,11	0,00	0,04	-0,07	-0,11	0,09	-0,07	0,39
SR	0,32	0,23	-0,47	-0,25	0,11	0,02	-0,02	-1,01	-0,06	-0,31	-0,96	-0,08	-0,12	-0,01	-0,41	0,32	-0,17	0,09	0,31	-0,25	-0,11	-0,04	-0,01	0,25	0,37	-0,13	-0,02	-0,42	-0,04	-0,06	-0,52	-0,42	-0,34
SS	0,01	0,00	0,05	0,03	0,00	0,01	-0,01	0,00	0,01	0,01	0,00	-0,01	0,00	0,00	-0,01	0,04	0,02	0,01	-0,01	0,01	0,00	0,01	0,00	0,02	-0,01	-0,01	0,00	0,01	0,00	0,01	0,00	0,01	0,04
ST	-1,24	-0,79	0,49	-0,09	-0,53	-0,31	0,67	4,25	0,04	0,91	3,64	0,47	0,38	0,06	1,84	-2,29	0,10	-0,51	-0,79	-0,05	0,43	-0,07	-0,02	-1,47	-1,12	0,87	0,04	1,43	0,19	0,25	1,96	1,27	-0,62
SV	0,27	0,66	-1,80	-0,58	0,21	-0,06	-0,36	-2,47	-0,19	-1,34	-1,59	-0,10	-0,32	-0,20	-0,63	-0,73	-0,04	0,14	0,83	0,15	-0,18	-0,10	-0,18	0,63	1,17	-1,04	0,09	-0,99	-0,34	-0,61	-1,40	-1,20	-0,04
SY	-0,02	-0,06	0,12	0,01	-0,03	-0,01	0,06	0,24	0,01	0,04	0,18	0,01	0,02	0,00	0,09	0,00	0,07	-0,02	-0,05	-0,10	0,01	0,00	0,01	-0,04	-0,06	0,07	-0,01	0,10	0,02	0,04	0,12	0,07	-0,03
TD	-0,04	-0,06	0,11	0,01	-0,03	0,00	0,04	0,28	0,02	0,04	0,25	0,02	0,02	0,00	0,14	-0,07	0,14	-0,02	-0,05	-0,05	0,04	0,01	0,01	-0,02	-0,07	0,01	0,00	0,12	0,02	0,04	0,11	0,09	-0,07
TG	0,13	0,00	0,19	0,04	0,02	0,03	0,02	-0,12	0,02	-0,05	-0,15	-0,04	-0,02	0,00	-0,08	0,33	0,15	0,04	0,02	-0,17	-0,03	0,01	0,03	0,14	0,03	0,00	-0,02	0,00	0,01	0,05	-0,05	-0,03	0,03
TJ	-0,02	-0,01	0,02	0,02	-0,01	0,00	0,00	0,05	0,00	0,01	0,06	0,00	0,01	0,00	0,02	-0,03	0,02	-0,01	-0,02	0,02	0,00	0,00	0,00	-0,01	-0,02	0,00	0,00	0,02	0,00	0,00	0,03	0,02	0,04
TL	-0,09	-0,05	0,11	0,04	-0,03	-0,02	0,03	0,25	0,00	0,07	0,21	0,03	0,02	0,01	0,06	-0,08	-0,05	-0,03	-0,08	0,01	0,00	0,00	-0,01	-0,11	-0,08	0,11	0,00	0,09	0,00	0,00	0,15	0,09	0,14
TN	0,03	0,05	-0,07	0,02	0,03	0,01	-0,07	-0,23	0,00	-0,04	-0,16	-0,02	-0,01	-0,01	-0,08	0,04	-0,02	0,02	0,03	0,10	-0,01	0,01	-0,01	0,07	0,05	-0,08	0,01	-0,08	-0,02	-0,04	-0,11	-0,06	0,09
TT	0,27	0,23	-0,32	-0,02	0,15	0,07	-0,21	-1,14	-0,02	-0,15	-0,99	-0,10	-0,08	0,00	-0,47	0,43	-0,21	0,12	0,22	0,18	-0,08	0,02	0,00	0,30	0,26	-0,23	0,00	-0,43	-0,04	-0,09	-0,53	-0,32	0,02
UA	0,04	0,02	-0,03	-0,06	0,00	-0,01	0,05	-0,07	-0,01	-0,13	-0,06	-0,01	-0,03	-0,02	-0,04	0,02	0,05	0,00	0,05	-0,17	-0,03	-0,02	-0,01	0,04	0,09	0,00	-0,01	-0,02	-0,02	-0,02	-0,04	-0,08	-0,01
UY	0,02	0,03	-0,14	-0,04	0,03	0,01	-0,02	-0,12	-0,01	0,23	-0,24	0,00	0,02	0,05	-0,09	0,08	-0,36	0,00	0,00	0,15	0,01	0,01	0,01	-0,10	-0,08	0,12	-0,01	-0,11	0,05	0,04	-0,03	0,03	-0,25

