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Effects of Capital Flow Management Measures on Wealth Inequality: New Evidence from Counterfactual Estimators

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Abstract

We provide cross-country evidence that variations in capital flow management measures (CFMs) result in differences in wealth inequality and distribution by using counterfactual estimators for causal inference. The imposition of aggregate CFMs increases wealth inequality in advanced economies, and the imposition of aggregate CFMs on outflows increases wealth inequality in emerging economies significantly. Diverging from previous studies, we analyze the impacts of ten distinct asset-specific CFMs. In particular, the imposition of the related CFMs to money market and derivatives reduces wealth inequality significantly. The decrease in wealth inequality is due to a decrease in the wealth shares of the top 1% and 10% groups along with an increase in the wealth shares of the middle 40% and bottom 50%. Overall, the effects of CFMs on wealth inequality and distribution are quite heterogeneous; they depend on income levels, capital flow directions, and asset categories.

Keywords: Capital flow management measures (CFMs), Wealth inequality, Gini coefficient, Wealth distribution, Counterfactual estimator

JEL classification: D63, E21, F38, G15, G28, O16

1 Introduction

Although capital flow management measures (CFMs) (or capital controls) are not a new policy instrument, it is not until the global financial crisis that the potential effects of CFMs have been rigorously examined as one of the most important topics in international economics.[1] During the global financial crisis and its aftermath, policymakers in emerging economies have struggled with unprecedented magnitude in international capital flows. In fact, some emerging economies such as such as Brazil, Indonesia, South Korea, Taiwan, and Thailand have responded to instability by imposing CFMs. As well known, even the International Monetary Fund (IMF), a former critic of CFMs, has been forced to reconsider such measures as an important policy response to volatile capital flows under certain circumstances. Against this background, an increasing number of researchers have extended studies on CFMs in a variety of new directions. A strand of the literature focuses on pecuniary externalities associated with financial crises and provides a rationale for CFMs to prevent excessive borrowing (e.g., Jeanne and Korinek, 2010; Jeanne et al., 2012; Brunnermeier and Sannikov, 2015). More effects of CFMs as a policy tool have been rigorously examined from a broader perspective (e.g., Chang et al., 2015; Kitano and Takaku, 2018; Agénor and Jia, 2020; Kitano and Takaku, 2020; Nispi Landi and Schiavone, 2021). Overall, the theoretical studies tend to emphasize that CFMs are a good policy tool for managing the risk from volatile capital flows, and then CFMs enhance welfare in the capital recipient economies.

However, as argued above, the IMF used to emphasize the desirability of free capital movements among countries. Free international capital mobility benefits both lenders and borrowers owing to the traditional gains from trade (Obstfeld and Rogoff, 2009). Lending countries can earn higher returns to their capital and achieve greater portfolio diversification. Borrowing countries can expect that capital inflows are invested in productive investments, or allow their residents to smooth their consumption. Borrowing countries can also benefit from foreign investment such as foreign direct investment (FDI) as it may lead to technology transfer to the recipient countries. In fact, many empirical studies show

[1] The term "capital flow management measures (CFMs)" refers to policy measures that are specifically designed to limit capital flows (IMF, 2012). Although some prefer the term "CFMs" to "capital controls", the latter is also used by the IMF Article of Agreement for such interventions and they are generally used interchangeably in the related literature (Erten et al., 2021).

that capital flows benefit source and recipient economies (e.g., Desai et al., 2009; Reinhardt et al., 2013).

In examining the advantage and disadvantage of CFMs as a policy tool, it is necessary to take into account how CFMs affect income (or wealth) inequality and distribution. The topic's importance leads to the proliferation of the related literature on the relationship between CFMs (liberalization) and inequality. A priori reasoning for the relationship seems quite difficult as CFMs affect income and wealth distributions through multiple channels. Obviously, CFMs in various asset markets such as FDI, international bank lending, portfolio debt, and equity affect the corresponding capital flows separately, and their effects are not necessarily the same across the various asset markets. In addition, it is not straightforward to understand how the different channels of capital flows influence inequality and distribution. For example, Jaumotte et al. (2013) argue that "[i]f financial flows make resources available to a broader cross-section of the work-force, they would serve to reduce inequality by allowing investment in skills and human capital. However, if they make more financial resources available to those who already have capital and collateral, this would likely exacerbate inequality" (line 23-28, page 285). The aggregate indices of CFMs consist of the various asset-specific CFMs on different capital flows, and the asset specific CFMs affect different groups of income and wealth distribution through different channels. Therefore, it is not an easy task to grasp how CFMs affect inequality in total. However, as we argue in the literature review, most previous studies examine the relationship between CFMs and inequality by using the aggregate (not asset specific) indices of CFMs.

In order to fill the above mentioned gap in the literature, in this paper, we explore the effects of both aggregate and disaggregated CFMs on wealth (not income) inequality using counterfactual estimators for causal inference with a dataset encompassing 100 economies from 1995 to 2019. Using the full sample, we first examine the effects of aggregate CFMs on wealth equality. Our result suggests that tightening aggregate CFMs reduces wealth inequality, which is consistent with the literature on income inequality, but the result is not statistically significant. Separating the full sample into advanced and emerging economies, we obtain a statistically significant result for advanced economies, but the result indicates that tightening aggregate CFMs increases (not reduces) wealth inequality.

The result for advanced economies is consistent with the some previous studies arguing that capital account liberalization decreases income inequality when the economy's financial and institutional level is high. Differentiating further between capital inflows and outflows, we obtain a statistically significant result for emerging economies indicating that tightening aggregate CFMs on outflow increases wealth inequality. The result for aggregate CFMs on outflow in emerging economies seems opposite to the previous literature.

Using the disaggregated indices of CFMs by Fernández et al. (2016b), we next examine the effects of asset-specific CFMs on wealth inequality. We obtain statistically significant results showing that tightening CFMs in the asset markets of money market and derivative decreases wealth inequality, which is consistent with the previous literature. Further differentiation between inflow and outflow yields the statistically significant results showing that tightening asset-specific inflow CFMs reduces wealth inequality in the assets of equity, money market, and direct investment, whereas it increases wealth inequality in the bond market. The result on inflow for the former group (equity, money market, and direct investment) is consistent with the previous literature, while that for the latter (bond) is not. As for outflows, our statistical significant results indicate that tightening CFMs in the derivative market reduces wealth inequality, but tightening CFMs in the asset markets of financial credit, direct investment, and real estate increases wealth inequality. The result on outflow for the former (derivative) is consistent with the previous literature, while the result on outflow for the latter group (financial credit, direct investment, and real estate) is not.

We also explore how CFMs change wealth distribution in order to understand the linkage between CFMs and the wealth inequality (i.e., Gini coefficients). We find that the increase in wealth inequality due to tightening aggregate CFMs in advance economies can be explained by a fall in the share of the bottom 50% group. We also find that the increase in wealth inequality due to tightening aggregate outflow CFMs in emerging economies can be explained by a rise in the wealth share of the top 10% group. As for specific asset markets, we find that tightening CFMs in the money market reduces the wealth shares of the top 1% and 10% groups, but increases those of middle 40% and bottom 50% groups, both of which explain the decrease in wealth inequality due to tightening CFMs in the money market. We also find that tightening CFMs in the derivative market reduces the wealth shares of the top 10% group, but increases that of the bottom 50% group, both of which explain the decrease in wealth inequality due to tightening CFMs in the derivative market.

In summary, differentiating among income levels, capital flow directions, and asset categories, we find that the effects of CFMs on wealth equality and distribution vary considerably. Some of our results are consistent with the previous studies that employ aggregate data, but others are not. Our results suggest that using only aggregate data might not be enough to fully understand how CFMs affect wealth inequality and distribution.

We contributes to the literature in threefold. To the best of our knowledge, this paper is the first to provide empirical evidence on the effects of CFMs on wealth inequality (not income inequality). As we argue in the next section, the previous literature suggests that wealth inequality is a much more serious issue than income inequality, and demonstrates that income distribution cannot perfectly substitute for wealth distribution. However, most of the previous literature focus on income inequality, and only a very few studies analyze wealth inequality. Furthermore, we examine the effects of CFMs on wealth distribution. We show how various CFMs affect the wealth shares of different distribution groups in line with the studies by Li and Su (2021) and Teixeira (2023).

In addition, our study is the first to use the up-to-date counterfactual estimators for causal inference with time-series cross-sectional data proposed by Liu et al. (2024) for establishing causality from CFMs to wealth inequality. Most of the previous literature use the generalized method of moments (GMM) and system GMM estimator for dynamic panels to mitigate the endogeneity of the capital account liberalization variables. In this study, we use the counterfactual estimators rather than GMM. As we explain in detail in the next section, the method of counterfactual estimators differ from traditional ones and more appropriate for establishing causality in our case.

Our third contribution is that we consider not only aggregate CFMs indicators but also those across various specific capital markets. This point is important because policymakers regulate various capital accounts individually rather than the entire capital account collectively. We also make a distinction between capital inflow and outflow managements in the asset specific market because the direction of capital movements is of concern to policy makers, especially in emerging economies.

The remainder of this paper is organized as follows. Section 2 reviews the empirical literature related to income and wealth inequality. Section 3 describes the data on wealth inequality, CFMs, and control variables used in our estimation. Section 4 presents the methodology of the counterfactual estimator to establish causality from CFMs to wealth inequality. Section 5 presents the empirical results on the effects of various types of CFMs on wealth distribution. Section 6 presents a series of robustness exercises. Finally, conclusions are presented in Section 7.

2 Literature review

This paper studies the effects of CFMs on wealth inequality. There is a vast literature on the effects of capital account liberalization on income inequality (not wealth inequality). Previous early studies tend to suggest that capital account liberalization increases income inequality. In most early studies, the relationship between capital account liberalization and income inequality is unconditional. Jaumotte et al. (2013) examine the relationship between the rapid pace of financial globalization and the rise in income inequality in 51 developed and developing countries, and report that financial globalization, particularly in FDI, is associated with an increase in inequality. Asteriou et al. (2014) also investigate the relationship between income inequality and globalization, and suggest that financial globalization through FDI and capital account openness have been the driving force of inequality in the EU27 countries. Batuo and Asongu (2015) apply two methods, the dynamic panel econometric method and the "before and after" approach for African countries, and show that in their first method, financial liberalization tends to escalate income inequality, whereas in their second method, financial liberalization has made considerable progress towards decreasing income inequality. Using a sample of 143 countries, Zhang and Naceur (2019) suggest that financial sector liberalization (both external and domestic) tend to widen the inequality and poverty gap.

Other related studies show that the relationship between capital account liberalization and income inequality is conditional. Furceri and Loungani (2018) investigate three channels through which capital account liberalization increases income inequality. They show that the effect of liberalization on income inequality intensifies when financial inclusion is low, when liberalization precedes a financial crisis, and when there is a shift in the relative bargaining power between firms and workers. Furceri et al. (2017) are similar to Furceri and Loungani (2018), but they focus on low-income countries and find that the effects of capital account liberalization on inequality are more pronounced in countries with underdeveloped credit markets and limited financial inclusion. De Haan and Sturm (2017) and Gallagher et al. (2019) both show that institutional quality may condition the relationship between capital account liberalization and income inequality. Assuming that economic institutional strength is associated with GDP per capita, Gallagher et al. (2019) find that in most developing countries with weak institutions, the lack of preemptive policies means that capital account liberalization is likely to exacerbate income inequality during economic downturns. Utilizing the indicator of the quality of political institutions as a proxy for economic institutional strength, De Haan and Sturm (2017) find that with higher levels of democratic accountability, the positive impact of financial liberalization on inequality intensifies. Bumann and Lensink (2016) suggest that capital account liberalization is likely to reduce income inequality when the ratio of private credit to GDP, which indicates financial depth, exceeds 25 percent.

As we argue above, income inequality has been extensively studied.[2] However, few studies focus on wealth (not income) inequality. As argued by Hasan et al. (2020), the scarcity of empirical research on wealth inequality is mainly due to limitations in wealth inequality data. Most wealth inequality data are based on household surveys. For instance, Carpantier et al. (2018) utilize data from the Eurosystem Household Finance and Consumption Survey (HFCS) to demonstrate the relationship between loan-to-value (LTV) ratios and wealth inequality.[3] Similarly, Hasan et al. (2020) assess wealth inequality using the wealth Gini coefficient derived from the Credit Suisse Wealth Databook (CSWD),

[2]Income inequality has been studied from many other different perspectives. For example, Jatmiko et al. (2023) examine the relationship between Sukūk development and income inequality.

[3]According to Albacete et al. (2012), the Eurosystem Household Finance and Consumption Survey (HFCS) is carried out in a decentralized manner by the central bank or statistical institute of each country. The fourth wave of the HFCS was primarily conducted in 2021, the third wave in 2017, the second wave in 2014, and the first wave in 2010. This survey gathers comprehensive data on households' real assets, financial assets, debt, and expenditures, enabling detailed scientific analyses of household balance sheets in accordance with international standards.

which is based on Household Balance Sheet (HBS) data.[4] [5] However, household surveys, which are traditionally used to observe inequality dynamics, fail to accurately capture the evolution of wealth and income, especially at the highest levels. The World Inequality Database (WID) addresses this shortcoming by integrating various data sources, including national accounts, survey data, fiscal data, and wealth rankings. This approach allows for a more precise tracking of income and wealth evolution across all levels, from the bottom to the top (Alvaredo et al., 2016). Therefore, we use the WID data as it is most suitable for our analysis.

CFMs influence wealth inequality through their impact on cross-border capital flows. Macroprudential policies, another policy related to CFMs, affect inequality by directly influencing domestic credit. The literature on the impact of macroprudential policies on inequality is also closely related to this paper. Delis et al. (2014) examine the impact of banking regulations on income distribution across various countries. They find that the general liberalization of banking systems significantly reduces income inequality. Yet, this impact fades and becomes negligible in countries with low levels of economic and institutional development. Frost and Van Stralen (2018) differentiate between the effects of macroprudential policies on market income inequality (income inequality before redistributive policies) and net inequality (inequality after redistribution). In contrast to Delis et al. (2014), their finding reveals that macroprudential policies such as concentration limits, macroprudential reserve requirements, and interbank exposure limits are positively correlated with market income inequality, while LTV limits are positively associated with net inequality. Teixeira (2023) first examines the effects of macroprudential policy on wealth inequality and also analyses the wealth share of the top 1%, top 10%, and bottom 50% of the distribution. In line with Frost and Van Stralen (2018), Teixeira (2023) finds that implementing macroprudential policies causes a 3.4% point increase in wealth concentration within the affected countries over ten years. This outcome can be attributed to an

[4]The Credit Suisse Wealth Databook (CSWD) offers estimates on the level and distribution of wealth for over 200 countries, spanning from the year 2000, using the methodology developed by Davies et al. (2011). It examines the patterns and trends of wealth among individuals aged 20 and above at both regional and national levels. However, as noted by Hasan et al. (2020), the CSWD has been available on an annual basis only from 2010 onwards.

[5]The primary source for the CSWD is the Household Balance Sheet (HBS) data, which are currently provided by 51 countries. However, data on nonfinancial wealth are missing for many of these countries, necessitating supplementation through econometric estimations (Hasan et al., 2020). Of these 51 countries, only 24 provide information on both financial assets and debts.

increase in the wealth share of the top 1% combined with a significant reduction in the wealth share of the bottom 50%.

Unlike monetary policies that can be succinctly described by using quantifiable indicators such as policy interest rates, CFMs are considerably more complex and depend significantly on institutional details. Although CFMs can be categorized into various categories, most previous studies on the impact of CFMs use aggregate (synthetic) indicators. Gallagher et al. (2019), Furceri and Loungani (2018), Furceri et al. (2017), Batuo and Asongu (2015), Bumann and Lensink (2016), and Asteriou et al. (2014) use the aggregate *de jure* indicator of financial openness KAOPEN developed by Chinn and Ito (2006, 2008). Although the earlier literature frequently employ them, aggregate indicators such as KAOPEN can only describe the overall level of capital account openness of a country as a whole, and are not adequate for precisely describing CFMs in individual capital markets and different capital flows. Researches on income inequality using disaggregated indices of CFMs are scarce. Few exceptions are Li and Su (2021) and Jaumotte et al. (2013). However, they only cover equities, bonds, and FDI, and the other asset categories appear in aggregate forms and are not further classified. According to Fernández et al. (2016a)'s categorization, there exist up to ten distinct CFMs. As the effects of such policies employing specific asset categories can differ from those employing aggregate indicators, further analysis utilizing the disaggregated indices is necessary.

The contribution of this paper to the literature is in threefold. Firstly, to the best of our knowledge, this paper is the first to provide empirical evidence on the effects of CFMs on wealth inequality rather than income inequality. Highlighting its higher levels, more pronounced concentration, and much thicker tail compared to income inequality, Saez (2017) and Osakwe and Solleder (2023) argue that wealth inequality is a much more serious issue than income inequality.[6] Compared to wages or earned income, capital income is more susceptible to the effects of CFMs because these policies are implemented to prevent large fluctuations of capital flows. A significant portion of these flows moves into the stock (or bond) market in the form of hot money for portfolio investment, while another portion

[6] Zucman (2019)'s study indicates that the recent history of global wealth is more complex than that of the pre-1980s era, given that global wealth growth rates vary significantly across the distribution. Davies et al. (2011) note that "in all countries with the requisite data, wealth distribution is more unequal than income." Hasan et al. (2020) and Bagchi and Svejnar (2015) demonstrate that income distribution cannot perfectly substitute for wealth distribution.

accelerates the financial cycle through real estate investment. Teixeira (2023) argues that if policies make it difficult for people to obtain a loan or profit from increases in asset prices, wealth inequality is better suited for capturing the true effects of such policies compared to income inequality. However, most of the empirical research focus on income inequality, and only a very few studies analyze wealth inequality. Hasan et al. (2020) investigate the determinants of wealth inequality, capturing various economic, financial, political, institutional, and geographical indicators. However, they do not consider the impact of economic policies on wealth inequality. Teixeira (2023) examines the effects of macroprudential policy on wealth inequality. We are the first to study the effects of CFMs on wealth inequality.[7] Furthermore, we examine not only the effects of CFMs on the Gini coefficient but also the effects of CFMs on different wealth shares of the distribution. We show how these effects vary among these groups in line with Li and Su (2021) and Teixeira (2023).

Secondly, this study is the first to use the up-to-date counterfactual estimators for causal inference with time-series cross-sectional data proposed by Liu et al. (2024) for establishing causality from CFMs to wealth inequality. Most of the previous literature use the generalized method of moments (GMM) and system GMM estimator for dynamic panels developed by Arellano and Bond (1991) and Blundell and Bond (1998) to mitigate the endogeneity of the capital account liberalization variables (Furceri and Loungani, 2018; Bumann and Lensink, 2016; Batuo and Asongu, 2015; Asteriou et al., 2014; Delis et al., 2014; Li and Yu, 2014; Hamori and Hashiguchi, 2012). Although the GMM method can address fixed effects and the endogeneity of regressors by using the lagged dependent variable as instrumental variables to avoid dynamic panel bias, there is a commonly encountered issue in the application of GMM and system GMM known as instrument proliferation (Roodman, 2009).[8] In this study, we use the counterfactual estimators rather than GMM. Our methods of counterfactual estimators differ from traditional ones in the

[7] Although both types of policy measures are likely to make the economy more stable and reduce the incidence and severity of crises, capital controls and macroprudential policies are not the same. Korinek and Sandri (2016) detail the difference. In simple terms, macroprudential policies restrict borrowing by domestic agents, regardless of whether it is financed by domestic or foreign lenders; capital controls restrict financial transactions between residents and non-residents.

[8] If T equals 3, difference GMM produces merely one instrument per variable for instrumenting, and system GMM produces only two. However, as T increases, the number of instruments can quickly expand relative to the sample size, potentially leading to misleading asymptotic results concerning the estimators and associated specification tests.

following respects. Compared to the traditional two-way fixed effects model, the counterfactual estimators that we utilize in this study offer more reliable causal inferences in situations where the treatment effect is heterogeneous or when there are unobserved time-varying confounders. Specifically, the two-way fixed effects approach is predicated on the assumption of "absence of time-varying confounders", which is often not easily satisfied. To address violations of this assumption, Liu et al. (2024) employ the interactive fixed-effect counterfactual (IFEct) and matrix completion (MC) estimators, attempting to account for unobserved, time-varying confounders using a latent factor approach. Mathematically, both estimators aim to construct a lower-rank approximation of the outcome data matrix, utilizing information from only non-treated observations. The equivalence test also indicates that the two-way fixed effects estimator cannot pass the test, meaning that there is a very high probability of the presence of potential time-varying confounders. Moreover, compared to the generalized synthetic control method (GSCM) proposed by Xu (2017), the counterfactual estimators are capable of handling more intricate time-series cross-section designs, including staggering adoption (Athey et al., 2021) and treatment reversal, which are characteristics of data pattern in this paper. Besides, we consider not only the short- to medium-term effects occurring within 5 years but also the long-term effects extending beyond 5 years (up to 10 years) following the implementation of CFMs.