Synthetic Controls, Wealth Top 1%

UNTREATED COUNTRIES (ROWS) / TREATED COUNTRIES (COLUMNS)	AE	BH	BR	BT	CA	CR	CY	CZ	DZ	EE	FI	GB	GR	HR	HU	ID	IL	IN	JO	KZ	LK	LT	LV	MN	MU	NL	NO	NP	NZ	PL	RO	SA	SE	TR
AL	0,02	0,00	0,06	0,00	0,00	-0,01	0,04	0,09	0,01	0,05	-0,01	0,01	0,01	0,00	0,01	0,04	0,00	0,01	0,00	-0,05	0,06	0,03	0,01	0,00	0,01	0,03	0,00	0,00	0,03	0,02	0,03	-0,07	0,05	-0,01
AM	0,00	0,00	0,00	0,01	0,01	0,01	-0,03	-0,03	0,01	-0,01	0,00	-0,01	-0,01	0,00	0,00	0,00	0,04	0,02	0,01	0,02	-0,01	0,00	0,00	0,01	0,02	0,01	0,00	0,00	0,00	0,00	0,01	0,02	0,00	-0,03
AO	0,07	0,01	0,33	0,03	-0,11	-0,11	-0,08	0,65	0,03	0,36	-0,12	0,19	0,10	0,01	-0,04	0,49	-0,55	0,13	-0,08	-0,39	0,58	0,16	0,06	-0,08	-0,01	0,15	-0,20	0,09	0,18	0,01	-0,03	-0,61	0,18	0,42
AT	0,09	0,00	0,09	-0,15	-0,07	-0,10	0,16	0,49	0,00	0,22	-0,06	0,15	0,10	0,00	-0,02	0,36	-0,36	0,11	-0,06	-0,15	0,27	0,13	0,02	-0,04	-0,05	0,00	-0,12	0,05	0,09	0,05	0,04	-0,41	0,11	-0,03
BE	-0,03	0,00	-0,04	0,07	0,04	0,06	-0,09	-0,23	0,00	-0,10	0,03	-0,07	-0,05	0,00	0,01	-0,15	0,19	-0,03	0,03	0,08	-0,13	-0,06	-0,01	0,03	0,04	0,01	0,05	-0,02	-0,04	-0,02	-0,01	0,19	-0,04	-0,02
BF	-0,21	0,00	-0,21	0,08	-0,03	-0,02	-0,08	-0,48	-0,07	-0,27	0,03	-0,05	-0,04	-0,06	-0,06	-0,38	-0,11	-0,22	-0,02	0,18	-0,37	-0,21	-0,09	-0,08	-0,15	-0,17	0,08	-0,05	-0,13	-0,17	-0,22	0,42	-0,31	0,64
BG	0,05	0,01	0,11	-0,02	-0,01	-0,04	0,14	0,23	0,01	0,14	-0,02	0,02	0,04	0,01	0,02	0,07	-0,04	-0,03	-0,02	-0,14	0,16	0,07	0,02	-0,01	-0,03	0,05	0,03	0,01	0,05	0,05	0,07	-0,16	0,12	-0,03
BI	0,02	0,00	0,10	0,00	-0,04	-0,03	0,06	0,17	0,00	0,07	-0,03	0,05	0,03	-0,01	-0,01	0,11	-0,12	0,06	-0,02	-0,07	0,08	0,03	0,00	-0,03	-0,01	0,01	-0,03	0,01	0,05	0,00	0,00	-0,13	0,03	0,16
BJ	0,07	0,01	0,08	-0,30	-0,21	-0,23	0,43	0,75	-0,05	0,20	-0,14	0,30	0,18	-0,06	-0,10	0,54	-0,80	0,27	-0,14	-0,06	0,11	0,11	-0,04	-0,15	-0,17	-0,21	-0,21	0,06	0,12	-0,02	-0,06	-0,55	-0,06	0,54
BO	-0,14	-0,01	-0,32	0,09	0,09	0,11	-0,29	-0,69	-0,02	-0,31	0,10	-0,15	-0,11	0,00	0,01	-0,42	0,30	-0,21	0,06	0,25	-0,34	-0,17	-0,03	0,05	0,01	-0,06	0,09	-0,04	-0,18	-0,08	-0,10	0,53	-0,21	-0,16
BW	0,01	0,00	0,08	0,03	0,01	0,00	0,08	0,04	0,01	0,05	0,00	-0,03	0,00	0,00	0,02	-0,04	0,08	-0,03	0,01	-0,06	0,03	0,01	0,01	0,00	0,00	0,04	0,05	-0,01	0,03	0,02	0,04	-0,01	0,06	0,02
BY	0,01	0,00	0,00	-0,01	0,01	0,00	0,04	0,00	0,00	0,00	0,01	-0,01	0,00	0,00	0,01	-0,01	0,05	0,00	0,01	0,01	-0,01	0,01	0,00	0,01	0,00	0,00	0,02	-0,01	0,00	0,02	0,02	0,01	0,02	-0,08
CF	0,15	-0,01	-0,13	-0,40	-0,10	-0,08	0,49	0,35	-0,02	-0,08	-0,06	0,17	0,07	-0,04	-0,05	0,39	-0,19	0,48	-0,04	0,37	-0,36	0,06	-0,05	-0,02	0,02	-0,31	-0,13	0,01	0,01	0,05	0,07	-0,18	-0,12	-0,24
CH	0,07	0,00	0,06	-0,13	-0,07	-0,07	0,22	0,33	-0,01	0,10	-0,05	0,10	0,07	-0,01	-0,02	0,23	-0,22	0,14	-0,04	-0,03	0,06	0,07	0,00	-0,04	-0,04	-0,06	-0,06	0,02	0,06	0,03	0,03	-0,24	0,03	0,05
CI	-0,09	0,01	0,09	0,31	0,13	0,10	-0,29	-0,37	0,03	0,02	0,08	-0,20	-0,08	0,04	0,08	-0,41	0,40	-0,37	0,07	-0,22	0,17	-0,05	0,04	0,05	0,02	0,24	0,18	-0,03	-0,03	0,00	0,02	0,26	0,12	-0,04
CL	0,02	0,00	0,03	0,01	-0,01	0,03	-0,08	-0,03	0,01	-0,06	-0,02	0,02	-0,03	0,00	-0,02	0,08	0,03	0,16	0,01	0,09	-0,08	-0,01	0,00	0,01	0,09	-0,02	-0,06	0,01	0,01	-0,01	-0,01	0,00	-0,04	0,04
CM	0,02	0,00	0,06	-0,06	-0,07	-0,07	0,05	0,26	-0,01	0,09	-0,05	0,10	0,05	-0,01	-0,03	0,20	-0,28	0,08	-0,04	-0,07	0,12	0,04	0,00	-0,05	-0,04	-0,02	-0,09	0,03	0,05	-0,01	-0,03	-0,22	0,01	0,19
CV	-0,22	0,00	-0,17	0,33	0,16	0,15	-0,33	-0,90	-0,02	-0,29	0,15	-0,31	-0,16	0,01	0,06	-0,79	0,60	-0,49	0,10	0,07	-0,28	-0,22	-0,03	0,06	-0,02	0,09	0,29	-0,09	-0,17	-0,09	-0,08	0,73	-0,12	0,04
DE	0,03	0,00	-0,01	-0,10	-0,02	-0,05	0,19	0,17	-0,01	0,08	-0,01	0,03	0,05	0,00	0,01	0,05	-0,08	-0,02	-0,02	-0,04	0,04	0,04	0,00	-0,01	-0,07	-0,03	0,02	0,00	0,01	0,04	0,05	-0,10	0,04	-0,09
DO	-0,12	-0,01	-0,25	0,12	0,10	0,11	-0,26	-0,63	-0,01	-0,27	0,10	-0,16	-0,11	0,01	0,02	-0,42	0,35	-0,22	0,06	0,20	-0,29	-0,15	-0,02	0,05	0,02	-0,02	0,11	-0,05	-0,15	-0,06	-0,07	0,50	-0,15	-0,17
ES	0,00	0,00	-0,04	-0,10	-0,06	-0,07	0,10	0,17	-0,02	0,03	-0,03	0,09	0,05	-0,02	-0,04	0,14	-0,28	0,05	-0,05	0,01	0,01	0,01	-0,02	-0,05	-0,08	-0,08	-0,07	0,02	0,01	-0,02	-0,05	-0,13	-0,05	0,16
ET	0,06	0,00	0,09	-0,04	-0,02	-0,02	0,10	0,21	0,01	0,09	-0,03	0,04	0,03	0,00	0,00	0,14	-0,05	0,09	-0,01	-0,05	0,09	0,06	0,01	0,00	0,02	0,02	-0,03	0,01	0,05	0,04	0,05	-0,16	0,07	-0,02
FR	-0,04	0,00	0,01	-0,01	-0,06	-0,06	0,03	0,09	-0,02	0,03	-0,03	0,06	0,04	-0,02	-0,04	0,05	-0,27	-0,03	-0,04	-0,05	0,03	-0,01	-0,02	-0,06	-0,09	-0,05	-0,04	0,01	0,01	-0,05	-0,08	-0,07	-0,06	0,35
GE	0,09	0,00	0,06	-0,11	-0,06	-0,05	-0,05	0,39	0,02	0,16	-0,06	0,15	0,06	0,01	-0,03	0,41	-0,32	0,19	-0,04	-0,09	0,26	0,12	0,04	-0,01	0,03	0,03	-0,19	0,07	0,08	0,03	0,01	-0,37	0,08	-0,10
GH	0,01	0,00	0,04	0,02	0,03	0,00	0,03	0,05	0,01	0,09	0,01	-0,03	0,01	0,02	0,03	-0,04	0,05	-0,12	0,00	-0,12	0,13	0,04	0,02	0,01	-0,03	0,07	0,05	0,00	0,01	0,04	0,05	-0,04	0,10	-0,13
GN	-0,10	0,00	0,02	0,15	0,02	0,01	-0,13	-0,19	-0,01	-0,02	0,02	-0,07	-0,02	-0,01	0,00	-0,23	0,02	-0,22	0,00	-0,10	0,04	-0,07	-0,01	-0,03	-0,06	0,06	0,08	-0,02	-0,02	-0,06	-0,07	0,14	-0,03	0,31
GQ	0,01	0,00	-0,03	-0,08	-0,01	-0,04	0,17	0,10	-0,01	0,05	0,00	0,02	0,04	0,00	0,01	0,00	-0,06	-0,06	-0,02	-0,03	0,02	0,03	-0,01	-0,01	-0,08	-0,04	0,04	-0,01	0,00	0,03	0,03	-0,05	0,03	-0,08
GT	-0,11	-0,01	-0,25	0,11	0,10	0,11	-0,29	-0,61	-0,01	-0,25	0,10	-0,15	-0,10	0,01	0,02	-0,39	0,32	-0,20	0,06	0,19	-0,25	-0,14	-0,02	0,06	0,02	-0,01	0,09	-0,04	-0,15	-0,05	-0,07	0,46	-0,14	-0,20
GW	0,33	0,00	0,20	-0,42	-0,14	-0,05	0,67	0,78	0,03	0,06	-0,14	0,22	0,08	-0,02	-0,04	0,75	-0,07	0,90	-0,02	0,32	-0,26	0,18	0,00	0,02	0,23	-0,24	-0,22	0,03	0,17	0,15	0,24	-0,51	0,06	-0,32
GY	-0,06	-0,01	-0,27	0,01	0,11	0,08	-0,12	-0,47	-0,01	-0,18	0,10	-0,14	-0,07	0,02	0,05	-0,34	0,34	-0,23	0,05	0,16	-0,20	-0,08	-0,01	0,07	-0,01	-0,01	0,12	-0,04	-0,14	0,01	0,01	0,38	-0,07	-0,47
HN	-0,04	-0,01	-0,18	0,01	0,02	0,08	-0,24	-0,36	0,00	-0,26	0,03	-0,02	-0,08	-0,01	-0,04	-0,05	0,12	0,15	0,04	0,30	-0,31	-0,10	-0,02	0,04	0,11	-0,10	-0,07	-0,01	-0,09	-0,06	-0,08	0,26	-0,20	-0,06
IQ	0,01	0,00	0,04	0,03	0,01	0,02	-0,03	-0,01	0,01	0,00	0,00	-0,02	-0,02	0,01	0,01	0,01	0,07	0,04	0,01	0,00	0,00	0,00	0,01	0,01	0,05	0,03	0,00	0,00	0,01	0,01	0,02	0,00	0,02	-0,02
IS	0,01	0,00	-0,16	-0,16	0,01	-0,06	0,27	0,04	-0,03	0,01	0,03	0,00	0,05	-0,01	0,02	-0,08	-0,03	-0,13	-0,02	0,05	-0,08	0,01	-0,03	-0,01	-0,13	-0,10	0,08	-0,02	-0,05	0,04	0,04	0,04	-0,01	-0,23
IT	0,03	0,00	0,07	0,05	0,01	0,03	-0,14	0,04	0,03	0,04	-0,01	0,01	-0,01	0,02	0,00	0,10	0,02	0,08	0,01	-0,04	0,11	0,03	0,03	0,02	0,08	0,07	-0,06	0,02	0,03	0,01	0,01	-0,08	0,05	-0,07
JM	-0,10	-0,02	-0,38	0,01	0,10	0,10	-0,26	-0,63	-0,02	-0,30	0,11	-0,13	-0,10	0,01	0,01	-0,35	0,30	-0,18	0,06	0,29	-0,33	-0,13	-0,03	0,07	0,01	-0,07	0,07	-0,04	-0,19	-0,04	-0,07	0,49	-0,19	-0,40
KE	0,02	0,02	0,19	0,10	0,11	-0,02	0,33	0,12	0,01	0,28	0,06	-0,17	0,04	0,04	0,14	-0,35	0,34	-0,48	0,02	-0,40	0,32	0,08	0,04	0,02	-0,15	0,21	0,32	-0,05	0,04	0,13	0,20	-0,01	0,33	-0,27
KH	0,01	0,00	0,07	0,04	0,00	0,01	0,02	0,01	0,01	0,01	-0,01	-0,01	-0,01	0,00	0,00	-0,01	0,06	0,04	0,01	-0,01	-0,01	0,00	0,00	0,00	0,03	0,02	0,01	-0,01	0,02	0,00	0,02	0,00	0,02	0,08
LA	0,03	0,00	0,05	-0,01	0,00	0,00	0,07	0,08	0,01	0,03	-0,01	0,00	0,01	0,00	0,01	0,04	0,03	0,05	0,00	-0,01	0,01	0,02	0,01	0,00	0,02	0,01	0,00	0,00	0,02	0,02	0,04	-0,05	0,04	-0,03
LS	-0,16	-0,02	-0,17	0,36	0,16	0,30	-0,63	-1,14	0,03	-0,57	0,12	-0,29	-0,27	0,01	0,00	-0,58	0,82	0,06	0,16	0,47	-0,65	-0,28	-0,02	0,13	0,31	0,01	0,10	-0,08	-0,18	-0,13	-0,11	0,86	-0,29	0,00
MA	0,02	0,00	-0,01	0,00	0,03	0,03	-0,01	-0,05	0,01	-0,02	0,02	-0,03	-0,02	0,01	0,02	-0,03	0,13	0,01	0,02	0,03	-0,02	0,01	0,01	0,03	0,04	0,02	0,02	-0,01	-0,01	0,02	0,04	0,04	0,03	-0,18