Thirdly, this paper considers policy indicators across various specific capital markets as well as aggregate indicators of CFMs. It matters because policymakers typically regulate various capital accounts individually rather than the entire capital account collectively. Depending exclusively on aggregate indicators, we cannot capture the heterogeneous effects of these policy interventions (Zhou, 2024). Furthermore, as stated in Asteriou et al. (2014) and Li and Su (2021), the composition of financial flows is significant for the net effect of globalization on inequality.[9] We also make a distinction between capital inflow and outflow managements because the direction of capital movements is of concern to policy makers, especially in emerging economies. While capital inflows like FDI can boost technology

[9] Performing an extensive review of the literature, Li and Su (2021) show that the composition of capital flows matters and even an identical type of capital flows such FDI can have diverse impacts. For example, FDI, which often targets high-skilled sectors, typically leads to increased inequality (Choi, 2006; Acharyya, 2011; Wu and Hsu, 2012; Jaumotte et al., 2013). Herzer and Nunnenkamp (2013) find that FDI lowers income inequality over the long-term, while it may have a positive effect in the short-term. IMF (2007) shows that the liberalization of foreign bank lending is likely to be linked to better financial access for the poor, thereby reducing inequality.

and growth, they may also cause economies to overheat and housing prices to surge. On the other hand, capital outflows or sudden stops can lead to currency devaluation and economic declines. The effects of inflows and outflows on financial cycles are obviously distinct. Moreover, there are variations in the combination of policymakers' inflow and outflow controls. Fernández et al. (2016b) show that controls on direct investment and real estate have a lower correlation between their inflow and outflow controls, which implies that policymakers' decisions on inflow and outflow controls are significantly different.

3 Data

This section presents the data used for our estimation. We collect the data from 100 economies between 1995 to 2019 on annual basis.[10] The core variables are the measures of wealth inequality and CFMs. As for wealth inequality, we use the Gini coefficients and the wealth share groups of wealth distribution. As for CFMs, we use the capital control restrictions data, in which economies implementing capital controls can be categorized as the treatment group while those not implementing them can be categorized as the control group. Table 3.1 reports the data sources and descriptive statistics of the variables used in this paper.

3.1 Wealth inequality

The data used for the Gini coefficients are from the World Inequality Database (WID). The choice of indicators from this database is based on two main reasons. (i) In contrast to the previous literature, this paper focuses on the effects on wealth inequality rather than income inequality. The most suitable indicators come from the WID. This database provides the aggregate wealth-income ratios and the changing structure of national wealth, allowing for the production of reliable estimates of wealth inequality. However, the traditional World Income Inequality Database (WIID) and the Standardized World Income Inequality Database (SWIID) do not provide corresponding indicators. (ii) The Gini coefficient provides an overall distribution structure but does not indicate which part of the distribution is affected by CFMs. Therefore, the Gini coefficient is not sufficient enough to

[10]The samples (treatment and control group) used for estimation are actually less than 100 economies due to the data missing, no pretreatment period, and few control units.

Table 3.1: Descriptive statistics and data sources

	Obs	Mean	Std. Dev	Min	Max	Source
Gini coefficient, wealth	2500	0.773	0.065	0.536	1.061	World inequality database (WID)
Wealth share, top 1%	2500	0.300	0.085	0.121	0.582	World inequality database (WID)
Wealth share, top 10%	2500	0.632	0.080	0.408	0.909	World inequality database (WID)
Wealth share, middle 40%	2500	0.331	0.061	0.157	0.478	World inequality database (WID)
Wealth share, bottom 50%	2500	0.037	0.025	-0.081	0.160	World inequality database (WID)
Capital control episode "ka"	2500	0.062	0.240	0.000	1.000	Fernandez et al. (2016)
Capital control episode (inflow) "kai"	2500	0.055	0.228	0.000	1.000	Fernandez et al. (2016)
Capital control episode (outflow) "kao"	2500	0.088	0.284	0.000	1.000	Fernandez et al. (2016)
Equity control "eq"	2500	0.596	0.491	0.000	1.000	Fernandez et al. (2016)
Equity control (inflow) "eqi"	2500	0.455	0.498	0.000	1.000	Fernandez et al. (2016)
Equity control (outflow) "eqo"	2500	0.550	0.498	0.000	1.000	Fernandez et al. (2016)
Equity control: purchase locally by nonresident	2473	0.309	0.462	0.000	1.000	Fernandez et al. (2016)
Equity control: sale or issue abroad by resident	2463	0.366	0.482	0.000	1.000	Fernandez et al. (2016)
Equity control: purchase abroad by resident	2480	0.412	0.492	0.000	1.000	Fernandez et al. (2016)
Equity control: sale or issue locally by nonresident	2469	0.469	0.499	0.000	1.000	Fernandez et al. (2016)
Bond control "bo"	2500	0.540	0.498	0.000	1.000	Fernandez et al. (2016)
Bond control (inflow) "boi"	2500	0.393	0.489	0.000	1.000	Fernandez et al. (2016)
Bond control (outflow) "boo"	2500	0.514	0.500	0.000	1.000	Fernandez et al. (2016)
Bond control: purchase locally by nonresident	2225	0.246	0.431	0.000	1.000	Fernandez et al. (2016)
Bond control: sale or issue abroad by resident	2222	0.394	0.489	0.000	1.000	Fernandez et al. (2016)
Bond control: purchase abroad by resident	2222	0.423	0.494	0.000	1.000	Fernandez et al. (2016)
Bond control: sale or issue locally by nonresident	2207	0.489	0.500	0.000	1.000	Fernandez et al. (2016)
Money market control "mm"	2500	0.553	0.497	0.000	1.000	Fernandez et al. (2016)
Money market (inflow) "mmi"	2500	0.391	0.488	0.000	1.000	Fernandez et al. (2016)
Money market (outflow) "mmo"	2500	0.534	0.499	0.000	1.000	Fernandez et al. (2016)
Money market: purchase locally by nonresident	2452	0.254	0.436	0.000	1.000	Fernandez et al. (2016)
Money market: sale or issue abroad by resident	2434	0.347	0.476	0.000	1.000	Fernandez et al. (2016)
Money market: purchase abroad by resident	2441	0.423	0.494	0.000	1.000	Fernandez et al. (2016)
Money market: sale or issue locally by nonresident	2431	0.445	0.497	0.000	1.000	Fernandez et al. (2016)
Collective inv. control "ci"	2500	0.557	0.497	0.000	1.000	Fernandez et al. (2016)
Collective inv. (inflow) "cii"	2500	0.358	0.480	0.000	1.000	Fernandez et al. (2016)
Collective inv. (outflow) "cio"	2500	0.544	0.498	0.000	1.000	Fernandez et al. (2016)
Collective inv.: purchase locally by nonresident	2419	0.234	0.424	0.000	1.000	Fernandez et al. (2016)
Collective inv.: sale or issue abroad by resident	2418	0.300	0.458	0.000	1.000	Fernandez et al. (2016)
Collective inv.: purchase abroad by resident	2457	0.405	0.491	0.000	1.000	Fernandez et al. (2016)
Collective inv.: sale or issue locally by nonresident	2414	0.468	0.499	0.000	1.000	Fernandez et al. (2016)
Derivatives control "de"	2500	0.504	0.500	0.000	1.000	Fernandez et al. (2016)
Derivatives (inflow) "dei"	2500	0.366	0.482	0.000	1.000	Fernandez et al. (2016)
Derivatives (outflow) "deo"	2500	0.498	0.500	0.000	1.000	Fernandez et al. (2016)
Derivatives: purchase locally by nonresident	2299	0.306	0.461	0.000	1.000	Fernandez et al. (2016)
Derivatives: sale or issue abroad by resident	2311	0.342	0.474	0.000	1.000	Fernandez et al. (2016)
Derivatives: purchase abroad by resident	2357	0.412	0.492	0.000	1.000	Fernandez et al. (2016)
Derivatives: sale or issue locally by nonresident	2300	0.433	0.496	0.000	1.000	Fernandez et al. (2016)
Commercial credit control "cc"	2500	0.397	0.489	0.000	1.000	Fernandez et al. (2016)
Commercial credit (inflow) "cci"	2483	0.270	0.444	0.000	1.000	Fernandez et al. (2016)
Commercial credit (outflow) "cco"	2463	0.335	0.472	0.000	1.000	Fernandez et al. (2016)
Financial credit control "fc"	2500	0.507	0.500	0.000	1.000	Fernandez et al. (2016)
Financial credit (inflow) "fci"	2485	0.373	0.484	0.000	1.000	Fernandez et al. (2016)
Financial credit (outflow) "fco"	2459	0.436	0.496	0.000	1.000	Fernandez et al. (2016)
Guarantees control "gs"	2500	0.376	0.485	0.000	1.000	Fernandez et al. (2016)
Guarantees (inflow) "gsi"	2455	0.251	0.433	0.000	1.000	Fernandez et al. (2016)
Guarantees (outflow) "gso"	2458	0.343	0.475	0.000	1.000	Fernandez et al. (2016)
Direct inv. control "di"	2500	0.560	0.496	0.000	1.000	Fernandez et al. (2016)
Direct inv. (inflow) "dii"	2499	0.447	0.497	0.000	1.000	Fernandez et al. (2016)
Direct inv. (outflow) "dio"	2464	0.356	0.479	0.000	1.000	Fernandez et al. (2016)
Real estate control "re"	2500	0.675	0.468	0.000	1.000	Fernandez et al. (2016)
Real estate: purchase locally by nonresident "rei"	2456	0.566	0.496	0.000	1.000	Fernandez et al. (2016)
Real estate: purchase abroad by resident "re_pabr"	2445	0.349	0.477	0.000	1.000	Fernandez et al. (2016)
Real estate: sale locally by nonresident "re_slbn"	2387	0.270	0.444	0.000	1.000	Fernandez et al. (2016)
Financial development	2500	0.393	0.239	0.038	1.000	Svirydzenka (2016)
Inflation	2397	10.573	104.284	-8.484	4145.106	World development indicators (WDI)
Total population	2500	60.605	177.980	0.268	1400.939	World inequality database (WID)
Gov. expenditure on education	1908	4.493	1.518	0.000	9.897	World development indicators (WDI)
Real GDP per capita	2467	16559.640	18538.626	231.468	87123.660	World development indicators (WDI)
Average education	1918	87.795	26.950	5.283	163.935	World development indicators (WDI)
Population growth	2500	1.334	1.557	-3.758	19.360	World development indicators (WDI)
Government subsidies	1718	43.083	18.792	0.234	84.610	World development indicators (WDI)
Money supply	2034	62.155	48.315	6.823	403.314	World development indicators (WDI)

provide an adequate picture of wealth inequality (Piketty and Zucman, 2014). The WID perfectly offers series on the entire distribution of wealth from the bottom to the top.

The Gini coefficient ranges from 0 to 1. The value of 0 represents perfect equality, and that of 1 represents that a single individual has all the wealth while all others have none. All wealth series are based on the concept of "net personal wealth for equal-split adults," which means that the wealth is distributed to adults and distributed equally within couples or households (Alvaredo et al., 2016).[11] Following Teixeira (2023) and Alvaredo et al. (2016), we choose the wealth share of the top 1%, top 10%, middle 40% and bottom 50% of the distribution. These four groups map relatively well to the idea of top, upper, middle and lower classes. They summarize changes happening to the overall distribution fairly well. Although wealth inequality shows slow annual variation over time, it becomes a suitable candidate for quasi-synthetic controls. This is so because if the outcome of interest experiences significant volatility, minor interventions such as adjustments in the disaggregated level of CFMs might be indistinguishable from other shocks (Abadie, 2021; Teixeira, 2023).

3.2 Capital flow management measures

Capital flow management measures (CFMs) are our explanatory variables of interest in this paper. We use the *de jure* indicator of capital controls from Fernández et al. (2016b) (FKRSU hereafter). The previous literature has predominantly focused on studying the aggregate level of capital account liberalization. However, the ease or tightness of aggregate level of restrictions depends on the strength of various asset specific restrictions such as restriction on equity inflow. Policymakers take into account the effects of particular policies when formulating their strategies. Therefore, this paper places a greater emphasis on the impact of granular level of asset-specific CFMs on wealth inequality.

The FKRSU dataset provides such granular indices by distinguishing the directions and categories of capital flows, enabling us to analyze these effects more comprehensively. This dataset ranges from 1995 to 2019 for 100 economies. Furceri and Loungani (2018) argue

[11] Alvaredo et al. (2016) use "equal-split adults" as their benchmark variables. On the one hand, as the primary goal is to study the wealth of different individuals, it is logical to allocate wealth predominantly to working adults. On the other hand, although the concept of equal distribution of income and wealth among partners in couples might seem simplistic, it is also unrealistic to expect zero resource sharing and this approach tends to underestimate the resources accessible to nonworking spouses.

that many capital account liberalization episodes have occurred in the 1990s and 2010s, particularly in the 1990s. Binici and Das (2021) find that between 2008 and 2019, over 40 countries adjusted their CFMs including both liberalization and tightening. These adjustments involved either relaxing or strengthening existing measures, as well as introducing new measures or removing existing ones. In other words, the coverage of time period is sufficient for identifying the policy effects of CFMs.

The granular indices of FKRSU dataset are 0-1 dummy, for 1 representing the presence of a restriction and 0 representing no restriction for a given country at a given time. According to Klein (2012)'s classification, we can categorize the countries in our sample into three groups: "open", "gate", and "wall". An "open" country has virtually no CFMs on any asset category throughout the sample period. A "wall" country has extensive restrictions on all or almost all categories of assets, and a "gate" country applies CFMs episodically. In line with these classifications, "open" countries are suitable for using as the control group. The counterfactual estimator that we employ in this paper utilizes data from the control group to construct models and estimate counterfactuals for treated observations based on the models (Liu et al., 2024). "Gate" countries can be classified as the treatment group. However, "wall" countries, especially those implementing controls continuously from the outset of our sample in 1995, cannot provide a sufficient pretreatment period. In such cases, the presence of "incidental parameters" can lead to biased estimates of treatment effects, and these samples are subsequently excluded (Xu, 2017). This data pattern aligns with the counterfactual estimation approach developed by Liu et al. (2024) for time-series cross-sectional (TSCS) data with dichotomous treatments that switch back and forth.

The FKRSU dataset contains CFMs for 10 types of specific asset categories and we use the two-letter abbreviations in Fernández et al. (2016b) for simplicity: equity ("eq" hereafter), bonds or other debt securities ("bo" hereafter), money market instruments ("mm" hereafter), collective investment ("ci" hereafter), derivatives ("de" hereafter), commercial credits ("cc" hereafter), financial credit ("fc" hereafter), guarantees, sureties and financial back-up facilities ("gs" hereafter), direct investment ("di" hereafter), and real estate ("re" hereafter). Fernández et al. (2016b) differentiate among transaction types based on the residency of either the buyer or the seller, and the nature of the transaction, whether it involves a purchase, sale, or issuance. Using "eq" as an illustration, there are four transaction control categories: two concerning inflows, which encompass purchase locally by non-residents ("eq_plbn") and sale or issue abroad by residents ("eq_siar"), and two related to outflows, purchase abroad by residents ("eq_pabr") and sale or issue locally by non-residents ("eq_siln"). As we have mentioned above, all the granular indices of FKRSU dataset are 0-1 dummy. Regarding aggregate indices, rather than employing Fernández et al. (2016b)'s arithmetic mean of each granular index, we opt for an alternative approach to maintain the aggregate indices in binary form. Taking equity "eq" as an example, the construction process of the equity inflow/outflow management indicator is as follows:

$$\text{equity inflow management: "eqi"} = \begin{cases} 1 & \text{If either "eq\_plbn" or "eq\_siar" equals 1,} \\ 0 & \text{otherwise.} \end{cases}$$

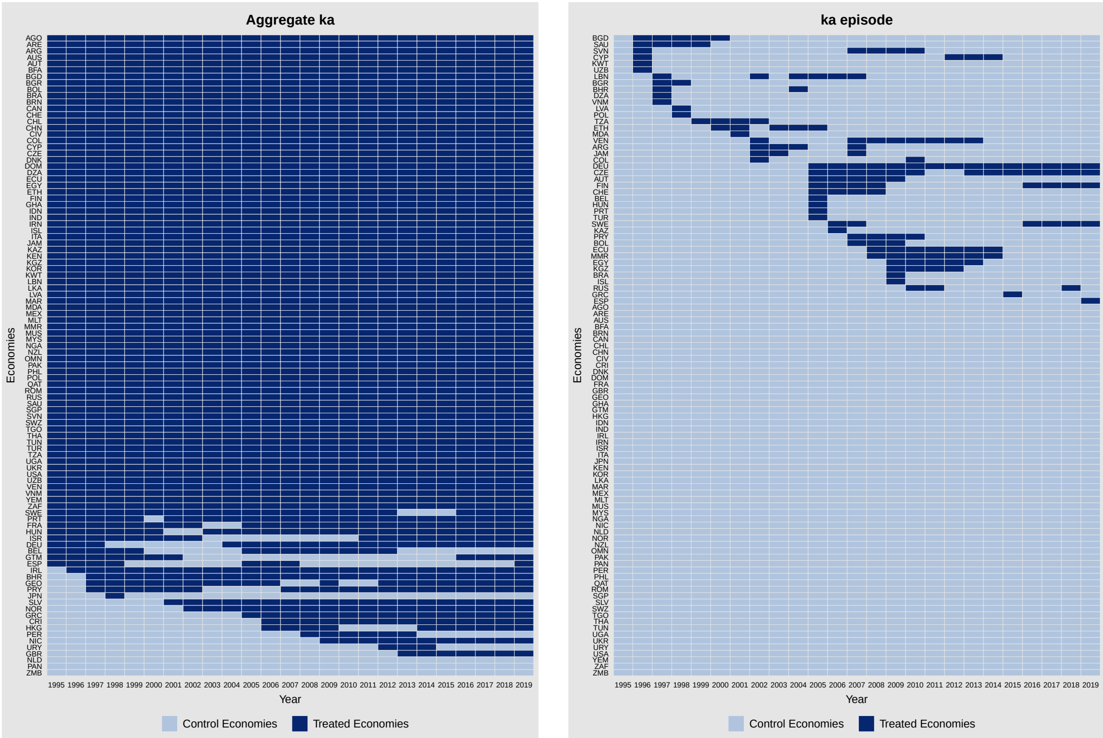
$$\text{equity outflow management: "eqo"} = \begin{cases} 1 & \text{If either "eq\_pabr" or "eq\_siln" equals 1,} \\ 0 & \text{otherwise.} \end{cases}$$

$$\text{equity flow management: "eq"} = \begin{cases} 1 & \text{If either "eqi" or "eqo" equals 1,} \\ 0 & \text{otherwise.} \end{cases}$$

As for aggregated indicator for all categories, we can also compile it in the same way:

$$\text{aggregate all categories: "ka"} = \begin{cases} 1 & \text{if one of the asset specific index equals 1,} \\ 0 & \text{otherwise.} \end{cases}$$

Figure 3.1a presents the visualization results of the treatment and control groups for aggregated "ka". The control group and the treatment group are marked in light and dark blue, respectively. This indicates that upwards of 74% of the observations, classified as treated economies (dark blue) from the year 1995, are excluded from the sample intended for estimation. Furthermore, only three countries, the Netherlands, Panama, and Zambia, have remained in a state of no capital flow management since 1995, resulting in a control group sample size such that it is too limited to allow for precise estimation of factors and the derivation of estimated counterfactuals (Xu, 2017). Therefore, the aggregated "ka" indicator is not suitable for the counterfactual estimator, and the use of this indicator may result in a significant estimation bias.



(a) Aggregate all categories "ka"

(b) "ka" episode using Furceri & Loungani (2018)

Figure 3.1: Visualization results of the treatment and control groups

Following Furceri and Loungani (2018), we reconstruct the aggregated indicator to identify the capital flow management episodes. Fernández et al. (2016b) provide the aggregated "ka" as the arithmetic mean of each granular index. We identify these episodes by assuming that an episode takes place when, for a specific country at a specific time, the annual fluctuation in the arithmetic mean "ka" indicator surpasses the average annual change across all observations by two standard deviations. Figure 3.1b presents the visualization results of the treatment and control groups for these episodes. Based on these criteria, the identification of episodes classifies more than half of the economies as having no CFMs. These criteria selectively identify economies that have implemented substantial regulatory measures on their capital accounts compared to previous periods from an average perspective (without depending on the overall degree of the restriction of such economies' capital account). Unlike in the previous case of Figure 3.1a, this identification method places greater emphasis on the extent of changes in CFMs rather than the specific levels of tightness or looseness of their implementation. As a substitute measure for the aggregated “ka”, we later explore the impacts of the identified CFMs episodes on wealth inequality.

As we employ different CFMs indicators, the countries composing the treatment and control groups vary for each specific indicator. Additionally, as there are economies where policy indicators have been implemented before the beginning of our sample period in 1995, the corresponding samples are removed. Consequently, the actual number of samples used in each model specification differs. We will annotate the number of economies in the treatment and control groups in each regression result and specify the economies included in the treatment group.