MG	-0,01	0,00	-0,04	-0,06	-0,02	-0,01	0,21	-0,05	-0,02	-0,10	0,00	-0,01	0,00	-0,03	-0,01	-0,08	0,04	0,06	0,00	0,15	-0,25	-0,05	-0,04	-0,02	-0,03	-0,13	0,05	-0,03	-0,02	-0,02	0,00	0,12	-0,09	0,12
MK	0,01	0,00	0,05	0,04	0,01	0,02	0,00	-0,02	0,01	0,00	0,00	-0,03	-0,02	0,00	0,01	-0,02	0,09	0,02	0,02	-0,01	-0,01	0,00	0,01	0,01	0,04	0,03	0,02	-0,01	0,01	0,01	0,03	0,02	0,03	-0,01
ML	-0,03	0,00	-0,04	0,02	0,01	0,00	-0,06	-0,06	-0,01	0,01	0,02	-0,01	0,00	0,00	0,01	-0,06	-0,02	-0,11	0,00	-0,04	0,04	-0,01	0,00	0,00	-0,04	0,02	0,02	0,00	-0,02	-0,01	-0,02	0,03	0,00	-0,02
MM	0,02	0,00	0,08	0,03	0,01	0,01	0,03	0,06	0,01	0,05	-0,01	-0,01	0,00	0,01	0,01	0,01	0,05	0,02	0,01	-0,05	0,05	0,02	0,01	0,01	0,02	0,04	0,02	0,00	0,03	0,02	0,04	-0,04	0,06	0,00
MR	-0,02	0,00	0,09	0,18	0,09	0,06	-0,22	-0,12	0,03	0,11	0,05	-0,11	-0,03	0,04	0,06	-0,18	0,21	-0,26	0,04	-0,24	0,28	0,02	0,05	0,05	0,02	0,22	0,09	0,00	0,01	0,03	0,04	0,04	0,17	-0,19
MT	0,09	0,01	0,12	-0,15	-0,08	-0,11	0,25	0,52	0,00	0,23	-0,07	0,14	0,11	0,00	-0,01	0,31	-0,33	0,08	-0,06	-0,17	0,26	0,13	0,01	-0,05	-0,08	0,00	-0,07	0,04	0,10	0,06	0,06	-0,40	0,12	0,03
MV	-0,11	0,00	-0,05	0,09	-0,05	-0,09	-0,44	0,11	-0,01	0,17	-0,02	0,12	0,06	0,01	-0,06	0,13	-0,60	-0,29	-0,08	-0,32	0,50	0,02	0,03	-0,08	-0,16	0,13	-0,16	0,08	0,00	-0,09	-0,20	-0,24	-0,01	0,35
MW	-0,02	0,00	0,10	-0,08	-0,17	-0,10	0,22	0,24	-0,04	-0,07	-0,11	0,16	0,05	-0,07	-0,11	0,23	-0,43	0,32	-0,07	0,14	-0,24	-0,05	-0,07	-0,12	-0,03	-0,22	-0,13	0,01	0,06	-0,10	-0,13	-0,14	-0,20	0,84
MX	0,15	-0,01	0,07	-0,13	-0,01	0,03	0,15	0,22	0,03	0,00	-0,04	0,05	0,00	0,01	0,00	0,28	0,12	0,37	0,02	0,15	-0,09	0,08	0,02	0,05	0,16	-0,04	-0,09	0,01	0,06	0,08	0,12	-0,16	0,06	-0,34
MZ	0,01	0,00	0,03	0,01	0,00	-0,02	-0,03	0,11	0,01	0,10	0,00	0,02	0,03	0,01	0,01	0,05	-0,08	-0,07	-0,01	-0,13	0,18	0,04	0,02	0,00	-0,03	0,07	-0,01	0,02	0,02	0,02	0,01	-0,11	0,08	-0,06
NE	-0,16	0,01	0,00	0,43	0,22	0,12	-0,58	-0,54	0,04	0,14	0,16	-0,29	-0,08	0,08	0,14	-0,63	0,45	-0,82	0,07	-0,49	0,53	-0,03	0,08	0,09	-0,08	0,44	0,27	-0,02	-0,08	0,02	0,01	0,31	0,26	-0,36
NG	-0,03	0,00	-0,07	0,00	-0,01	0,03	0,00	-0,21	-0,01	-0,19	0,00	-0,02	-0,05	-0,02	-0,03	-0,07	0,09	0,14	0,02	0,22	-0,31	-0,09	-0,04	0,00	0,05	-0,12	-0,01	-0,02	-0,04	-0,05	-0,05	0,19	-0,16	0,16
NI	-0,03	0,00	-0,17	-0,09	0,03	-0,02	0,19	-0,14	-0,03	-0,07	0,05	-0,05	0,01	-0,01	0,02	-0,20	0,09	-0,17	0,00	0,09	-0,17	-0,04	-0,03	0,00	-0,11	-0,09	0,12	-0,04	-0,08	0,01	0,02	0,18	-0,05	-0,18
PA	-0,13	-0,01	-0,32	0,06	0,09	0,10	-0,24	-0,65	-0,02	-0,31	0,10	-0,14	-0,10	0,00	0,01	-0,39	0,29	-0,19	0,05	0,27	-0,35	-0,16	-0,04	0,05	0,00	-0,07	0,09	-0,04	-0,17	-0,07	-0,09	0,51	-0,20	-0,20
PE	-0,01	0,01	0,18	0,11	0,04	-0,02	0,10	0,12	0,01	0,20	0,01	-0,07	0,03	0,02	0,06	-0,15	0,09	-0,27	0,00	-0,30	0,27	0,05	0,03	-0,01	-0,08	0,16	0,15	-0,02	0,05	0,05	0,08	-0,07	0,19	0,04
PG	0,01	0,00	0,02	0,02	0,03	0,02	0,02	-0,03	0,01	0,00	0,01	-0,04	-0,01	0,01	0,02	-0,05	0,13	-0,01	0,02	0,00	-0,01	0,01	0,01	0,02	0,02	0,03	0,04	-0,01	0,00	0,02	0,04	0,04	0,04	-0,12
PH	-0,07	-0,01	-0,17	-0,08	-0,09	-0,06	-0,15	-0,04	-0,04	-0,12	-0,03	0,13	0,02	-0,03	-0,09	0,14	-0,42	0,09	-0,06	0,15	-0,12	-0,06	-0,04	-0,07	-0,06	-0,15	-0,17	0,03	-0,05	-0,11	-0,19	-0,01	-0,23	0,37
PY	-0,09	0,00	-0,18	0,08	0,09	0,07	-0,11	-0,47	-0,01	-0,18	0,08	-0,14	-0,07	0,00	0,03	-0,38	0,31	-0,22	0,05	0,12	-0,22	-0,11	-0,02	0,04	-0,02	-0,01	0,14	-0,05	-0,11	-0,03	-0,03	0,39	-0,09	-0,15
QA	0,01	0,00	0,07	0,03	-0,03	0,02	-0,09	0,02	0,01	-0,05	-0,03	0,04	-0,02	-0,01	-0,03	0,12	-0,04	0,19	0,00	0,06	-0,05	-0,01	0,00	0,00	0,09	-0,02	-0,09	0,01	0,03	-0,03	-0,04	-0,04	-0,05	0,18
QT	0,10	0,00	0,18	0,04	0,00	0,06	-0,09	0,15	0,05	0,04	-0,04	0,02	-0,03	0,02	0,00	0,23	0,11	0,30	0,03	0,01	0,07	0,06	0,04	0,04	0,19	0,08	-0,10	0,02	0,08	0,04	0,07	-0,16	0,09	-0,09
RU	0,11	0,02	0,23	-0,14	-0,05	-0,18	0,44	0,77	0,00	0,49	-0,06	0,11	0,18	0,02	0,05	0,25	-0,37	-0,22	-0,09	-0,50	0,60	0,22	0,04	-0,06	-0,22	0,13	0,05	0,04	0,15	0,13	0,15	-0,58	0,34	-0,09
RW	-0,04	0,00	0,00	0,21	0,12	0,15	-0,28	-0,46	0,04	-0,14	0,08	-0,18	-0,12	0,03	0,06	-0,31	0,52	-0,09	0,09	0,06	-0,10	-0,07	0,03	0,09	0,15	0,13	0,11	-0,04	-0,06	0,01	0,04	0,33	0,03	-0,27
SC	-0,03	0,00	0,02	0,04	-0,01	0,00	-0,03	-0,03	-0,01	-0,02	-0,01	0,00	-0,01	-0,01	-0,01	-0,03	-0,03	-0,01	0,00	0,00	-0,02	-0,02	-0,01	-0,02	-0,01	-0,01	0,00	0,00	0,00	-0,03	-0,03	0,03	-0,03	0,16
SD	0,01	0,00	0,06	0,01	-0,02	-0,02	0,02	0,09	0,00	0,05	-0,02	0,02	0,01	0,00	-0,01	0,05	-0,06	0,02	-0,01	-0,05	0,06	0,02	0,00	-0,01	0,00	0,01	-0,02	0,01	0,03	0,00	0,00	-0,07	0,02	0,10
SI	0,14	-0,01	0,03	-0,26	-0,14	-0,14	-0,08	0,73	0,02	0,28	-0,12	0,32	0,14	0,01	-0,08	0,78	-0,76	0,31	-0,11	-0,15	0,48	0,21	0,05	-0,05	-0,01	0,00	-0,38	0,13	0,12	0,04	-0,04	-0,71	0,08	-0,13
SL	-0,06	0,00	-0,02	0,00	-0,04	-0,06	0,06	0,03	-0,03	0,02	-0,01	0,03	0,04	-0,02	-0,02	-0,06	-0,21	-0,13	-0,04	-0,07	0,02	-0,03	-0,03	-0,06	-0,12	-0,04	0,02	0,00	-0,01	-0,05	-0,08	-0,01	-0,06	0,33
SN	-0,08	0,00	-0,03	0,00	-0,08	-0,06	0,04	-0,02	-0,04	-0,07	-0,03	0,06	0,02	-0,04	-0,06	-0,03	-0,27	-0,01	-0,05	0,05	-0,13	-0,07	-0,05	-0,08	-0,10	-0,12	-0,03	0,00	-0,01	-0,10	-0,13	0,04	-0,16	0,55
SR	-0,07	-0,01	-0,36	-0,03	0,10	0,10	-0,17	-0,58	-0,01	-0,29	0,11	-0,13	-0,09	0,01	0,02	-0,32	0,35	-0,13	0,06	0,31	-0,36	-0,12	-0,03	0,08	0,03	-0,08	0,08	-0,04	-0,17	-0,02	-0,03	0,46	-0,17	-0,49
SS	0,00	0,00	0,04	0,03	0,00	0,01	-0,01	-0,01	0,00	0,00	0,00	-0,01	-0,01	0,00	0,00	-0,01	0,04	0,02	0,01	-0,01	-0,01	0,00	0,00	0,00	0,02	0,02	0,01	0,00	0,01	0,00	0,01	0,01	0,01	0,04
ST	0,32	0,02	0,17	-0,76	-0,34	-0,53	0,84	2,11	-0,04	0,94	-0,25	0,68	0,49	-0,01	-0,09	1,42	-1,78	0,19	-0,32	-0,66	1,15	0,53	0,05	-0,23	-0,46	-0,10	-0,44	0,21	0,31	0,19	0,10	-1,70	0,38	-0,07
SV	-0,24	-0,04	-0,89	-0,29	-0,10	-0,06	-0,56	-0,63	-0,10	-0,52	0,05	0,21	-0,01	-0,06	-0,20	0,07	-0,76	-0,02	-0,09	0,62	-0,51	-0,23	-0,12	-0,07	-0,18	-0,44	-0,34	0,06	-0,31	-0,26	-0,47	0,37	-0,69	0,10
SY	0,02	0,00	0,05	0,00	0,00	0,01	0,01	0,05	0,01	0,01	-0,01	0,01	0,00	0,00	0,00	0,05	0,02	0,07	0,01	0,00	0,01	0,01	0,01	0,01	0,04	0,01	-0,02	0,00	0,02	0,01	0,02	-0,04	0,02	-0,01
TD	0,06	0,00	0,09	-0,03	-0,02	-0,01	0,06	0,17	0,01	0,05	-0,03	0,04	0,01	0,00	-0,01	0,15	-0,03	0,15	0,00	-0,01	0,04	0,04	0,01	0,00	0,05	0,00	-0,05	0,01	0,05	0,02	0,04	-0,13	0,04	0,01
TG	0,01	0,00	0,11	0,13	0,04	0,10	-0,08	-0,21	0,03	-0,12	0,01	-0,10	-0,09	0,00	0,02	-0,11	0,35	0,15	0,06	0,11	-0,19	-0,05	0,01	0,05	0,17	0,03	0,04	-0,03	0,01	0,00	0,04	0,18	-0,01	0,03
TJ	0,00	0,00	0,04	0,01	-0,01	-0,01	0,03	0,04	0,00	0,02	-0,01	0,01	0,01	0,00	0,00	0,01	-0,02	0,01	0,00	-0,03	0,02	0,00	0,00	-0,01	0,00	0,01	0,00	0,00	0,02	0,00	0,00	-0,03	0,01	0,08
TL	0,00	0,00	0,06	0,03	-0,01	-0,01	-0,01	0,04	0,00	0,03	-0,01	0,00	0,00	0,00	0,00	0,01	-0,03	-0,01	0,00	-0,06	0,05	0,01	0,00	-0,01	0,00	0,03	0,00	0,00	0,02	0,00	0,00	-0,04	0,02	0,10
TN	-0,04	0,00	-0,01	0,05	0,01	0,01	-0,07	-0,11	0,00	-0,03	0,01	-0,03	-0,02	0,00	0,00	-0,08	0,03	-0,05	0,01	0,00	-0,02	-0,03	-0,01	0,00	-0,01	0,01	0,02	-0,01	-0,02	-0,02	-0,03	0,08	-0,03	0,08
TT	-0,13	-0,01	-0,27	0,12	0,11	0,11	-0,29	-0,65	-0,01	-0,27	0,11	-0,16	-0,11	0,01	0,02	-0,43	0,34	-0,24	0,06	0,19	-0,27	-0,15	-0,02	0,06	0,01	-0,01	0,11	-0,05	-0,16	-0,06	-0,08	0,50	-0,15	-0,19
UA	0,02	0,00	-0,03	-0,07	-0,03	0,01	0,09	-0,04	-0,01	-0,13	-0,02	0,02	-0,02	-0,02	-0,03	0,06	0,02	0,22	0,01	0,21	-0,26	-0,04	-0,03	0,00	0,06	-0,12	-0,04	-0,01	-0,01	-0,02	-0,01	0,06	-0,11	0,08
UY	-0,05	0,00	-0,22	-0,03	0,09	0,01	-0,01	-0,21	-0,01	0,00	0,09	-0,09	0,00	0,02	0,06	-0,27	0,14	-0,36	0,01	-0,04	0,05	-0,01	0,00	0,04	-0,12	0,04	0,13	-0,02	-0,10	0,04	0,03	0,18	0,04	-0,45