3.3 Control variables

We choose control variables based on three reasons: (i) we follow the existing research on policy and inequality (Teixeira, 2023; Hasan et al., 2020; Frost et al., 2020; Delis et al., 2014); (ii) the control variables should also be closely related to wealth inequality; (iii) the selection should guarantee that the synthetic controls can precisely replicate the characteristics of countries using CFMs. From these reasons, we choose financial development, inflation growth, population, real GDP per capita, and government expenditure on education (%GDP) as the control variables. In Section 6, for robustness checks, we employ the average of education and population growth as alternative variables of the government expenditure on education (%GDP) and population. We also consider other policy indicators that might affect wealth inequality, such as the government subsidies as a proxy for fiscal policy and broad money as a proxy for conventional monetary policy.[12]

4 Methodology

In this paper, we utilize the up-to-date counterfactual estimator proposed by Liu et al. (2024) to establish causality from CFMs to wealth inequality. The proposed counterfactual estimator is adopted from the following reasons. Firstly, our data pattern is a time-series cross-sectional setting with a dichotomous treatment. Secondly, the treatment effects of CFMs are heterogeneous both across units and over time. For example, although several

[12] Xu (2023) argues that the estimator permitting user discretion such as cherry-picking covariates to improve the appearance of pre-treatment fit results in spurious findings.

economies implement such policies, they have heterogeneous impacts on different economies depending on the economy's demographics, level of economic development, and financial market structure, etc. The counterfactual estimators used in this paper can capture such differential trends, and the no-time-varying-confounder assumption will be relaxed to a certain extent.[13] Thirdly, our data pattern involves reversal and staggering adoption, making the conventional generalized synthetic control method (GSCM) invalid.[14] Additionally, as we do not focus on the specific policies of a specific country, the synthetic control method (SCM) itself is not applicable. Lastly, technically, the counterfactual estimator method is embedded in a cross-validation scheme that can choose the number of potential factors (or tuning parameter) based on model performance measured by mean-squared prediction error (MSPE).

4.1 Identification

The outcome of interest, wealth inequality $Gini_{it}$, is given by a linear factor model:

$$Gini_{it} = \delta_{it} CFMs_{it} + \boldsymbol{X}'_{it}\boldsymbol{\beta} + \boldsymbol{\lambda}'_i \boldsymbol{f}_t + \alpha_i + \xi_t + \varepsilon_{it}, \tag{1}$$

where $CFMs_{it}$ is the aggregated or asset specific CFMs that equal 1 when an economy i is treated at time t and equal 0 otherwise. Differing from a conventional two-way fixed effects model in which δ_{it} is constant, we allow δ_{it}, which is our interest treatment effect, to be heterogeneous across economy i over time t. $\boldsymbol{X}$ is a $(p \times 1)$ vector of observed covariates. $\boldsymbol{\beta} = [\beta_1, \cdots, \beta_p]'$ is a $(p \times 1)$ vector of unknown parameters. $\boldsymbol{f}_t = [f_{1t}, \cdots, f_{rt}]'$ is an $(r \times 1)$ vector of unobserved common factors and $\boldsymbol{\lambda}_i = [\lambda_{1r}, \cdots, \lambda_{ir}]$ is an $(r \times 1)$ vector of unknown factor loadings. ε_{it} represents unobserved idiosyncratic shocks for economy i at time t and has zero mean. α_i and ξ_t are the two-way fixed effects. For identification, we need to impose two constrains (Liu et al., 2024):

$$\boldsymbol{\Lambda}'\boldsymbol{\Lambda} = \text{diagonal}; \quad \frac{\boldsymbol{F}'\boldsymbol{F}}{T} = \mathbf{I},$$

[13] Fixed-effect counterfactual estimator (FEct) can result in biased estimates in the presence of unobserved time-varying confounders. Bai (2009), and Bai and Ng (2021) suggest that the use of factor-augmented models resolves this issue when the confounders can be broken down into time-specific factors interacting with unit-specific factor loadings (Liu et al., 2024).

[14] In Xu (2017), he does not consider the cases in which the treatment switches on and off (or "multiple-treatment-time").

where $\boldsymbol{\Lambda} = [\lambda_1, \cdots, \lambda_N]'$ captures the heterogeneous impacts caused by each economy's various unobserved characteristics and $\boldsymbol{F} = [f_1, \cdots, f_T]'$ can be understood as time-varying trends that affect each economy differently. The causal parameter of interest is the average treatment effect on the treated economies (ATT):

$$ATT = \mathbb{E}\left[\delta_{it} | CFMs_{it} = 1, \forall i \in \mathcal{T}, \forall t\right] = \mathbb{E}\left[Gini_{it}(1) - \widehat{Gini_{it}}(0) | CFMs_{it} = 1, \forall i \in \mathcal{T}, \forall t\right],$$

where the potential outcomes of $Gini_{it}$ are defined as $Gini_{it}(CFMs_{it})$. $\mathcal{T}$ is defined as units whose treatment status has changed during the observed time window. The counterfactual estimators treat data under the treatment group as missing and use data under the control group to build a model for non-treated potential outcome $Gini_{it}(0)$. Then, we predict $Gini_{it}(0)$ for the observations under the treatment conditions. ATT is obtained by taking an average of the differences between $Gini_{it}(1)$ and $\widehat{Gini_{it}}(0)$ for the treated observations.

4.2 Estimators

We use two counterfactual estimators constructing the treated counterfactuals $\widehat{Gini_{it}}(0)$: interactive fixed-effect counterfactual (IFEct) estimator and matrix completion (MC) estimator. As argued by Liu et al. (2024), both IFEct and MC use lower-rank matrix approximation techniques to enhance predictions for treated counterfactuals. Their distinction lies in the regularization approach: IFEct chooses the number of factors, whereas MC depends on a tuning parameter θ.

In the absence of covariates, the non-treated potential outcome matrix $\boldsymbol{Gini}(\mathbf{0}) = [Gini_{it}(0)]_{i=1,2,\cdots,N;t=1,2,\cdots T}$ can be approximated by $\boldsymbol{L} = [L_{it}]_{i=1,2,\cdots,N;t=1,2,\cdots T}$:

$$\boldsymbol{Gini}(\mathbf{0}) = \boldsymbol{L} + \varepsilon, \quad \mathbb{E}[\varepsilon | \boldsymbol{L}] = \mathbf{0},$$

where ε_{it} can be thought of as measurement error. The goal is to estimate the matrix $\boldsymbol{L}$. As for IFEct, $\boldsymbol{L}$ can be expressed as the product of two matrices: $\boldsymbol{L} = \boldsymbol{\Lambda}\boldsymbol{F}$. Using an Expectation-Maximization (EM) algorithm, the factors and factor loadings can be estimated by minimizing the least squares objective function:

$$\left(\hat{\boldsymbol{F}}^{(h+1)}, \hat{\boldsymbol{\Lambda}}^{(h+1)}\right) = \arg\min \mathbf{tr}\left[\left(\mathbf{W}^{(h+1)} - \tilde{\boldsymbol{F}}\tilde{\boldsymbol{\Lambda}}'\right)'\left(\mathbf{W}^{(h+1)} - \tilde{\boldsymbol{F}}\tilde{\boldsymbol{\Lambda}}'\right)\right]$$
$$s.t. \quad \frac{\tilde{\boldsymbol{F}}'\tilde{\boldsymbol{F}}}{T} = \mathbf{I}_r,\ \tilde{\boldsymbol{\Lambda}}'\tilde{\boldsymbol{\Lambda}} = \text{diagonal},$$

where

$$\mathbf{W}^{(h+1)} := \begin{cases} Gini_{it} - \boldsymbol{X}'_{it}\hat{\boldsymbol{\beta}}^{(h+1)}, & CFMs_{it} = 0 \\ \hat{\alpha}_i^{(h)} + \hat{\xi}_t^{(h)} + \hat{\boldsymbol{\lambda}}_i^{(h)\prime}\hat{\boldsymbol{f}}_t^{(h)}, & CFMs_{it} = 1, \end{cases},$$

and h means the h round in the EM algorithm.[15]

Different from IFEct, the MC estimator directly estimates $\boldsymbol{L}$ by solving the following minimization problem:

$$\hat{\boldsymbol{L}} = \arg\min_{\boldsymbol{L}}\left[\sum_{(i,t)\in\mathcal{O}}\frac{(Gini_{it} - L_{it})^2}{|\mathcal{O}|} + \theta\|\boldsymbol{L}\|\right],$$

where $\mathcal{O} = \{(i,t)\,|CFMs_{it} = 0\}$ and $|\mathcal{O}|$ is the number of element in $\mathcal{O}$. $\|\boldsymbol{L}\|$ is the matrix norm of $\boldsymbol{L}$ and θ is the tuning parameter. Athey et al. (2021) develop an iterative algorithm to obtain $\hat{\boldsymbol{L}}$ and we provide an abridged description as follows: (i) Given a tuning parameter θ and start with the initial value

$$\mathbf{L}_0(\theta) = P_{\mathcal{O}}(\boldsymbol{Gini}) = \begin{cases} \boldsymbol{Gini}_{it}, & (i,t) \in \mathcal{O} \\ 0, & (i,t) \notin \mathcal{O} \end{cases};$$

(ii) For $h = 0, 1, 2, \ldots$, calculate

$$\mathbf{L}_{(h+1)}(\theta) = \text{shrink}_\theta\left[P_{\mathcal{O}}(\boldsymbol{Gini}) + P_{\mathcal{O}}^{\perp}(\mathbf{L}_h(\theta))\right] = \mathbf{S}\tilde{\boldsymbol{\Sigma}}\mathbf{R}^T,$$

where $\tilde{\boldsymbol{\Sigma}}$ equals to $\boldsymbol{\Sigma}$ with the i-th singular value $\sigma_i\left(P_{\mathcal{O}}(\boldsymbol{Gini}) + P_{\mathcal{O}}^{\perp}(\mathbf{L}_h(\theta))\right)$ replaced by $\max\left(\sigma_i\left(P_{\mathcal{O}}(\boldsymbol{Gini}) + P_{\mathcal{O}}^{\perp}(\mathbf{L}_h(\theta))\right) - \theta, 0\right)$[16]; (iii) Repeat until the sequence $\{\mathbf{L}_h(\theta)\}_{h\geq 0}$

[15] The interested reader is referred to the original paper by Liu et al. (2024) for additional details.

[16] This form is called soft impute and there is another form called hard impute, where $\sigma_i\left(P_{\mathcal{O}}(\boldsymbol{Gini}) + P_{\mathcal{O}}^{\perp}(\mathbf{L}_h(\theta))\right)$ can be replaced by

$$\sigma_i\left(P_{\mathcal{O}}(\boldsymbol{Gini}) + P_{\mathcal{O}}^{\perp}(\mathbf{L}_h(\theta))\right)\mathbf{1}\left\{\sigma_i\left(P_{\mathcal{O}}(\boldsymbol{Gini}) + P_{\mathcal{O}}^{\perp}(\mathbf{L}_h(\theta))\right) \geq \theta\right\}.$$

converges.