Synthetic Controls, Wealth Top 10%

UNTREATED COUNTRIES (ROWS) / TREATED COUNTRIES (COLUMNS)	AE	BH	BR	BT	CA	CR	CY	CZ	DZ	EE	FI	GB	GR	HR	HU	ID	IL	IN	JO	KZ	LK	LT	LV	MN	MU	NL	NO	NP	NZ	PL	RO	SA	SE	TR
AL	0,04	0,00	0,11	0,00	-0,02	0,01	0,02	0,16	0,01	0,01	-0,01	0,18	0,00	0,00	0,02	0,06	0,00	-0,03	0,00	-0,03	0,12	0,03	0,01	0,01	0,01	-0,04	0,03	0,00	0,06	0,01	0,04	-0,01	0,05	-0,03
AM	-0,01	0,00	-0,03	0,00	0,01	0,00	-0,02	-0,04	0,00	0,00	0,00	-0,04	0,00	0,00	-0,01	0,01	0,00	0,06	0,00	0,01	-0,02	0,00	0,00	0,00	0,02	0,01	-0,03	0,00	-0,01	0,00	-0,01	0,00	-0,01	-0,02
AO	0,04	-0,02	0,43	0,21	-0,07	0,03	-0,20	0,53	0,06	0,07	0,04	0,81	0,00	0,01	0,00	0,30	-0,27	0,14	-0,01	-0,20	0,80	0,12	0,07	-0,01	0,13	-0,09	-0,10	0,06	0,29	-0,02	-0,03	-0,09	0,19	0,34
AT	0,03	0,00	0,07	-0,03	-0,01	0,00	0,05	0,10	0,00	0,00	-0,01	0,08	0,00	0,00	0,02	-0,01	0,03	-0,10	0,00	0,00	0,01	0,00	0,00	0,01	-0,03	-0,03	0,07	-0,01	0,02	0,01	0,04	0,01	0,01	-0,02
BE	0,03	0,00	0,07	-0,05	-0,01	0,00	0,09	0,08	0,00	-0,01	-0,03	0,04	0,00	0,00	0,02	-0,04	0,06	-0,12	0,00	0,02	-0,07	-0,01	-0,01	0,00	-0,04	-0,03	0,09	-0,02	0,01	0,01	0,04	0,02	-0,01	0,00
BF	-0,29	0,05	-0,48	-0,11	0,04	-0,12	0,02	-0,74	-0,11	-0,11	-0,04	-0,71	0,05	-0,07	-0,13	-0,41	-0,24	-0,07	-0,03	0,21	-0,75	-0,27	-0,12	-0,14	-0,19	0,30	-0,05	0,00	-0,30	-0,19	-0,34	0,11	-0,39	0,77
BG	0,09	-0,01	0,25	-0,01	-0,05	0,02	0,06	0,37	0,03	0,02	0,00	0,40	0,00	0,01	0,05	0,09	0,00	-0,17	-0,01	-0,06	0,27	0,06	0,02	0,02	-0,02	-0,11	0,11	-0,01	0,13	0,03	0,10	-0,02	0,10	-0,07
BI	0,05	0,00	0,21	0,01	-0,04	0,01	0,02	0,27	0,02	0,00	-0,03	0,35	0,00	0,00	0,01	0,09	-0,07	-0,03	-0,01	-0,04	0,21	0,04	0,01	0,00	0,01	-0,05	0,04	0,00	0,12	0,00	0,03	-0,02	0,05	0,10
BJ	0,24	0,03	0,62	-0,49	-0,23	-0,08	0,45	1,25	0,00	-0,24	-0,37	1,57	0,07	-0,06	0,02	0,35	-0,61	-0,23	-0,07	0,17	0,15	0,07	-0,09	-0,06	-0,09	-0,13	0,29	-0,05	0,40	-0,08	0,08	-0,04	-0,08	0,35
BO	-0,22	0,00	-0,62	0,19	0,16	0,01	-0,23	-1,00	-0,04	0,09	0,20	-1,22	-0,02	0,02	-0,05	-0,31	0,29	0,13	0,04	0,00	-0,40	-0,11	0,01	0,00	-0,01	0,17	-0,20	0,02	-0,36	0,01	-0,13	0,05	-0,09	-0,16
BW	0,03	0,00	0,11	0,03	-0,01	0,01	0,03	0,07	0,01	0,02	0,00	0,04	-0,01	0,00	0,02	-0,02	0,10	-0,07	0,00	-0,03	0,04	0,01	0,01	0,01	-0,01	-0,04	0,06	-0,01	0,03	0,02	0,05	0,01	0,04	0,00
BY	0,02	0,00	-0,01	-0,03	0,00	0,00	0,03	0,02	0,00	-0,01	-0,01	0,00	0,00	0,00	0,01	0,01	0,02	0,00	0,00	0,02	-0,04	0,01	0,00	0,01	0,00	-0,01	0,01	-0,01	0,00	0,01	0,02	0,00	0,00	-0,07
CF	0,31	0,03	0,09	-0,73	-0,13	-0,07	0,60	0,72	-0,03	-0,34	-0,50	0,72	0,05	-0,05	0,02	0,29	-0,30	0,17	-0,03	0,43	-0,71	0,05	-0,12	0,02	0,02	-0,04	0,18	-0,08	0,13	0,02	0,18	-0,01	-0,23	-0,51
CH	0,13	0,00	0,25	-0,19	-0,08	-0,01	0,17	0,54	0,01	-0,08	-0,13	0,64	0,02	-0,01	0,03	0,19	-0,20	-0,05	-0,02	0,05	0,12	0,06	-0,02	0,00	0,00	-0,08	0,10	-0,02	0,17	0,00	0,08	-0,03	0,02	-0,04
CI	-0,18	-0,02	-0,09	0,50	0,11	0,06	-0,35	-0,62	0,02	0,24	0,33	-0,76	-0,06	0,04	0,02	-0,30	0,45	-0,16	0,04	-0,27	0,29	-0,05	0,08	0,02	-0,02	0,01	-0,06	0,03	-0,15	0,03	-0,05	0,04	0,16	0,16
CL	0,00	0,00	0,01	0,01	0,00	0,00	-0,01	-0,03	0,00	-0,01	-0,03	-0,03	-0,01	0,00	-0,01	0,01	0,02	0,10	0,01	0,01	-0,04	0,00	0,00	0,00	0,04	0,02	-0,03	0,00	0,00	0,00	-0,01	0,00	-0,01	0,03
CM	0,05	0,01	0,15	-0,11	-0,07	-0,03	0,06	0,39	0,00	-0,06	-0,07	0,55	0,03	-0,01	-0,01	0,16	-0,30	-0,02	-0,03	0,02	0,19	0,04	-0,01	-0,03	-0,01	-0,03	0,02	0,01	0,14	-0,03	-0,01	-0,03	0,00	0,13
CV	-0,36	0,01	-0,60	0,39	0,20	0,00	-0,28	-1,39	-0,06	0,16	0,29	-1,70	-0,05	0,00	-0,06	-0,66	0,53	-0,12	0,04	-0,06	-0,55	-0,25	-0,01	-0,03	-0,12	0,23	-0,11	0,01	-0,48	-0,05	-0,22	0,14	-0,13	0,35
DE	0,06	0,00	0,03	-0,13	-0,02	-0,01	0,12	0,14	0,00	-0,05	-0,07	0,12	0,01	0,00	0,02	0,03	-0,01	-0,05	-0,01	0,07	-0,11	0,01	-0,02	0,01	-0,02	-0,03	0,07	-0,02	0,02	0,02	0,06	0,00	-0,02	-0,14
DO	-0,23	0,00	-0,57	0,18	0,14	0,00	-0,20	-0,98	-0,04	0,07	0,17	-1,17	-0,02	0,01	-0,06	-0,35	0,27	0,11	0,03	0,02	-0,46	-0,14	-0,01	-0,01	-0,02	0,18	-0,17	0,02	-0,35	-0,02	-0,15	0,06	-0,12	0,00
ES	0,05	0,02	0,10	-0,22	-0,07	-0,05	0,15	0,33	-0,02	-0,13	-0,18	0,47	0,04	-0,03	-0,03	0,13	-0,32	0,07	-0,03	0,13	-0,11	0,00	-0,05	-0,04	0,00	0,03	0,02	-0,01	0,10	-0,06	-0,04	-0,02	-0,11	0,20
ET	0,10	-0,01	0,21	-0,05	-0,04	0,01	0,07	0,35	0,02	-0,02	-0,06	0,39	0,00	0,00	0,03	0,14	-0,04	0,00	0,00	-0,01	0,17	0,06	0,01	0,02	0,04	-0,08	0,05	-0,01	0,13	0,03	0,08	-0,03	0,07	-0,09
FR	0,00	0,02	0,07	-0,12	-0,05	-0,04	0,08	0,18	-0,02	-0,08	-0,10	0,30	0,03	-0,03	-0,03	0,04	-0,24	-0,01	-0,02	0,07	-0,05	-0,03	-0,04	-0,04	-0,03	0,03	0,03	0,00	0,06	-0,06	-0,06	0,00	-0,09	0,27
GE	0,08	-0,01	0,14	-0,03	-0,04	0,01	-0,02	0,35	0,03	0,00	-0,01	0,46	0,01	0,01	0,02	0,22	-0,16	0,07	-0,01	-0,04	0,32	0,10	0,03	0,02	0,06	-0,07	-0,03	0,02	0,14	0,02	0,05	-0,06	0,09	-0,14
GH	0,02	-0,01	0,06	0,06	0,00	0,02	-0,04	0,07	0,02	0,06	0,08	0,05	-0,01	0,02	0,03	0,01	0,07	-0,13	0,00	-0,08	0,21	0,04	0,03	0,02	-0,02	-0,07	0,04	0,00	0,03	0,04	0,05	-0,01	0,09	-0,12
GN	-0,16	0,01	-0,10	0,18	0,04	-0,01	-0,15	-0,34	-0,02	0,07	0,12	-0,32	0,00	-0,01	-0,04	-0,19	0,01	-0,10	0,00	-0,07	0,04	-0,08	0,00	-0,05	-0,06	0,08	-0,04	0,02	-0,09	-0,06	-0,14	0,03	-0,03	0,41
GQ	0,06	0,01	0,04	-0,15	-0,03	-0,02	0,14	0,20	-0,01	-0,05	-0,05	0,19	0,02	-0,01	0,02	0,02	-0,05	-0,15	-0,01	0,06	-0,06	0,01	-0,03	0,00	-0,07	-0,04	0,11	-0,02	0,03	0,01	0,05	0,01	-0,02	-0,11
GT	-0,22	0,00	-0,58	0,17	0,14	0,00	-0,22	-0,94	-0,04	0,07	0,18	-1,12	-0,02	0,01	-0,06	-0,31	0,22	0,12	0,03	0,01	-0,39	-0,12	0,00	-0,01	-0,02	0,17	-0,18	0,02	-0,34	-0,01	-0,15	0,05	-0,10	-0,05
GW	0,60	-0,01	0,76	-0,69	-0,21	0,01	0,71	1,47	0,06	-0,34	-0,67	1,48	0,00	-0,03	0,10	0,62	-0,09	0,41	0,00	0,34	-0,35	0,21	-0,05	0,11	0,24	-0,24	0,26	-0,10	0,46	0,12	0,44	-0,08	-0,01	-0,73
GY	-0,08	0,00	-0,38	0,04	0,10	0,01	-0,05	-0,54	-0,02	0,05	0,14	-0,76	-0,02	0,02	0,01	-0,22	0,28	-0,11	0,02	0,01	-0,29	-0,05	0,00	0,03	-0,07	0,04	-0,01	-0,01	-0,23	0,05	0,01	0,05	-0,03	-0,35
HN	-0,10	0,00	-0,30	0,09	0,07	0,00	-0,15	-0,47	-0,01	0,00	0,02	-0,50	-0,01	0,00	-0,07	-0,04	0,02	0,36	0,02	0,04	-0,23	-0,04	0,01	-0,01	0,11	0,14	-0,22	0,03	-0,14	-0,02	-0,11	-0,01	-0,08	0,01
IQ	0,01	-0,01	0,03	0,04	0,00	0,01	-0,02	0,00	0,01	0,01	0,00	-0,01	-0,01	0,00	0,00	0,01	0,05	0,05	0,01	-0,02	0,03	0,01	0,01	0,01	0,03	-0,01	-0,02	0,00	0,01	0,01	0,01	-0,01	0,02	-0,02
IS	0,03	0,01	-0,07	-0,11	0,00	-0,01	0,09	0,04	-0,01	-0,03	-0,01	-0,01	0,01	0,00	0,02	-0,02	0,00	-0,14	-0,01	0,05	-0,10	0,00	-0,02	0,01	-0,07	-0,02	0,07	-0,02	-0,03	0,02	0,04	0,01	-0,02	-0,18
IT	0,07	-0,06	0,42	0,58	0,05	0,15	-0,37	0,08	0,12	0,29	0,26	0,00	-0,10	0,08	0,09	0,13	0,53	0,14	0,07	-0,41	0,88	0,17	0,17	0,11	0,23	-0,20	-0,09	0,04	0,17	0,15	0,18	-0,07	0,44	-0,24
JM	-0,21	0,00	-0,76	0,06	0,16	-0,01	-0,18	-1,00	-0,05	0,03	0,17	-1,21	0,00	0,01	-0,06	-0,27	0,16	0,17	0,03	0,08	-0,53	-0,11	-0,01	0,00	-0,02	0,19	-0,22	0,02	-0,40	0,01	-0,14	0,04	-0,15	-0,32
KE	0,07	-0,01	0,26	0,15	0,01	0,06	0,05	0,18	0,03	0,16	0,19	-0,02	-0,04	0,04	0,13	-0,17	0,44	-0,64	0,01	-0,20	0,35	0,04	0,04	0,07	-0,17	-0,22	0,31	-0,05	0,04	0,11	0,22	0,05	0,23	-0,26
KH	0,01	0,00	0,07	0,02	-0,01	0,01	0,01	0,02	0,01	0,00	-0,03	0,02	-0,01	0,00	0,00	0,00	0,04	0,05	0,01	0,00	-0,01	0,00	0,00	0,00	0,03	0,00	0,00	0,00	0,02	0,00	0,01	0,00	0,00	0,06
LA	0,06	0,00	0,12	-0,03	-0,02	0,01	0,06	0,16	0,01	-0,01	-0,04	0,15	-0,01	0,00	0,02	0,04	0,04	-0,01	0,00	0,00	0,02	0,02	0,00	0,02	0,01	-0,04	0,05	-0,01	0,06	0,02	0,06	0,00	0,03	-0,06
LS	-0,30	0,00	-0,70	0,32	0,22	0,02	-0,27	-1,59	-0,06	0,06	0,07	-1,92	-0,07	-0,02	-0,14	-0,52	0,57	0,61	0,08	0,10	-1,02	-0,25	-0,01	-0,02	0,15	0,36	-0,35	0,02	-0,51	-0,05	-0,24	0,10	-0,26	0,24
MA	0,04	0,00	0,02	0,00	0,01	0,02	0,04	0,00	0,01	0,01	0,00	-0,08	-0,01	0,01	0,03	-0,02	0,14	-0,03	0,01	0,00	-0,05	0,01	0,01	0,03	0,00	-0,03	0,04	-0,01	-0,01	0,04	0,07	0,01	0,03	-0,16