In both the IFEct and MC estimators, we use a k-fold cross-validation scheme to select the optimal number of factors r and the tuning parameter θ that minimizes mean-squared prediction error (MSPE) of the wealth outcome. For the IFEct estimator, we restrict the analysis to countries with at least 7 pre-treatment periods (Xu, 2017; Teixeira, 2023). The uncertainty estimates are obtained through a block bootstrap procedure for 1000 times.

4.3 Statistical Tests

Following Liu et al. (2024), we conduct two diagnostic tests to examine the validity of the identifying assumptions. The first assumption is the function form of equation (1) of a linear (interactive) fixed effects model with a dichotomous treatment. The second assumption is a strict exogeneity for interactive fixed effects as follows: for any $i, j = 1, 2, \cdots, N$ and $t, s = 1, 2, \cdots, T$,

$$\varepsilon_{it} \perp\!\!\!\perp CFMs_{js}, \boldsymbol{X}_{js}, \alpha_j, \xi_s, \boldsymbol{\lambda}_j, \boldsymbol{f}_t.$$

We first use the equivalence test proposed by Hartman and Hidalgo (2018). The basic idea is to jointly test a set of null hypotheses that the average of residuals for any pre-treatment period fall within a pre-specified narrow range:

$$ATT_s < -\theta_2 \text{ or } ATT_s > \theta_1, \quad \forall s \leq 0.$$

Rejecting the null hypothesis suggests that for any $s \leq 0$, the range of $-\theta_2 \leq ATT_s \leq \theta_1$ is maintained. In other words, if we collect sufficient data to demonstrate that the averages of pre-treatment residuals lie within a narrowly defined range, it serves as evidence backing the assumption that there are no time-varying confounders. The range $[-\theta_2, \theta_1]$ is thus referred to as the equivalence bound. Following the suggestion in Liu et al. (2024) and Hartman and Hidalgo (2018), we use the two one-side test to check the equivalence of ATT_s to zero for each $s < 0$. We use a conservative standard that the null hypothesis is deemed rejected only if the tests across all pre-treatment periods yield significant outcomes. The

Liu et al. (2024) show that hard impute is equivalent to IFEct since both algorithms impose penalties based on the number of factors, and the only difference between MC with hard impute and IFEct lies in their initial values.

equivalence bound is set $\theta_1 = \theta_2 = 0.36\sigma_\varepsilon$ where σ_ε is the standard deviation of residualized non-treated outcome. The equivalence test is criticized on two fronts: on one hand, the selection of the equivalence bound is seen as overly arbitrary, and using $0.36\sigma_\varepsilon$ might be too forgiving when the effect size is small compared to the outcome's variance. On the other hand, this test suffers from an over-fitting issue, making it very easy to pass this test.

The second test is the (out-of-sample) placebo test. The fundamental concept involves presuming that the treatment begins S periods prior to its actual start for each economy in the treatment group, and subsequently utilizing the identical counterfactual estimator to compute ATT_s for $s = -(S-1), \cdots, -1, 0$. If the assumption of no time-varying confounders is valid, then this ATT estimate should be statistically equivalent to zero, with an insignificant outcome supporting the assumption's credibility. S should not be excessively large since a larger S means there will be fewer pre-treatment periods available for estimation. Conversely, if both S and the number of treatment observations are too small, the test might lack sufficient power. In this and subsequent examples, we have chosen $S = 3$. Unlike the in-sample equivalence test, the placebo test involves out-of-sample predictions of $\boldsymbol{Gini}(\mathbf{0})$ during the placebo periods, making it more robust to model mis-specification and immune to over-fitting.

Table 4.1 shows the results of the equivalence test and placebo test explained above for different CFMs indicators (equity "eq", bond "bo", money market "mm", collective investment "ci", derivative "de", commercial credit "cc", financial credit "fc", guarantee and sureties "gs", direct investment "di", real estate "re", and aggregated index "ka") based on three different estimators: FEct, IFEct, and MC. For FEct, all asset specific indices indicate a trend towards the treatment onset that exceeds the equivalence bound. Consequently, we cannot dismiss the null hypothesis that pre-treatment residual averages are not zero, which means that we cannot assert with high probability that equivalence is maintained. From this perspective, even if they all pass the placebo test, this paper will not employ the fixed-effect counterfactual estimator (FEct) to estimate the ATT of CFMs. As for IFEct, apart from restrictions on commercial credit "cc" and guarantees and sureties "gs", all other asset specific indicators pass the equivalence test. This also means that the confidence intervals of pre-treatment residual averages fall within the equivalence bounds. As for MC, we find that for all the CFMs indices, both tests passed the examination.

Regarding the choice between IFEct and MC, Liu et al. (2024) suggest that researchers may choose between the two models either based on two diagnostic tests aforementioned and/or their relative predictive power (e.g., as measured by the Mean Squared Prediction Error (MSPE)). We utilize the toolbox from Liu et al. (2024) to determine which estimator has a smaller MSPE. When both IFEct and MC pass the equivalence and placebo tests, we make a choice for each indicator basically depending on the MSPE.

Table 4.1: Equivalence tests and placebo tests

Estimators	Tests	"eq"	"bo"	"mm"	"ci"	"de"	"cc"	"fc"	"gs"	"di"	"re"	"ka"
FEct	Equivalence tests	×	×	×	×	×	×	×	×	×	×	×
	Placebo tests	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
IFEct	Equivalence tests	✓	✓	✓	✓	✓	×	✓	×	✓	✓	✓
	Placebo tests	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
MC	Equivalence tests	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
	Placebo tests	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: The above table shows the results of the equivalence tests and placebo tests based on three different estimators (FEct, IFEct, and MC) for different asset specific CFMs indicators (equity "eq", bond "bo", money market "mm", collective investment "ci", derivative "de", commercial credit "cc", financial credit "fc", guarantee and sureties "gs", direct investment "di", real estate "re", and aggregate index "ka"). Checked mark "✓" means it passes the test, and "×" mark means not. Each equivalence holds when the bootstrapped one-side confidence interval of ATTs falls within $[-0.36\hat{\sigma}_\varepsilon, 0.36\hat{\sigma}_\varepsilon]$, the equivalence bound. Three pre-treatment periods ($s = -2, -1, 0, \cdots$) serve as the placebo. Standard errors are based on 1000 parametric bootstraps at the country level.

5 Empirical Results

This section presents the effects of CFMs on wealth inequality. We first provide the results using the aggregated indicators. We examine the effects for aggregated index "ka", and aggregated inflow "kai", outflow index "kao". We also investigate these effects under different income levels (i.e., advanced and emerging economies). As one of the contributions of this paper, we investigate the effects of asset specific indicators on wealth inequality, and we also consider both inflow and outflow asset-specific indicators. Lastly, since the Gini coefficients do not fully capture all segments of the wealth distribution, we also examine the effects of CFMs on different wealth shares groups.

5.1 Aggregate CFMs and wealth inequality

Figure 5.1 shows that implementing aggregate CFMs reduces wealth inequality. The result aligns with previous research on income inequality, where Li and Su (2021), Gallagher et al. (2019), Furceri et al. (2017), Furceri and Loungani (2018), De Haan and Sturm (2017), and Bumann and Lensink (2016) all found that capital account liberalization could increase income inequality. However, unlike their findings, the effect of CFMs on wealth inequality is not statistically significant in our analysis. This insignificant result may be related to two reasons. On one hand, as we have already mentioned earlier, the effects of CFMs on wealth inequality are not uniform across economies; in fact, this policy encompasses a high degree of heterogeneity. Erten et al. (2021) show that the practical implementation of CFMs is highly complex and heavily reliant on institutional specifics. On the other hand, previous research has shown that the impact of capital account liberalization on income inequality is conditional. This conditionality depends on many factors, such as financial depth, financial development, quality of institutions, and crises, most of which are closely related to the income level of the country. Gallagher et al. (2019) argue that there is a consensus among researchers regarding the importance of institutions, noting that countries with robust institutions typically experience greater penetration of financial services. Therefore, we further categorize our sample into advanced and emerging economies based on the World Bank Income Group and investigate the effects of CFMs for different income levels of economies.

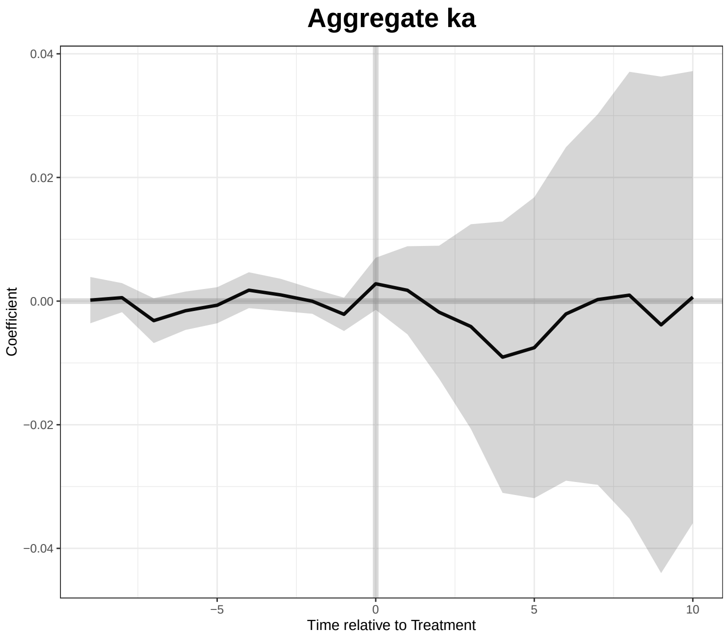


Figure 5.1: The dynamic cumulative average treatment effects (ATT) of aggregate CFMs "ka" on wealth inequality

Note: The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficient of the average effect ATT is estimated using IFEct.

Table 5.1 show the results for aggregated index "ka" for the full sample, advanced economies, and emerging economies. As we explain above (Figure 5.1), in the full sample case, although CFMs can reduce wealth inequality, their results are not statistical significant. However, Figure 5.2 shows that the use of CFMs in advanced economies actually increases wealth inequality, and this result is significant in the $T_0 + 8$ period. After 8 years of CFMs, the Gini coefficient of wealth concentration in the treated economies is estimated to be 3.8 percentage points higher than that in the control economies. Our finding is in line with those obtained by Gallagher et al. (2019), Furceri and Loungani (2018), and Bumann and Lensink (2016) for income inequality. Gallagher et al. (2019) show that capital openness correlates with reduced inequality as a country moves into a higher income group, typically characterized by stronger institutional quality. Furceri and Loungani (2018) show that the effects between capital account openness and income inequality are more precisely estimated for high-income economies. Bumann and Lensink

Table 5.1: The effects of aggregate CFMs "ka" on wealth inequality

Indices	"ka"		
Income level	All	AEs	EMs
Number of treatment economies	18	12	6
Number of control economies	64	26	38
Gini coefficient (↓ or ↑)	↓	↑*	↓

Note: The above table presents the cumulative average treatment effects of aggregate CFMs ("ka") on the treated economies (ATT) for different income levels (full sample, advanced economies (AEs), and emerging economies (EMs)) after aggregate CFMs adoption. The result with ↑ mark means that CFMs increase wealth inequality, and ↓ mark means CFMs decrease wealth inequality. We also show the number of treatment and control economies used in the sample in the third and forth row. The arrow mark with asterisk means the result is statistically significant at the 10% level.