MG	0,02	0,01	0,04	-0,08	-0,01	-0,01	0,12	-0,01	-0,01	-0,05	-0,09	-0,07	0,00	-0,02	0,00	-0,07	0,08	-0,01	0,00	0,07	-0,25	-0,04	-0,03	0,00	-0,02	0,02	0,07	-0,03	-0,02	-0,01	0,02	0,03	-0,07	0,05
MK	0,02	0,00	0,05	0,04	0,00	0,01	0,00	-0,01	0,01	0,01	-0,01	-0,04	-0,01	0,00	0,01	-0,01	0,09	0,03	0,01	-0,02	-0,01	0,00	0,01	0,01	0,02	-0,01	0,01	0,00	0,01	0,01	0,02	0,00	0,02	-0,01
ML	-0,06	0,00	-0,11	0,03	0,02	-0,01	-0,07	-0,12	-0,01	0,03	0,08	-0,11	0,01	0,01	-0,01	-0,04	-0,05	-0,06	-0,01	-0,03	0,05	-0,01	0,00	-0,01	-0,04	0,02	-0,03	0,01	-0,05	-0,01	-0,05	0,00	0,00	0,01
MM	0,04	-0,01	0,12	0,03	-0,01	0,02	0,01	0,11	0,02	0,02	0,00	0,11	-0,01	0,01	0,02	0,03	0,06	-0,02	0,00	-0,04	0,10	0,02	0,01	0,01	0,02	-0,04	0,03	0,00	0,05	0,02	0,04	-0,01	0,05	-0,02
MR	-0,09	-0,03	-0,03	0,37	0,07	0,06	-0,30	-0,26	0,04	0,21	0,30	-0,31	-0,04	0,05	0,03	-0,08	0,25	-0,13	0,02	-0,26	0,50	0,04	0,09	0,04	0,01	-0,06	-0,07	0,04	-0,04	0,06	0,01	-0,01	0,21	-0,08
MT	0,31	0,01	0,69	-0,35	-0,20	-0,02	0,36	1,37	0,04	-0,10	-0,18	1,59	0,05	0,00	0,11	0,40	-0,38	-0,46	-0,06	0,00	0,58	0,17	-0,02	0,02	-0,11	-0,29	0,35	-0,05	0,44	0,04	0,24	-0,06	0,16	-0,14
MV	-0,24	0,00	-0,17	0,27	0,01	-0,03	-0,43	-0,12	0,00	0,11	0,28	0,22	0,05	0,01	-0,08	0,09	-0,54	0,00	-0,04	-0,20	0,73	0,01	0,05	-0,09	-0,03	0,08	-0,26	0,10	0,02	-0,11	-0,29	-0,07	0,06	0,53
MW	0,04	0,05	0,25	-0,38	-0,14	-0,11	0,34	0,45	-0,05	-0,26	-0,41	0,69	0,06	-0,09	-0,09	0,08	-0,51	0,13	-0,05	0,29	-0,46	-0,10	-0,13	-0,11	-0,03	0,13	0,09	-0,03	0,14	-0,18	-0,14	0,02	-0,31	0,77
MX	0,23	-0,02	0,25	-0,17	-0,05	0,04	0,19	0,48	0,04	-0,07	-0,18	0,44	-0,02	0,01	0,06	0,26	0,08	0,20	0,02	0,07	-0,01	0,12	0,02	0,07	0,13	-0,12	0,05	-0,03	0,16	0,09	0,21	-0,05	0,08	-0,48
MZ	0,00	0,00	0,00	0,01	-0,01	0,00	-0,05	0,11	0,01	0,02	0,06	0,16	0,01	0,01	0,01	0,06	-0,10	-0,09	-0,01	-0,05	0,22	0,04	0,01	0,00	-0,03	-0,04	0,00	0,01	0,03	0,01	0,00	-0,02	0,05	-0,06
NE	-0,34	-0,04	-0,35	0,84	0,21	0,11	-0,72	-0,99	0,04	0,47	0,75	-1,16	-0,07	0,10	0,04	-0,39	0,55	-0,42	0,04	-0,55	0,89	-0,01	0,16	0,04	-0,09	-0,04	-0,16	0,08	-0,26	0,08	-0,10	0,02	0,37	-0,03
NG	-0,02	0,01	-0,10	-0,10	0,01	-0,02	0,10	-0,22	-0,03	-0,08	-0,13	-0,28	0,00	-0,03	-0,04	-0,10	0,06	0,16	0,01	0,14	-0,47	-0,08	-0,04	-0,02	0,02	0,10	-0,02	-0,02	-0,09	-0,03	-0,04	0,04	-0,15	0,10
NI	0,00	0,02	-0,17	-0,19	0,01	-0,03	0,17	-0,08	-0,04	-0,06	-0,03	-0,19	0,02	-0,01	0,02	-0,13	0,03	-0,23	-0,01	0,12	-0,34	-0,05	-0,05	0,00	-0,14	0,01	0,13	-0,04	-0,11	0,01	0,03	0,05	-0,10	-0,17
PA	-0,22	0,01	-0,68	0,04	0,13	-0,03	-0,14	-0,96	-0,06	0,01	0,12	-1,14	0,00	0,00	-0,07	-0,32	0,13	0,12	0,02	0,10	-0,59	-0,14	-0,03	-0,03	-0,05	0,21	-0,17	0,01	-0,38	-0,03	-0,17	0,06	-0,19	-0,07
PE	0,01	-0,01	0,30	0,19	-0,01	0,04	-0,02	0,15	0,03	0,13	0,14	0,09	-0,03	0,02	0,08	-0,12	0,26	-0,43	0,00	-0,19	0,37	0,01	0,04	0,02	-0,10	-0,14	0,20	-0,02	0,07	0,04	0,10	0,03	0,17	0,11
PG	0,02	0,00	0,01	0,02	0,01	0,02	0,01	-0,04	0,01	0,02	0,01	-0,10	-0,01	0,01	0,02	-0,03	0,13	-0,01	0,01	-0,01	-0,03	0,01	0,01	0,02	0,01	-0,02	0,02	-0,01	-0,02	0,03	0,05	0,01	0,03	-0,11
PH	-0,10	0,02	-0,21	-0,13	-0,01	-0,07	0,01	-0,12	-0,05	-0,10	-0,07	0,01	0,04	-0,03	-0,08	-0,01	-0,35	0,11	-0,03	0,13	-0,22	-0,07	-0,05	-0,07	-0,03	0,14	-0,10	0,02	-0,06	-0,10	-0,18	0,00	-0,18	0,31
PY	-0,11	0,01	-0,26	0,08	0,08	0,00	-0,03	-0,52	-0,03	0,05	0,10	-0,70	-0,02	0,00	0,00	-0,28	0,27	-0,12	0,02	0,01	-0,32	-0,09	-0,01	0,00	-0,08	0,07	0,02	-0,02	-0,21	0,01	-0,03	0,07	-0,06	-0,03
QA	0,01	0,00	0,09	0,03	-0,01	0,00	-0,02	0,05	0,01	-0,02	-0,07	0,11	0,00	-0,01	-0,02	0,06	-0,04	0,19	0,01	0,01	0,00	0,00	0,00	-0,01	0,08	0,03	-0,06	0,01	0,05	-0,02	-0,03	-0,02	-0,02	0,15
QT	0,08	0,00	-0,03	-0,09	0,01	0,01	0,10	-0,06	0,00	-0,06	-0,13	-0,17	-0,02	-0,01	0,00	0,02	0,15	0,25	0,02	0,11	-0,34	0,00	-0,01	0,03	0,10	0,02	-0,03	-0,02	-0,03	0,03	0,07	0,00	-0,05	-0,23
RU	0,25	-0,01	0,62	-0,11	-0,14	0,03	0,17	1,03	0,06	-0,02	-0,11	1,20	0,01	0,01	0,09	0,37	-0,16	-0,17	-0,02	-0,09	0,62	0,18	0,03	0,04	0,03	-0,24	0,19	-0,02	0,38	0,06	0,21	-0,07	0,21	-0,15
RW	-0,11	-0,02	-0,22	0,34	0,13	0,07	-0,24	-0,69	0,02	0,15	0,19	-0,92	-0,06	0,03	0,00	-0,22	0,50	0,15	0,06	-0,12	-0,10	-0,04	0,06	0,05	0,08	0,05	-0,13	0,02	-0,20	0,06	0,00	0,03	0,07	-0,15
SC	-0,04	0,01	0,00	0,00	-0,01	-0,02	-0,01	-0,04	-0,01	-0,01	-0,02	0,00	0,01	-0,01	-0,02	-0,03	-0,06	0,00	-0,01	0,01	-0,04	-0,03	-0,01	-0,03	-0,01	0,04	-0,01	0,00	-0,01	-0,04	-0,05	0,01	-0,04	0,19
SD	0,02	0,00	0,11	0,02	-0,02	0,00	-0,01	0,14	0,01	0,01	0,00	0,19	0,00	0,00	0,01	0,05	-0,04	-0,02	0,00	-0,03	0,14	0,02	0,01	0,00	0,01	-0,03	0,01	0,00	0,06	0,00	0,01	-0,01	0,04	0,06
SI	0,23	-0,01	0,43	-0,25	-0,15	-0,01	0,14	1,12	0,05	-0,07	-0,10	1,42	0,05	0,01	0,06	0,50	-0,53	-0,12	-0,05	-0,03	0,70	0,21	0,02	0,01	0,01	-0,21	0,10	0,01	0,38	0,03	0,13	-0,11	0,16	-0,23
SL	-0,07	0,02	-0,03	-0,07	-0,03	-0,05	0,03	0,02	-0,03	-0,04	-0,01	0,11	0,03	-0,02	-0,03	-0,07	-0,21	-0,17	-0,03	0,04	-0,03	-0,06	-0,04	-0,06	-0,11	0,05	0,05	0,00	-0,01	-0,08	-0,10	0,02	-0,09	0,34
SN	-0,08	0,03	0,00	-0,10	-0,04	-0,06	0,07	0,00	-0,04	-0,09	-0,10	0,13	0,03	-0,04	-0,06	-0,07	-0,26	-0,03	-0,03	0,10	-0,19	-0,09	-0,06	-0,08	-0,07	0,10	0,02	0,00	0,00	-0,11	-0,14	0,03	-0,16	0,52
SR	-0,15	0,01	-0,71	-0,04	0,13	-0,02	-0,08	-0,87	-0,06	-0,01	0,10	-1,10	0,00	0,01	-0,05	-0,25	0,16	0,15	0,02	0,13	-0,64	-0,10	-0,03	0,01	-0,03	0,17	-0,16	0,00	-0,37	0,01	-0,10	0,05	-0,17	-0,38
SS	0,00	0,00	0,03	0,03	0,00	0,01	-0,01	-0,02	0,00	0,01	0,00	-0,03	-0,01	0,00	0,00	-0,01	0,04	0,03	0,01	-0,01	0,01	0,00	0,01	0,00	0,02	0,00	-0,01	0,00	0,00	0,00	0,00	0,00	0,01	0,03
ST	0,57	0,00	1,05	-0,82	-0,44	-0,08	0,47	3,06	0,08	-0,24	-0,24	3,88	0,18	0,02	0,17	1,26	-1,59	-0,70	-0,17	-0,04	1,83	0,50	0,00	-0,01	-0,17	-0,57	0,41	0,01	0,98	0,04	0,32	-0,27	0,36	-0,54
SV	-0,45	0,06	-1,53	-0,48	0,10	-0,21	-0,19	-1,11	-0,19	-0,27	0,02	-0,83	0,16	-0,06	-0,28	-0,07	-1,17	0,48	-0,08	0,45	-0,91	-0,21	-0,16	-0,20	-0,14	0,54	-0,55	0,10	-0,53	-0,26	-0,63	-0,02	-0,62	0,18
SY	0,03	0,00	0,07	0,00	-0,01	0,01	0,01	0,08	0,01	-0,01	-0,03	0,09	0,00	0,00	0,00	0,05	0,01	0,07	0,00	0,00	0,02	0,02	0,01	0,01	0,04	-0,02	-0,01	0,00	0,04	0,01	0,02	-0,01	0,02	-0,02
TD	0,09	0,00	0,18	-0,06	-0,04	0,01	0,07	0,29	0,02	-0,03	-0,08	0,33	0,00	0,00	0,02	0,13	-0,04	0,07	0,00	0,01	0,08	0,05	0,01	0,01	0,05	-0,05	0,03	-0,01	0,11	0,02	0,06	-0,02	0,04	-0,05
TG	0,04	-0,01	0,12	0,09	0,02	0,03	0,03	-0,12	0,02	0,01	-0,07	-0,23	-0,04	0,00	0,01	-0,07	0,29	0,17	0,03	0,00	-0,20	-0,02	0,01	0,03	0,09	0,01	0,00	-0,02	-0,01	0,02	0,05	0,02	0,00	0,04
TJ	0,01	0,00	0,06	0,00	-0,01	0,00	0,01	0,07	0,00	0,00	-0,01	0,09	0,00	0,00	0,00	0,02	-0,01	-0,01	0,00	-0,01	0,05	0,01	0,00	0,00	0,00	-0,01	0,02	0,00	0,03	0,00	0,01	0,00	0,01	0,04
TL	-0,01	0,00	0,08	0,05	-0,01	0,00	-0,03	0,06	0,01	0,02	0,02	0,10	0,00	0,00	0,00	0,01	-0,02	-0,03	0,00	-0,04	0,12	0,00	0,01	-0,01	0,00	-0,01	0,01	0,01	0,04	-0,01	-0,01	0,00	0,03	0,12
TN	-0,06	0,00	-0,08	0,05	0,02	-0,01	-0,05	-0,17	-0,01	0,01	0,04	-0,17	0,00	0,00	-0,02	-0,07	0,00	0,01	0,00	-0,01	-0,05	-0,03	0,00	-0,02	-0,01	0,04	-0,04	0,01	-0,05	-0,02	-0,06	0,01	-0,03	0,11
TT	-0,24	0,00	-0,60	0,19	0,15	0,00	-0,22	-1,00	-0,05	0,08	0,19	-1,19	-0,02	0,01	-0,06	-0,34	0,25	0,10	0,03	0,01	-0,43	-0,13	0,00	-0,01	-0,03	0,19	-0,18	0,02	-0,36	-0,02	-0,16	0,06	-0,12	-0,02
UA	0,01	0,01	-0,03	-0,10	-0,01	-0,02	0,09	-0,03	-0,02	-0,07	-0,10	-0,05	0,00	-0,02	-0,02	-0,01	-0,03	0,11	0,00	0,10	-0,26	-0,03	-0,03	-0,01	0,02	0,05	0,00	-0,01	-0,02	-0,02	-0,02	0,01	-0,09	0,05
UY	-0,10	-0,01	-0,38	0,08	0,09	0,01	-0,13	-0,41	-0,01	0,10	0,24	-0,54	0,00	0,04	0,03	-0,15	0,14	-0,25	0,00	-0,07	0,05	-0,01	0,02	0,03	-0,11	-0,01	-0,01	0,01	-0,19	0,06	0,00	0,02	0,06	-0,38