(2016) find that capital account liberalization generally reduces income inequality only when financial depth, indicated by the ratio of private credit to GDP, is above 25 percent. Conversely, their findings suggest that in most emerging economies with limited financial depth, capital account liberalization is likely to exacerbate income inequality. We obtain a consistent result on emerging economies to them ("ka" EMs in Table 5.1). Our result regarding emerging economies ("ka" EMs) indicates that CFMs reduce wealth inequality, but it is not statistically significant as well as in the full sample case ("ka" All).

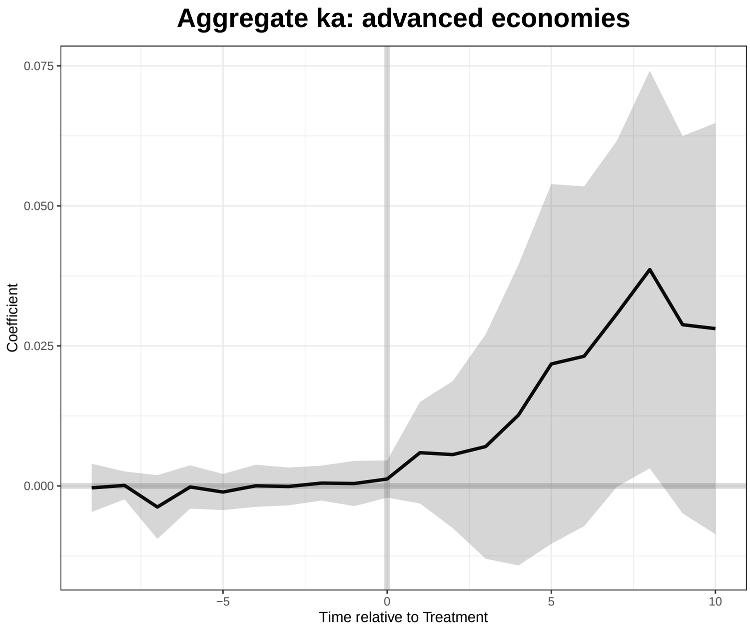


Figure 5.2: The dynamic cumulative average treatment effects (ATT) of aggregate CFMs "ka" on wealth inequality in advanced economies

Note: The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficient of the average effect ATT is estimated using IFEct.

Overall, CFMs significantly exacerbate wealth inequality in advanced economies, while in emerging economies and full sample, CFMs reduce wealth inequality, but the two results are not statistically significant. We will show how the Gini coefficient changes from the perspective of the wealth distribution in Section 5.6. More specifically, we will show how the CFMs affect the wealth share of the top 1%, top 10%, middle 40% and bottom 50% in each case.

5.2 Aggregate inflow/outflow CFMs and wealth inequality

The aggregate indices examined in Section 5.1 only reflect the overall level of CFMs. However, in practice, the direction of capital movements is of concern to policy makers especially in emerging economies. Capital flows like FDI can bring technology and promote economic growth in emerging economies, but they can also lead to an overheated economy and excessive increases in housing prices (Hernandez-Vega, 2023; Cesa-Bianchi et al., 2018; Kim and Yang, 2011). Conversely, capital outflows (or capital flight) or sudden stops can cause currency devaluations in emerging economies, leading to economic downturns (Bianchi, 2011; Mendoza, 2010). The impacts of capital inflows and outflows on economic (or financial) cycles are clearly different. Moreover, the policymakers' combination of controls on inflow and outflow varies across countries (Fernández et al., 2016b). In this section, we examine the effects of inflow and outflow controls on wealth inequality, and also explore those in advanced and emerging economies individually.

Table 5.2 summarizes the results for the aggregate inflow "kai" and outflow "kao" management indices. The full sample result of "kai" (All) indicates that inflow controls increase wealth inequality, although the result is not statistically significant. Even when the sample is divided into advanced and emerging economies, the results remain not statistically significant. The full sample result of "kao" (All), while consistent with the aggregate index "ka" (All), is also not statistically significant. However, dividing the full sample into advanced and emerging economies, we find that outflow controls significantly increase wealth inequality in emerging economies ("kao" EMs). We show this outcome in Figure 5.3, where, in the $T_0 + 3$ period, the adoption of outflow controls "kao" in emerging economies raises the Gini coefficient of wealth concentration by 6.25 percentage points. Although the result does not seem consistent to the related literature, we will explain how the increase in Gini coefficient in emerging economies is caused by the wealth distribution change in Section 5.5. Regarding advanced economies, "kao" also tends to increase wealth inequality, but this estimated result is not statistically significant.

Table 5.2: The effects of aggregate inflow CFMs "kai" and outflow CFMs "kao" on wealth inequality

Indices	"kai"			"kao"		
Income level	All	AEs	EMs	All	AEs	EMs
Number of treatment economies	12	5	7	21	14	7
Number of control economies	71	36	35	61	25	36
Gini coefficient (↓ or ↑)	↑	↓	↑	↓	↑	↑*

Note: The above table presents the cumulative average treatment effects of "kai" and "kao" on the treated economies (ATT) for different income levels (full sample, advanced economies (AEs), and emerging economies (EMs)) after the adoption of CFMs. The result with ↑ mark means that CFMs increase wealth inequality, and ↓ mark means CFMs decrease wealth inequality. We also show the number of treatment and control economies used in the sample in the third and forth row, respectively. The arrow mark with asterisk means the result is statistically significant at the 10% level.

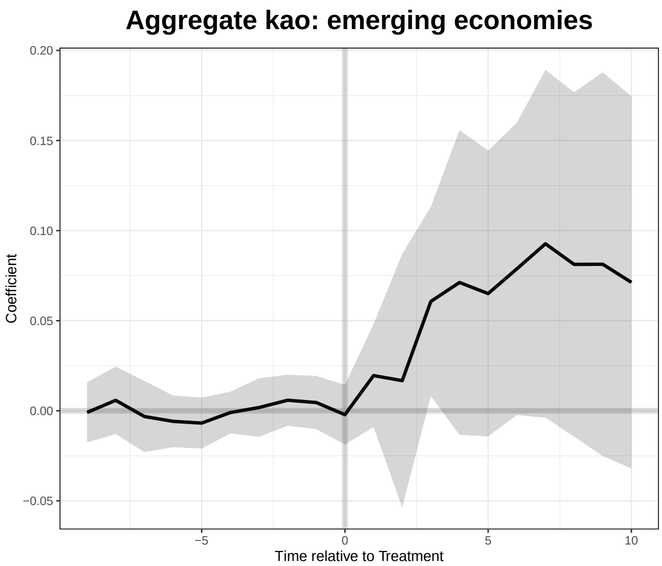


Figure 5.3: The dynamic cumulative average treatment effects (ATT) of aggregate outflow controls "kao" on wealth inequality in emerging economies

Note: The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficient of the average effect ATT is estimated using IFEct.

In summary, by examining capital inflow/outflow managements separately, we obtain different results from the full sample case. We find that in emerging economies, the implementation of capital outflow management "kao" increases wealth inequality, and the result is statistically significant.

5.3 Asset specific CFMs and wealth inequality

Compared with other economic policies, CFMs are considerably more complex and heavily reliant on institutional specifics. In practice, CFMs can be classified across various dimensions (Erten et al., 2021). One of the most critical dimensions is that CFMs involve different types of capital account transactions. The Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER) reports on the rules and regulations for international transactions across 10 asset categories covering a significant portion of global cross-national asset holdings. Policymakers rarely regulate the entire capital account as a whole, but instead impose controls on separate capital accounts.[17] Relying solely on aggregate indices, we cannot capture the heterogeneity in the policy effects in separate capital accounts (Kitano and Zhou, 2022; Zhou, 2024). In this section, therefore, we study the effects of different asset-specific CFMs on wealth inequality.

Table 5.3 presents the results of the relationship between CFMs and wealth inequality in terms of the different transaction (assets) categories. Different transaction types have different effects on wealth inequality. Overall, the effects of asset specific CFMs exhibit significant heterogeneity. Specifically, CFMs in the asset categories of equity "eq", bond "bo", collective investment "ci", financial credit "fc", direct investment "di", and real estate "re" seem to increase the Gini coefficient, but the results are not statistically significant. In contrast, CFMs in the asset categories of money market "mm", derivatives "de", commercial credit "cc", and guarantees and sureties "gs" seem to reduce the Gini coefficient. Among them, the effects of CFMs in the money market "mm" and derivatives "de" are statistically significant as shown in Figure 5.4. From Figure 5.4a, it is evident that the CFMs in "mm" reduces wealth inequality immediately after policy implementation, with long-lasting statistical significance from the $T_0 + 6$ period onwards. The largest decline in the Gini coefficient occurs at $T_0 + 9$, about a 4% point drop. Figure 5.4b shows that the CFMs in "de" also reduces wealth inequality, with statistical significance starting from $T_0 + 6$ period. The largest decline in the Gini coefficient occurs at $T_0 + 10$, about a 2.5% point drop. The decline in the Gini coefficient due to CFMs in "mm" and "de" is consistent with

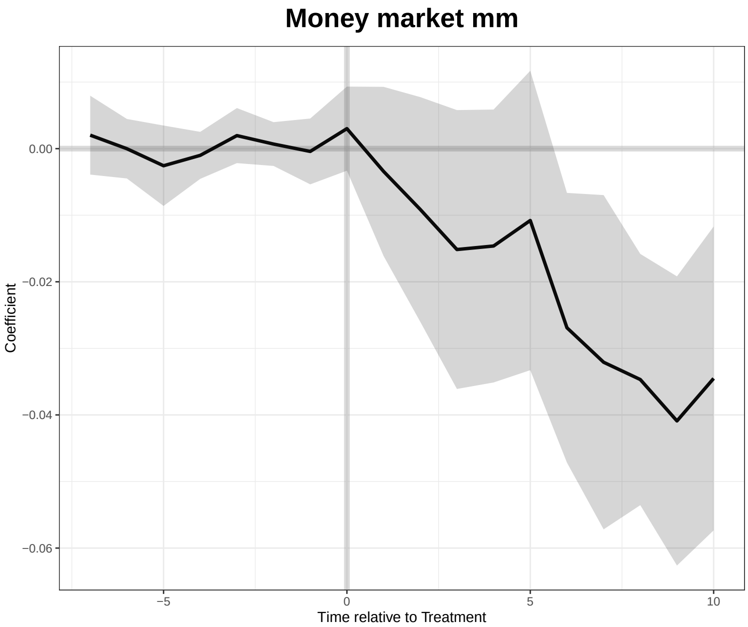
[17] For example, in January 2008, Brazil imposed a tax on inflows related to external loans. In October 2009, Brazil implemented the IOF tax, which covered, at varying rates, different types of capital flows, such as fixed-income securities, stocks, margin deposits, derivative contracts, and FDI (IMF, 2019).

the related literature implying that capital account liberalization leads to the increase in the Gini coefficient. As mentioned by Furceri and Loungani (2018) and Teixeira (2023), the Gini coefficient changes relatively slowly over time. Taking it into account, the effects of implementing CFMs in "mm" and "de" on wealth concentration are quite large. We will show how the specific CFMs affect the wealth share of the top 1%, top 10%, middle 40% and bottom 50% of the wealth distribution in Section 5.6.