Synthetic Controls, Wealth Bottom 50%

UNTREATED COUNTRIES (ROWS) / TREATED COUNTRIES (COLUMNS)	AE	BH	BR	BT	CA	CR	CY	CZ	DZ	EE	FI	GB	GR	HR	HU	ID	IL	IN	JO	KZ	LK	LT	LV	MN	MU	NL	NO	NP	NZ	PL	RO	SA	SE	TR
AL	0,00	0,00	0,03	0,00	0,01	0,01	-0,02	0,01	0,01	-0,01	0,04	0,01	0,01	0,01	0,00	0,02	-0,02	0,03	0,00	-0,01	0,06	0,02	0,01	0,01	0,00	-0,04	-0,06	0,01	0,01	0,01	0,00	0,01	0,02	-0,01
AM	0,01	0,00	-0,01	-0,01	0,00	0,00	0,01	0,00	0,00	0,00	-0,02	0,00	0,00	0,00	0,00	0,00	0,00	0,01	0,00	0,01	-0,02	0,00	0,00	0,00	0,00	0,01	0,00	0,00	0,00	0,00	0,00	0,00	-0,01	-0,02
AO	-0,16	0,06	0,25	0,17	0,04	0,07	-0,35	0,08	0,13	-0,10	0,21	0,22	0,09	0,10	0,02	0,14	-0,35	0,25	-0,03	-0,13	0,77	0,16	0,14	0,04	-0,08	-0,33	-0,59	0,15	0,06	0,04	-0,14	0,15	0,20	0,44
AT	0,03	-0,02	0,09	0,02	-0,01	-0,01	0,11	-0,04	-0,04	0,09	0,01	-0,08	-0,03	-0,02	0,01	-0,09	0,20	-0,19	0,06	0,01	-0,18	-0,07	-0,04	-0,02	0,03	0,01	0,30	-0,05	-0,02	-0,01	0,04	-0,03	-0,05	0,10
BE	-0,02	0,01	0,00	0,00	0,01	0,01	-0,07	0,02	0,03	-0,03	0,08	0,00	0,02	0,02	0,01	0,04	-0,08	0,03	-0,01	-0,02	0,14	0,04	0,03	0,01	-0,02	-0,06	-0,12	0,03	0,01	0,01	-0,02	0,03	0,04	-0,03
BF	-0,15	0,01	-0,29	0,15	-0,03	-0,07	-0,03	-0,07	-0,06	0,01	-0,36	-0,03	0,01	-0,04	-0,03	-0,15	-0,15	-0,32	-0,08	0,05	-0,02	-0,11	-0,07	-0,08	-0,21	0,20	0,24	-0,02	-0,06	-0,07	-0,16	-0,02	-0,09	0,56
BG	-0,02	0,01	0,02	0,00	0,01	0,02	-0,07	0,02	0,03	-0,02	0,10	0,00	0,02	0,02	0,01	0,04	-0,06	0,03	-0,01	-0,02	0,15	0,04	0,03	0,01	-0,02	-0,07	-0,11	0,03	0,01	0,02	-0,02	0,03	0,05	-0,02
BI	-0,02	0,00	0,01	0,03	0,00	0,00	-0,03	0,01	0,01	-0,02	-0,04	0,06	0,01	0,00	0,00	0,02	-0,05	0,07	-0,01	-0,01	0,06	0,01	0,01	0,00	0,00	-0,01	-0,09	0,02	0,01	0,00	-0,02	0,01	0,01	0,07
BJ	-0,04	0,01	-0,27	-0,01	-0,01	-0,01	-0,10	0,04	0,03	-0,14	-0,32	0,22	0,02	-0,01	-0,04	0,10	-0,33	0,36	-0,12	0,03	0,09	0,05	0,03	-0,01	-0,03	0,15	-0,42	0,05	0,02	-0,02	-0,07	0,01	0,00	-0,04
BO	-0,04	-0,01	-0,29	0,01	-0,03	-0,07	0,08	-0,07	-0,08	0,04	-0,20	-0,15	-0,01	-0,05	-0,02	-0,14	0,01	-0,35	-0,04	0,07	-0,23	-0,11	-0,08	-0,06	-0,13	0,21	0,36	-0,06	-0,07	-0,05	-0,06	-0,06	-0,11	0,12
BW	0,01	0,00	0,06	0,01	0,00	0,00	0,01	0,00	0,00	0,02	0,05	-0,02	0,00	0,00	0,01	-0,01	0,05	-0,04	0,02	-0,01	0,00	0,00	0,00	0,00	0,01	-0,04	0,05	0,00	0,00	0,00	0,01	0,00	0,01	0,03
BY	0,02	0,00	0,01	-0,03	0,00	0,00	0,01	0,00	0,00	0,00	0,04	-0,02	0,00	0,00	0,00	0,01	0,03	0,00	0,01	0,00	-0,03	0,00	0,00	0,01	0,01	-0,01	0,01	0,00	0,00	0,01	0,02	0,00	0,00	-0,08
CF	0,24	-0,06	-0,28	-0,27	-0,04	-0,03	0,27	0,02	-0,05	-0,06	-0,31	0,07	-0,08	-0,07	-0,04	0,07	0,13	0,41	-0,04	0,13	-0,68	-0,03	-0,07	0,02	0,23	0,34	-0,04	-0,09	0,01	-0,01	0,19	-0,12	-0,14	-0,86
CH	-0,01	0,01	-0,03	-0,01	0,00	0,01	-0,05	0,02	0,02	-0,04	-0,02	0,07	0,01	0,01	-0,01	0,05	-0,09	0,13	-0,03	0,00	0,08	0,03	0,02	0,01	0,01	-0,01	-0,17	0,02	0,01	0,00	-0,01	0,01	0,02	-0,05
CI	-0,10	0,03	0,22	0,11	0,03	0,03	-0,13	-0,01	0,03	0,06	0,33	-0,12	0,04	0,05	0,04	-0,04	0,01	-0,31	0,05	-0,08	0,37	0,02	0,04	0,01	-0,11	-0,25	0,09	0,05	-0,01	0,03	-0,07	0,07	0,09	0,36
CL	0,00	0,00	0,03	0,01	0,00	0,00	0,01	0,00	0,00	0,01	-0,01	0,01	0,00	0,00	0,00	-0,01	0,03	-0,01	0,01	0,00	-0,02	-0,01	0,00	0,00	0,01	0,00	0,03	0,00	0,00	0,00	0,01	0,00	0,00	0,04
CM	-0,05	0,01	-0,07	0,03	0,00	0,00	-0,07	0,01	0,02	-0,05	-0,09	0,08	0,02	0,01	-0,01	0,03	-0,15	0,09	-0,04	0,00	0,12	0,02	0,02	-0,01	-0,04	0,02	-0,16	0,03	0,01	-0,01	-0,05	0,02	0,01	0,10
CV	-0,12	0,02	0,03	0,11	0,01	-0,03	-0,03	-0,07	-0,04	0,11	0,19	-0,27	0,02	0,01	0,03	-0,16	0,09	-0,61	0,04	-0,02	0,13	-0,07	-0,03	-0,04	-0,19	-0,09	0,44	-0,02	-0,07	-0,01	-0,10	0,01	0,00	0,43
DE	0,05	-0,01	0,04	-0,06	0,00	0,01	0,04	0,01	0,00	0,01	0,10	-0,05	-0,01	0,00	0,01	0,01	0,08	0,00	0,02	0,00	-0,07	0,01	0,00	0,02	0,04	-0,02	0,04	-0,01	0,00	0,02	0,05	-0,01	0,00	-0,19
DO	0,00	-0,01	-0,06	-0,01	-0,01	-0,03	0,07	-0,04	-0,04	0,06	0,03	-0,16	-0,01	-0,02	0,01	-0,09	0,11	-0,28	0,02	0,02	-0,13	-0,06	-0,04	-0,02	-0,05	0,04	0,29	-0,04	-0,04	-0,01	0,00	-0,03	-0,05	0,02
ES	0,06	-0,02	-0,08	-0,02	-0,02	-0,01	0,07	0,01	-0,01	-0,04	-0,30	0,18	-0,03	-0,03	-0,03	0,03	-0,03	0,31	-0,03	0,04	-0,22	-0,01	-0,02	-0,01	0,10	0,16	-0,14	-0,02	0,03	-0,03	0,04	-0,04	-0,06	-0,10
ET	0,01	0,00	0,03	0,00	0,00	0,01	-0,01	0,01	0,01	-0,01	0,02	0,02	0,00	0,00	0,00	0,02	0,00	0,05	0,00	-0,01	0,02	0,01	0,01	0,01	0,02	-0,02	-0,05	0,01	0,01	0,01	0,01	0,01	0,01	-0,02
FR	0,03	-0,01	0,00	-0,01	-0,01	0,00	0,04	0,01	0,00	-0,01	-0,11	0,09	-0,01	-0,01	-0,01	0,02	0,01	0,14	-0,01	0,01	-0,09	0,00	-0,01	0,00	0,06	0,05	-0,06	-0,01	0,02	-0,01	0,03	-0,01	-0,02	-0,04
GE	0,01	0,00	0,03	-0,01	0,00	0,01	-0,02	0,02	0,01	-0,02	0,02	0,03	0,00	0,01	0,00	0,03	-0,02	0,09	0,00	-0,01	0,03	0,02	0,02	0,01	0,03	-0,03	-0,09	0,01	0,01	0,01	0,01	0,01	0,02	-0,06
GH	-0,03	0,02	0,02	-0,01	0,02	0,02	-0,07	0,01	0,02	-0,01	0,16	-0,06	0,02	0,03	0,01	0,02	-0,05	-0,06	0,00	-0,02	0,17	0,04	0,03	0,01	-0,05	-0,09	-0,05	0,03	0,00	0,02	-0,02	0,03	0,05	-0,02
GN	-0,10	0,02	0,01	0,11	0,00	-0,01	-0,09	-0,02	0,00	0,01	-0,01	-0,01	0,03	0,01	0,00	-0,04	-0,09	-0,16	-0,01	-0,03	0,19	-0,01	0,01	-0,02	-0,11	-0,05	0,03	0,03	-0,01	-0,01	-0,09	0,03	0,02	0,36
GQ	-0,01	0,01	-0,08	-0,05	0,01	0,00	-0,04	0,01	0,01	-0,03	0,08	-0,06	0,02	0,01	0,01	0,03	-0,07	-0,02	-0,03	0,00	0,07	0,03	0,02	0,01	-0,04	-0,01	-0,06	0,02	-0,01	0,01	-0,02	0,01	0,02	-0,14
GT	0,00	-0,01	-0,06	-0,02	0,00	-0,02	0,06	-0,04	-0,04	0,05	0,06	-0,16	-0,01	-0,01	0,01	-0,08	0,10	-0,28	0,02	0,02	-0,11	-0,05	-0,04	-0,02	-0,06	0,03	0,27	-0,04	-0,04	-0,01	0,00	-0,02	-0,04	-0,01
GW	0,38	-0,09	0,10	-0,26	-0,04	0,02	0,39	0,06	-0,03	-0,03	-0,36	0,31	-0,13	-0,08	-0,04	0,14	0,35	0,85	0,05	0,09	-0,85	-0,01	-0,05	0,05	0,51	0,27	-0,18	-0,11	0,09	0,00	0,35	-0,13	-0,13	-0,94
GY	0,02	-0,01	-0,20	-0,11	0,00	-0,03	0,06	-0,03	-0,04	0,02	0,11	-0,24	0,00	-0,01	0,01	-0,05	0,06	-0,28	-0,01	0,05	-0,15	-0,04	-0,04	-0,01	-0,08	0,07	0,25	-0,04	-0,05	0,01	0,01	-0,04	-0,04	-0,29
HN	0,10	-0,05	-0,27	-0,08	-0,05	-0,06	0,24	-0,05	-0,09	0,02	-0,42	0,01	-0,07	-0,09	-0,04	-0,09	0,14	0,02	-0,03	0,11	-0,59	-0,12	-0,11	-0,04	0,07	0,35	0,28	-0,10	-0,03	-0,06	0,07	-0,11	-0,18	-0,20
IQ	0,01	0,00	0,11	0,01	0,01	0,01	0,00	0,01	0,01	0,01	0,07	0,01	0,00	0,01	0,01	0,01	0,05	0,02	0,03	-0,02	0,04	0,01	0,01	0,01	0,03	-0,07	-0,01	0,01	0,01	0,01	0,02	0,01	0,02	0,02
IS	0,00	0,01	-0,03	-0,02	0,01	0,00	-0,02	0,01	0,01	-0,01	0,06	-0,04	0,01	0,01	0,00	0,02	-0,03	-0,02	-0,01	0,00	0,05	0,02	0,01	0,01	-0,02	-0,02	-0,03	0,01	0,00	0,01	-0,01	0,01	0,02	-0,08
IT	0,29	-0,07	0,47	-0,09	-0,02	0,06	0,25	0,07	0,02	0,02	-0,15	0,38	-0,10	-0,03	-0,02	0,13	0,36	0,79	0,12	-0,01	-0,47	0,03	0,01	0,06	0,49	0,00	-0,25	-0,05	0,12	0,02	0,30	-0,05	-0,03	-0,43
JM	0,00	-0,01	-0,29	-0,08	-0,02	-0,05	0,09	-0,05	-0,06	0,02	-0,07	-0,21	-0,01	-0,04	-0,01	-0,09	0,04	-0,30	-0,03	0,07	-0,25	-0,08	-0,07	-0,03	-0,10	0,18	0,31	-0,06	-0,06	-0,02	-0,02	-0,06	-0,09	-0,16
KE	-0,03	0,03	0,21	-0,05	0,05	0,05	-0,09	0,01	0,04	0,06	0,64	-0,31	0,04	0,07	0,06	0,01	0,11	-0,38	0,08	-0,07	0,32	0,06	0,05	0,05	-0,09	-0,32	0,14	0,03	-0,02	0,07	0,00	0,06	0,13	-0,15
KH	0,01	0,00	0,06	0,01	0,00	0,01	0,00	0,00	0,00	0,01	0,00	0,03	0,00	0,00	0,00	0,01	0,02	0,04	0,01	-0,01	0,01	0,00	0,00	0,00	0,02	-0,02	-0,02	0,00	0,01	0,00	0,01	0,00	0,01	0,04
LA	0,02	0,00	0,03	-0,01	0,00	0,01	0,00	0,01	0,01	0,00	0,03	0,01	0,00	0,00	0,00	0,02	0,01	0,04	0,01	0,00	0,00	0,01	0,01	0,01	0,02	-0,02	-0,04	0,00	0,01	0,01	0,02	0,00	0,01	-0,05
LS	0,16	-0,08	0,20	0,03	-0,05	-0,05	0,39	-0,10	-0,13	0,21	-0,27	-0,04	-0,11	-0,10	-0,01	-0,21	0,57	-0,19	0,14	0,06	-0,76	-0,20	-0,15	-0,05	0,20	0,20	0,70	-0,16	-0,02	-0,06	0,17	-0,13	-0,20	0,14
MA	0,03	0,00	0,02	-0,04	0,00	0,00	0,03	0,00	-0,01	0,02	0,09	-0,06	-0,01	0,00	0,01	0,00	0,07	-0,05	0,02	0,00	-0,05	0,00	0,00	0,01	0,01	-0,02	0,07	-0,01	0,00	0,01	0,03	-0,01	0,00	-0,12
MG	0,04	0,00	0,06	-0,04	0,01	0,01	0,03	0,00	0,00	0,02	0,14	-0,07	0,00	0,01	0,01	0,00	0,09	-0,06	0,03	-0,01	-0,03	0,01	0,00	0,01	0,02	-0,05	0,07	-0,01	0,00	0,02	0,04	0,00	0,01	-0,14
MK	0,01	0,00	0,04	0,00	0,00	0,01	0,00	0,00	0,00	0,01	0,03	0,00	0,00	0,00	0,00	0,00	0,02	0,01	0,01	-0,01	0,01	0,00	0,00	0,00	0,02	-0,02	0,00	0,00	0,01	0,00	0,01	0,00	0,01	-0,01
ML	-0,04	0,01	-0,04	0,01	0,01	0,00	-0,05	0,00	0,01	-0,01	0,05	-0,04	0,02	0,01	0,00	0,00	-0,07	-0,07	-0,02	-0,01	0,10	0,01	0,01	0,00	-0,06	-0,03	-0,02	0,02	-0,01	0,00	-0,04	0,02	0,02	0,04
MM	0,00	0,00	0,06	0,01	0,01	0,01	-0,02	0,01	0,01	0,00	0,05	0,01	0,00	0,01	0,00	0,01	0,00	0,02	0,01	-0,01	0,05	0,02	0,01	0,01	0,01	-0,05	-0,04	0,01	0,01	0,01	0,00	0,01	0,02	0,01
MR	-0,07	0,03	0,12	0,04	0,03	0,03	-0,13	0,01	0,04	0,00	0,29	-0,09	0,04	0,05	0,03	0,02	-0,06	-0,14	0,02	-0,06	0,33	0,05	0,05	0,02	-0,08	-0,20	-0,05	0,05	0,00	0,03	-0,05	0,06	0,09	0,11
MT	-0,06	0,04	-0,07	-0,02	0,03	0,03	-0,20	0,06	0,08	-0,12	0,12	0,07	0,05	0,05	0,00	0,13	-0,30	0,21	-0,07	-0,03	0,38	0,12	0,08	0,03	-0,06	-0,10	-0,44	0,09	0,02	0,03	-0,07	0,07	0,11	-0,11
MV	-0,34	0,10	-0,20	0,20	0,04	0,01	-0,48	0,03	0,10	-0,14	0,15	0,01	0,14	0,10	0,01	0,06	-0,64	-0,19	-0,14	-0,09	0,96	0,12	0,12	-0,01	-0,39	-0,21	-0,46	0,17	-0,02	0,01	-0,33	0,16	0,18	0,64
MW	-0,03	-0,02	-0,15	0,15	-0,05	-0,06	0,07	-0,03	-0,05	-0,02	-0,68	0,32	-0,04	-0,07	-0,06	-0,06	-0,10	0,26	-0,06	0,06	-0,25	-0,09	-0,06	-0,07	0,04	0,30	-0,04	-0,03	0,01	-0,09	-0,05	-0,05	-0,13	0,47
MX	0,17	-0,04	0,08	-0,14	-0,01	0,02	0,15	0,03	-0,01	0,00	-0,01	0,05	-0,05	-0,02	0,00	0,06	0,18	0,27	0,04	0,03	-0,32	0,01	-0,01	0,03	0,20	0,05	-0,03	-0,04	0,03	0,02	0,16	-0,05	-0,03	-0,48
MZ	-0,06	0,01	-0,10	0,03	0,00	0,00	-0,08	0,01	0,01	-0,04	-0,05	0,03	0,02	0,01	-0,01	0,02	-0,15	0,02	-0,04	0,00	0,13	0,02	0,01	-0,01	-0,07	0,01	-0,12	0,03	0,00	-0,01	-0,06	0,02	0,02	0,09
NE	-0,25	0,08	0,17	0,14	0,07	0,05	-0,36	0,00	0,08	0,03	0,70	-0,31	0,12	0,12	0,07	-0,02	-0,20	-0,62	0,02	-0,14	0,88	0,10	0,11	0,02	-0,33	-0,47	0,01	0,12	-0,04	0,07	-0,21	0,15	0,22	0,48
NG	0,05	-0,02	0,03	-0,01	-0,01	-0,01	0,09	-0,02	-0,03	0,04	-0,06	0,00	-0,03	-0,02	0,00	-0,03	0,12	0,00	0,03	0,02	-0,18	-0,04	-0,03	-0,01	0,06	0,05	0,12	-0,03	0,00	-0,01	0,05	-0,03	-0,04	-0,03
NI	0,04	-0,01	-0,26	-0,12	-0,01	-0,04	0,10	-0,03	-0,05	0,01	-0,02	-0,19	-0,02	-0,03	0,00	-0,05	0,05	-0,21	-0,03	0,07	-0,25	-0,05	-0,05	-0,01	-0,06	0,15	0,24	-0,05	-0,05	-0,01	0,02	-0,05	-0,07	-0,30
PA	0,00	-0,01	-0,17	-0,05	-0,01	-0,04	0,08	-0,04	-0,05	0,04	-0,01	-0,18	-0,01	-0,03	0,00	-0,08	0,07	-0,28	-0,01	0,05	-0,19	-0,06	-0,05	-0,02	-0,07	0,11	0,29	-0,05	-0,05	-0,01	0,00	-0,04	-0,06	-0,09
PE	-0,14	0,05	0,19	0,10	0,04	0,04	-0,22	0,01	0,06	0,01	0,40	-0,10	0,07	0,08	0,04	0,02	-0,12	-0,24	0,02	-0,10	0,55	0,07	0,08	0,02	-0,15	-0,31	-0,08	0,08	0,00	0,04	-0,11	0,10	0,14	0,32
PG	0,02	0,00	0,03	-0,02	0,00	0,01	0,01	0,00	0,00	0,01	0,07	-0,03	0,00	0,01	0,01	0,01	0,03	-0,01	0,01	0,00	0,00	0,01	0,00	0,01	0,01	-0,03	0,01	0,00	0,00	0,01	0,02	0,00	0,01	-0,08
PH	-0,04	0,00	-0,12	0,05	-0,02	-0,03	0,01	-0,03	-0,03	0,00	-0,20	0,03	0,00	-0,02	-0,02	-0,05	-0,05	-0,06	-0,03	0,02	-0,05	-0,04	-0,03	-0,03	-0,05	0,11	0,07	-0,01	-0,02	-0,03	-0,05	-0,02	-0,05	0,20
PY	-0,01	0,00	-0,06	-0,02	0,00	-0,02	0,05	-0,04	-0,04	0,05	0,06	-0,16	-0,01	-0,01	0,01	-0,08	0,09	-0,28	0,02	0,02	-0,10	-0,05	-0,04	-0,02	-0,06	0,03	0,27	-0,03	-0,04	-0,01	-0,01	-0,02	-0,04	0,00
QA	0,00	0,00	0,08	0,05	0,00	0,01	-0,01	0,01	0,01	-0,01	-0,11	0,14	-0,01	0,00	-0,01	0,02	-0,02	0,16	0,00	-0,01	0,02	0,01	0,01	0,00	0,05	0,00	-0,11	0,01	0,03	-0,01	0,00	0,01	0,00	0,14
QT	0,34	-0,11	0,19	-0,13	-0,06	-0,03	0,51	-0,04	-0,11	0,13	-0,44	0,17	-0,16	-0,12	-0,04	-0,07	0,61	0,36	0,13	0,10	-1,06	-0,15	-0,14	-0,01	0,44	0,32	0,40	-0,18	0,04	-0,05	0,32	-0,17	-0,23	-0,43
RU	0,12	-0,02	0,21	-0,03	0,00	0,04	0,07	0,05	0,03	-0,02	-0,08	0,23	-0,03	-0,01	-0,01	0,09	0,10	0,45	0,04	-0,01	-0,13	0,04	0,02	0,03	0,22	-0,02	-0,23	0,00	0,07	0,01	0,12	-0,01	0,01	-0,19
RW	0,03	-0,02	0,14	0,03	-0,01	-0,01	0,10	-0,04	-0,04	0,10	0,06	-0,10	-0,02	-0,02	0,01	-0,09	0,23	-0,23	0,07	0,00	-0,15	-0,07	-0,04	-0,01	0,03	-0,02	0,33	-0,05	-0,02	-0,01	0,04	-0,03	-0,04	0,13
SC	-0,03	0,00	-0,01	0,04	0,00	-0,01	-0,02	-0,01	0,00	0,00	-0,06	0,03	0,01	0,00	0,00	-0,01	-0,04	-0,01	-0,01	0,00	0,04	-0,01	0,00	-0,01	-0,03	0,01	-0,01	0,01	0,00	-0,01	-0,03	0,01	0,00	0,13
SD	-0,02	0,00	-0,01	0,03	0,00	0,00	-0,02	0,00	0,00	-0,01	-0,04	0,03	0,01	0,00	0,00	0,00	-0,04	0,02	-0,01	0,00	0,04	0,00	0,00	0,00	-0,02	0,00	-0,04	0,01	0,00	-0,01	-0,02	0,01	0,00	0,08
SI	-0,05	0,03	-0,06	-0,02	0,02	0,03	-0,16	0,05	0,06	-0,10	0,09	0,06	0,04	0,04	0,00	0,11	-0,24	0,18	-0,06	-0,03	0,31	0,10	0,07	0,03	-0,05	-0,08	-0,36	0,07	0,02	0,03	-0,05	0,05	0,09	-0,10
SL	-0,10	0,02	-0,10	0,07	0,00	-0,01	-0,09	-0,01	0,01	-0,02	-0,07	0,01	0,03	0,01	-0,01	-0,02	-0,15	-0,09	-0,04	-0,01	0,17	0,00	0,01	-0,02	-0,11	0,01	-0,04	0,03	-0,02	-0,02	-0,10	0,02	0,01	0,25
SN	-0,09	0,01	-0,16	0,10	-0,02	-0,03	-0,04	-0,03	-0,02	-0,02	-0,27	0,08	0,01	-0,02	-0,02	-0,05	-0,14	-0,04	-0,06	0,02	0,03	-0,04	-0,02	-0,04	-0,09	0,12	0,00	0,00	-0,02	-0,05	-0,10	0,00	-0,04	0,34
SR	0,06	-0,02	-0,22	-0,13	-0,01	-0,03	0,13	-0,03	-0,05	0,03	0,03	-0,22	-0,02	-0,03	0,00	-0,06	0,11	-0,24	-0,01	0,07	-0,29	-0,06	-0,06	-0,01	-0,04	0,14	0,29	-0,06	-0,05	0,00	0,04	-0,06	-0,08	-0,36
SS	-0,01	0,00	0,01	0,02	0,00	0,00	0,01	-0,01	0,00	0,01	-0,04	0,02	0,00	-0,01	0,00	-0,01	0,00	0,00	0,00	0,00	-0,02	-0,01	-0,01	-0,01	0,00	0,01	0,02	0,00	0,00	-0,01	0,00	0,00	-0,01	0,06
ST	-0,25	0,12	-0,54	-0,09	0,07	0,07	-0,65	0,18	0,22	-0,42	0,20	0,19	0,18	0,14	-0,01	0,40	-1,09	0,57	-0,31	-0,06	1,15	0,34	0,24	0,08	-0,31	-0,17	-1,37	0,27	0,04	0,07	-0,28	0,19	0,29	-0,39
SV	-0,12	0,00	-0,93	-0,06	-0,06	-0,13	0,02	-0,06	-0,09	-0,12	-0,68	-0,08	0,00	-0,09	-0,07	-0,10	-0,40	-0,20	-0,24	0,18	-0,32	-0,12	-0,11	-0,10	-0,28	0,57	0,12	-0,05	-0,11	-0,10	-0,18	-0,09	-0,20	-0,04
SY	0,01	0,00	0,03	0,00	0,00	0,00	0,01	0,01	0,00	0,00	-0,01	0,02	0,00	0,00	0,00	0,01	0,01	0,05	0,01	0,00	-0,02	0,00	0,00	0,00	0,03	0,00	-0,02	0,00	0,01	0,00	0,02	0,00	0,00	-0,03
TD	0,03	-0,01	0,05	-0,01	0,00	0,01	0,02	0,01	0,01	-0,01	-0,02	0,06	-0,01	0,00	0,00	0,03	0,02	0,12	0,01	0,00	-0,03	0,01	0,01	0,01	0,06	0,00	-0,07	0,00	0,02	0,00	0,03	0,00	0,00	-0,06
TG	0,09	-0,03	0,19	0,01	-0,01	0,00	0,15	-0,02	-0,03	0,07	-0,06	0,05	-0,05	-0,03	0,00	-0,04	0,24	0,05	0,07	0,00	-0,26	-0,05	-0,03	0,00	0,14	0,02	0,17	-0,05	0,02	-0,01	0,10	-0,04	-0,05	0,00
TJ	-0,02	0,00	0,02	0,02	0,00	0,00	-0,02	0,00	0,01	0,00	0,00	0,02	0,01	0,00	0,00	0,00	-0,02	0,01	0,00	-0,01	0,05	0,01	0,01	0,00	-0,01	-0,02	-0,03	0,01	0,00	0,00	-0,01	0,01	0,01	0,06
TL	-0,03	0,01	0,03	0,04	0,00	0,00	-0,04	0,00	0,01	0,00	0,01	0,03	0,01	0,01	0,00	0,01	-0,04	0,01	0,00	-0,02	0,09	0,01	0,01	0,00	-0,02	-0,03	-0,05	0,02	0,01	0,00	-0,02	0,02	0,02	0,10
TN	-0,02	0,00	0,00	0,02	0,00	0,00	-0,02	-0,01	0,00	0,00	0,00	0,00	0,01	0,00	0,00	-0,01	-0,02	-0,04	0,00	0,00	0,04	0,00	0,00	-0,01	-0,03	-0,01	0,01	0,00	0,00	0,00	-0,02	0,01	0,00	0,08
TT	-0,01	-0,01	-0,07	-0,02	-0,01	-0,02	0,06	-0,04	-0,04	0,05	0,04	-0,16	-0,01	-0,02	0,01	-0,08	0,09	-0,28	0,01	0,02	-0,11	-0,05	-0,04	-0,02	-0,06	0,04	0,28	-0,04	-0,04	-0,01	-0,01	-0,03	-0,04	0,00
UA	0,02	-0,01	-0,01	-0,01	0,00	0,00	0,03	0,00	-0,01	0,00	-0,04	0,01	-0,01	-0,01	0,00	0,00	0,02	0,03	0,00	0,01	-0,06	-0,01	-0,01	0,00	0,02	0,03	0,02	-0,01	0,00	0,00	0,01	-0,01	-0,02	-0,02
UY	-0,05	0,03	-0,11	-0,07	0,02	0,01	-0,10	0,01	0,02	-0,02	0,29	-0,21	0,04	0,04	0,03	0,02	-0,10	-0,24	-0,02	-0,01	0,22	0,04	0,03	0,02	-0,14	-0,10	0,01	0,03	-0,03	0,03	-0,05	0,03	0,06	-0,18

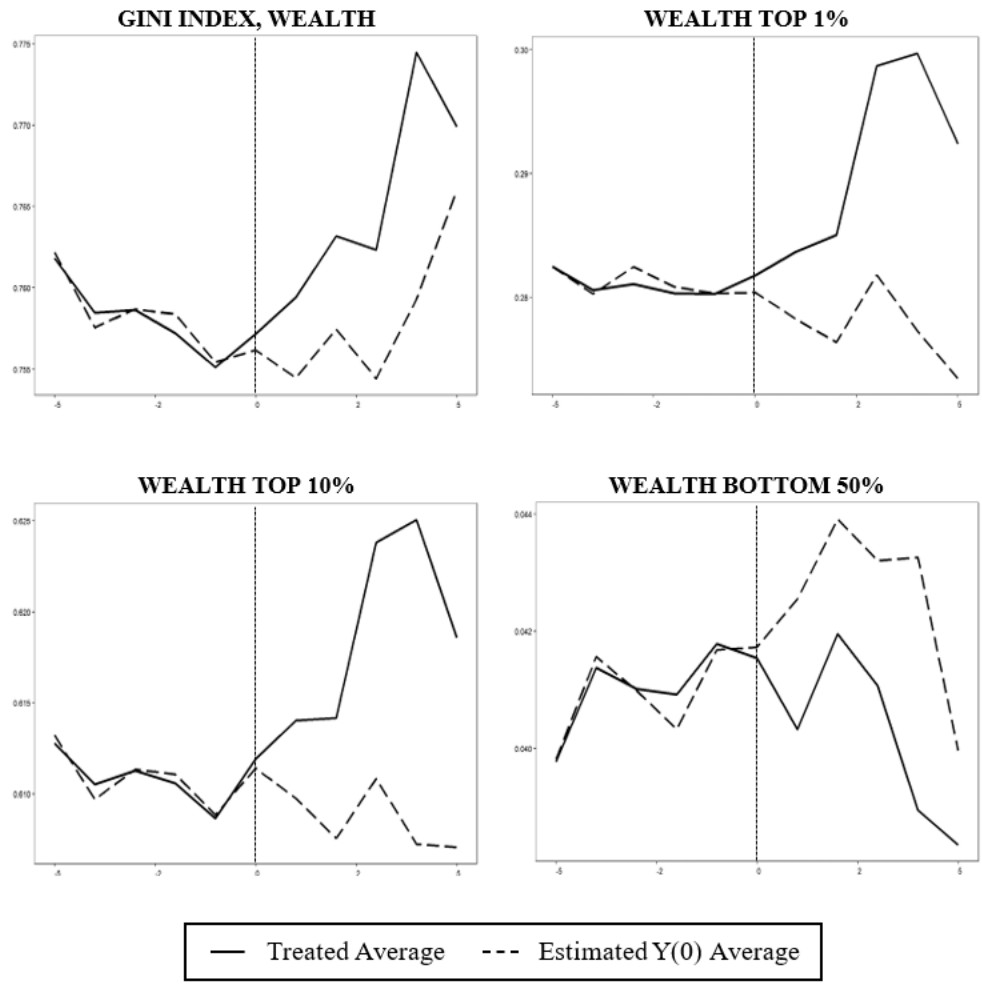
List of Country Codes

TREATED UNITS		CONTROL UNITS	
ID	**NAME**	**ID**	**NAME**
AE	United Arab Emirates	AL	Albania
BH	Bahrain	AM	Armenia
BR	Brazil	AO	Angola
BT	Bhutan	AT	Austria
CA	Canada	BE	Belgium
CR	Costa Rica	BF	Burkina Faso
CY	Cyprus	BG	Bulgaria
CZ	Czech Republic	BI	Burundi
DZ	Algeria	BJ	Benin
EE	Estonia	BO	Bolivia
FI	Finland	BW	Botswana
GB	United Kingdom	BY	Belarus
GR	Greece	CF	Central African Republic
HR	Croatia	CH	Switzerland
HU	Hungary	CI	Cote d'Ivoire
ID	Indonesia	CL	Chile
IL	Israel	CM	Cameroon
IN	India	CV	Cabo Verde
JO	Jordan	DE	Germany
KZ	Kazakhstan	DO	Dominican Republic
LK	Sri Lanka	ES	Spain
LT	Lithuania	ET	Ethiopia
LV	Latvia	FR	France
MN	Mongolia	GE	Georgia
MU	Mauritius	GH	Ghana
NL	Netherlands	GN	Guinea
NO	Norway	GQ	Equatorial Guinea
NP	Nepal	GT	Guatemala
NZ	New Zealand	GW	Guinea-Bissau
PL	Poland	GY	Guyana
RO	Romania	HN	Honduras
SA	Saudi Arabia	IQ	Iraq
SE	Sweden	IS	Iceland
TR	Turkey	IT	Italy
		JM	Jamaica
		KE	Kenya
		KH	Cambodia
		LA	Lao PDR
		LS	Lesotho
		MA	Morocco
		MG	Madagascar
		MK	North Macedonia
		ML	Mali
		MM	Myanmar
		MR	Mauritania
		MT	Malta
		MV	Maldives
		MW	Malawi

MX	Mexico
MZ	Mozambique
NE	Niger
NG	Nigeria
NI	Nicaragua
PA	Panama
PE	Peru
PG	Papua New Guinea
PH	Philippines
PY	Paraguay
QA	Qatar
QT	South Africa
RU	Russian Federation
RW	Rwanda
SC	Seychelles
SD	Sudan
SI	Slovenia
SL	Sierra Leone
SN	Senegal
SR	Suriname
SS	South Sudan
ST	Sao Tome and Principe
SV	El Salvador
SY	Syrian Arab Republic
TD	Chad
TG	Togo
TJ	Tajikistan
TL	Timor-Leste
TN	Tunisia
TT	Trinidad and Tobago
UA	Ukraine
UY	Uruguay

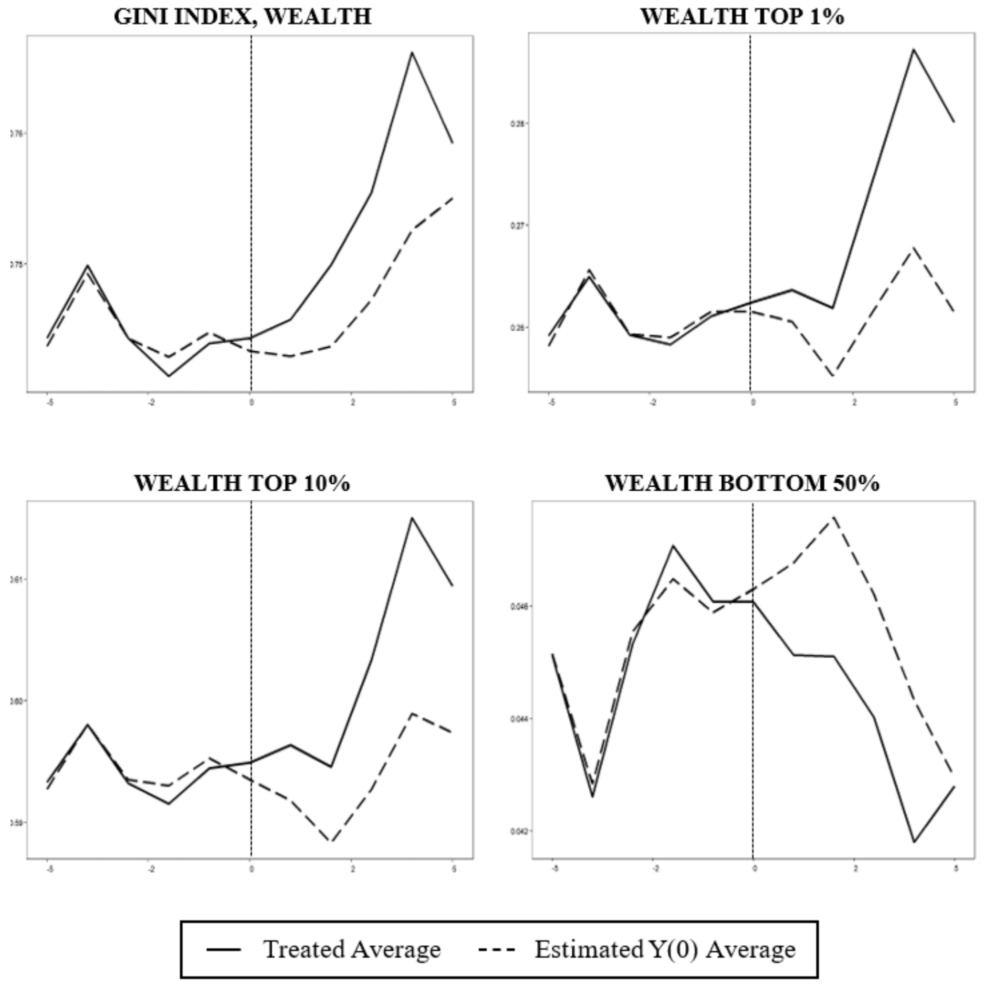
B.3. Robustness Checks

Figure B.3.1: Trends in Wealth Inequality, Alternative Conditioning Factors



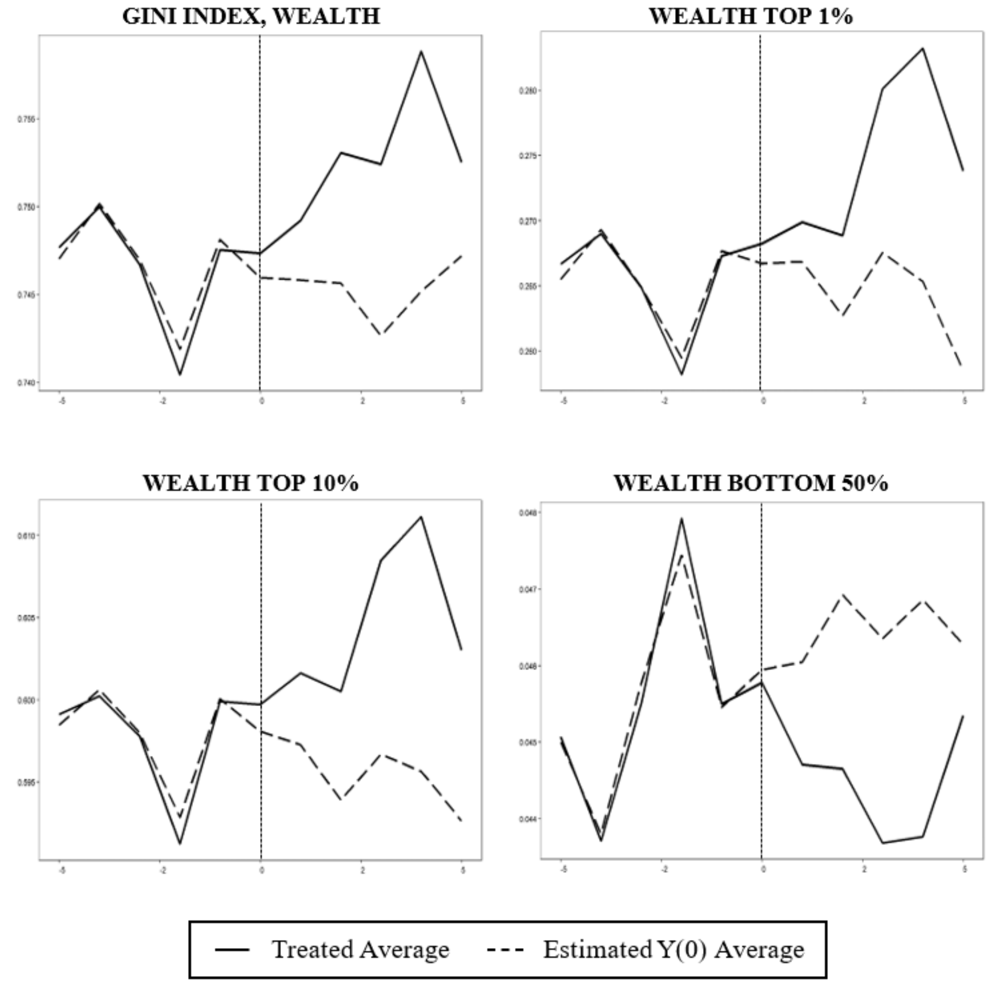
Note: The figure shows the average treatment effect of MaPP on the treated countries (ATT). "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years ($T_0 + 5$) after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include financial development, government expenditure on education, inflation, population growth, and real GDP per capita. The optimal number of factors is selected using cross-validation to minimize the MSPE.

Figure B.3.2: Trends in Wealth Inequality, Fiscal Policy



Note: The figure shows the average treatment effect of MaPP on the treated countries (ATT). "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years ($T_0 + 5$) after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include average education, financial development, government subsidies as a proxy for fiscal policy, inflation, population and real GDP per capita. The optimal number of factors is selected using cross validation to minimize the MSPE.

Figure B.3.3: Trends in Wealth Inequality, Unconventional Monetary Policy



Note: The figure shows the average treatment effect of MaPP on the treated countries (ATT). "Gini Index, Wealth" is the Gini index of wealth concentration. "Wealth" is the wealth share held by the top 1%, top 10%, and bottom 50% groups in each country. The outcome values refer to five years (T_0+5) after the treatment year (T_0). The estimated values based on synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include average education, financial development, forward guidance as a proxy for unconventional monetary policy, inflation, population, and real GDP per capita. The optimal number of factors is selected using cross-validation to minimize the MSPE.

Supplementary Materials for Chapter 4

C.1. Data Description

Data	Type	Source	Description
Bond Spread	Dependent	Datastream	The ten-yearr bond yield of the country relative to the U.S. Treasury bond rate in each quarter.
Bond Yield	Dependent	Datastream	Ten-year government bond yield of the country in each quarter.
CBI	Instrument	Romelli (2022)	Index of central bank independence proposed by Romelli (2022). The index is computed based on a wide range of central bank characteristics in 154 countries. We take the index exactly as published by the author.
CDS	Dependent	Datastream	Ten-year sovereign credit default swap (CDS) spread of the country in each quarter.
Credit-to-GDP	Control	IMF IFS	Domestic credit to the private sector as a percentage of GDP.
Crisis	Control	Laeven and Valencia (2018)	Indicator variable that equals one whenever a country experiences a financial crisis in a given quarter based on the dataset of Laeven and Valencia (2018). This variable accounts for banking crises, sovereign debt crises, and currency crises.
Debt	Independent	IMF IFS	Dummy variable that equals 1 when the country's debt level in the period is above the median debt level of the country during the sample period. To measure the level of debt, we use the debt-to-GDP ratio.
Debt-to-GDP	Control	IMF IFS	Gross public debt as a percent of GDP.
ED	Independent	World Bank Open Data	Dummy variable that equals 1 when a country is classified as an upper-middle or high-income economy according to the World Bank's country classification by income level, and zero otherwise
Forward Guidance	Instrument	Sutherland (2022)	Dummy variable that equals 1 whenever the central bank of the country uses forward guidance during the year.
GDP Growth	Control	IMF IFS	The quarterly growth rate of GDP adjusted for inflation.
Gov. Bonds Net Issues	Dependent	BIS Debt Securities Statistics	The growth rate in the net issue of government bonds (i.e., gross issue minus redemptions).
Gov. Expenditure	Control	IMF Global Debt	Total government expenditure as a percent of GDP.
Inflation	Control	IMF IFS	The quarterly growth rate in Consumer Price Index using the Laspeyres formula.
Openness	Control	Chinn and Itô (2020)	The Chinn-Itô financial openness index, which measures a country's degree of capital account openness. The index was initially introduced by Chinn and Ito (Journal of Development Economics, 2006). The variable is based on binary dummy variables that codify the tabulation of restrictions on cross-border financial transactions reported in the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER). This is the updated version of the index, which contains information on regulatory restrictions on cross-border financial transactions as of the end of 2020. We take the data exactly as published by the authors.
PR	Independent	IMF iMaPP	Overall cumulative macroprudential policy index that sums all the cumulative prudential rules in a country in each quarter. The index ranges from 0 to 32 prudential rules in effect.
Primary Balance	Control	IMF Global Debt	Primary net lending minus borrowing as a percent of GDP.
Public-Private Credit	Dependent	IMF IFS	Ratio of credit to the general government over domestic credit to the private sector.
Sovereign Credit Ratings	Dependent	Bloomberg	Simple average of the sovereign credit ratings of the three main credit rating agencies (Fitch, Moody's, and S&P500). When the credit rating is not available from the three agencies, we calculate the average rating based on the ratings given by the two remaining agencies or rely on a single rating provided by the only agency available.

C.2. Ratings Numerical Scale

Moody's	S&P	Fitch	Numerical Scale
Aaa	AAA	AAA	21
Aa1	AA+	AA+	20
Aa2	AA	AA	19
Aa3	AA-	AA-	18
A1	A+	A+	17
A2	A	A	16
A3	A-	A-	15
Baa1	BBB+	BBB+	14
Baa2	BBB	BBB	13
Baa3	BBB-	BBB-	12
Ba1	BB+	BB+	11
Ba2	BB	BB	10
Ba3	BB-	BB-	9
B1	B+	B+	8
B2	B	B	7
B3	B-	B-	6
Caa1	CCC+	CCC+	5
Caa2	CCC	CCC	4
Caa3	CCC-	CCC-	3
Ca	CC	CC	2
		C	2
C	SD	DDD	1
	D	DD	1
		D	1