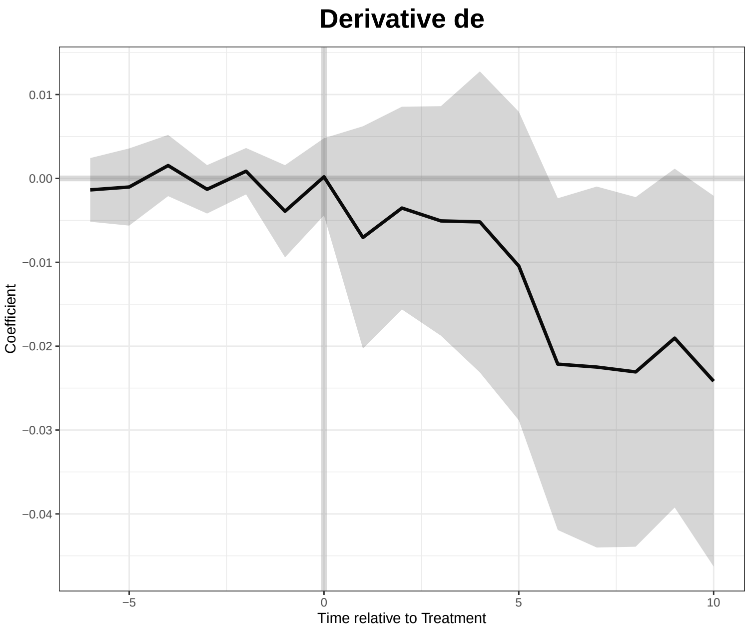
Table 5.3: The effects of asset-specific CFMs on wealth inequality

Indices	"eq"	"bo"	"mm"	"ci"	"de"	"cc"	"fc"	"gs"	"di"	"re"
Number of treatment economies	12	10	13	14	15	4	14	4	7	9
Number of control economies	18	20	22	24	22	44	29	47	25	16
Gini coefficient (↓ or ↑)	↑	↑	↓*	↑	↓*	↓	↑	↓	↑	↑

Note: The above table presents the cumulative average treatment effects of asset-specific CFMs indices (equity "eq", bond "bo", money market "mm", collective investment "ci", derivative "de", commercial credit "cc", financial credit "fc", guarantees and sureties "gs", direct investment "di", real estate "re") on the treated economies (ATT) after the adoption of CFMs. The mark ↑ (↓) means that tightening CFMs increases (decreases) Gini coefficient. We also show the number of treatment and control economies used in the sample in the third and forth row. The arrow mark with asterisk means this result is statistically significant at the 10% level.



(a) money market “mm”



(b) derivative “de”

Figure 5.4: The dynamic cumulative average treatment effects (ATT) of asset-specific CFMs on wealth inequality

Note: We show the statistically significant results for CFMs on (a) money market “mm” and (b) derivatives “de”. The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT are estimated using MC.

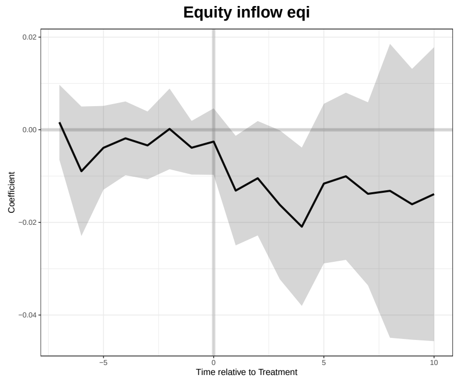
Table 5.4: The effects of asset-specific inflow CFMs on wealth inequality

Indices	"eqi"	"boi"	"mmi"	"cii"	"dei"	"cci"	"fci"	"gsi"	"dii"	"rei"
Number of treatment economies	5	8	9	9	9	3	10	6	10	11
Number of control economies	36	38	35	39	38	54	43	56	32	22
Results (↓ or ↑)	↓*	↑*	↓*	↑	↓	↓	↑	↓	↓*	↑

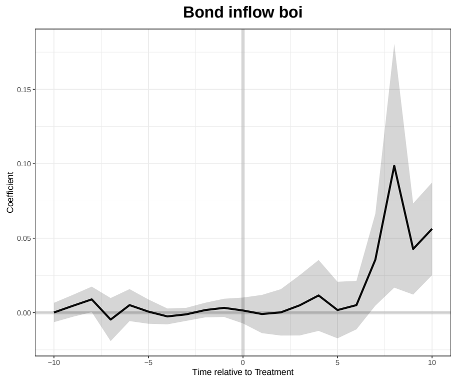
Note: The above table presents the cumulative average treatment effects of asset-specific inflow CFMs indices (quity "eqi", bond "boi", money market "mmi", collective investment "cii", derivative "dei", commercial credit "cci", financial credit "fci", guarantees and sureties "gsi", direct investment "dii", real estate "rei") on the treated economies (ATT) after the adoption of inflow CFMs. The result with ↑ (↓) mark means that tightening CFMs increases (decreases) wealth inequality. We also show the number of treatment and control economies used in the sample in the third and forth row. The arrow mark with asterisk means this result is statistically significant at the 10% level.

5.4 Asset specific inflow/outflow CFMs and wealth inequality

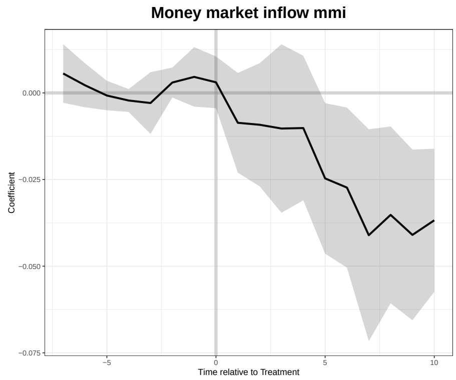
We also examine the effects of asset specific CFMs by breaking down into inflow and outflow indices. Table 5.4 shows the results for asset-specific inflow indices. Compared to Table 5.3, the asset-specific inflow CFMs exhibit a general consistency with the results in Table 5.3, but they differ in some important points. As shown in Figure 5.5a, inflow CFMs on equity "eqi" significantly reduces wealth inequality. The effect peaks in four years after policy implementation, and wealth inequality decreases by 2 percentage points. Figure 5.5d shows that inflow CFMs on direct investment "dii" also reduces wealth inequality by 6% points after four years, but the effect becomes non-significant after the $T_0 + 5$ period in the long-run. The effects of inflow CFMs on bonds "boi" and money market "mmi" align with the results in Table 5.3. The inflow CFMs on "boi" significantly increases wealth inequality after the $T_0 + 7$ period, reaching a 10% points peak at the 8th period as shown in Figure 5.5b. Figure 5.5c shows that the inflow CFMs on "mmi" significantly reduces wealth inequality after the $T_0 + 5$ period, reducing the Gini coefficient of wealth concentration by 4% points at the 7th period. Both exhibit long-term sustainability in their effects.



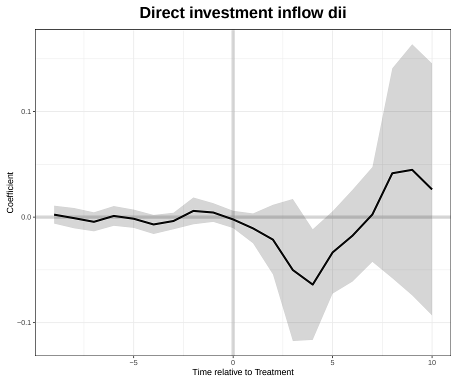
(a) Equity inflow “eqi”



(b) Bond inflow “boi”



(c) Money market inflow “mmi”



(d) Direct investment inflow “dii”

Figure 5.5: The dynamic cumulative average treatment effects (ATT) of asset-specific inflow CFMs on wealth inequality

Note: We show the statistically significant results for CFMs on (a) equity inflow “eqi”, (b) bond inflow “boi”, (c) money market inflow “mmi”, and (d) direct investment inflow “dii”. The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT in sub-figure 5.5a and 5.5c are estimated using MC, and the coefficients of the average effect ATT in sub-figure 5.5b and 5.5d are estimated using IFEct.

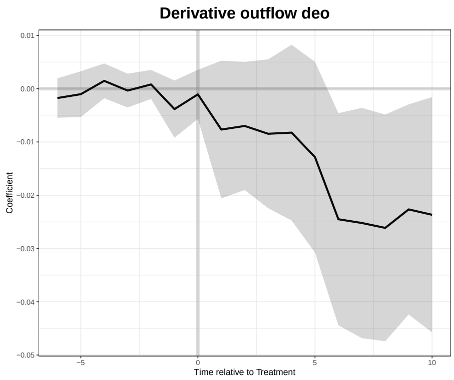
Table 5.5 shows the results for asset-specific outflow CFMs indices. The directions of their impacts on wealth inequality (i.e., increase or decrease) are consistent with the results in Table 5.3. Among them, outflow CFMs on derivatives “deo” significantly reduces wealth inequality as shown in Figure 5.6a. This effect becomes statistical significant in the long-term (after $T_0 + 6$) and reaches its peak at $T_0 + 8$ period, reducing the Gini coefficient of wealth concentration by 2.5 % points. Outflow CFMs on financial credit “fco”, direct investment “dio”, and real estate “reo” significantly increase wealth inequality, as shown in Figures 5.6b, 5.6c, and 5.6d, respectively. Unlike “deo” in Figure 5.6a, the effects of these

Table 5.5: The effects of asset-specific outflow CFMs on wealth inequality

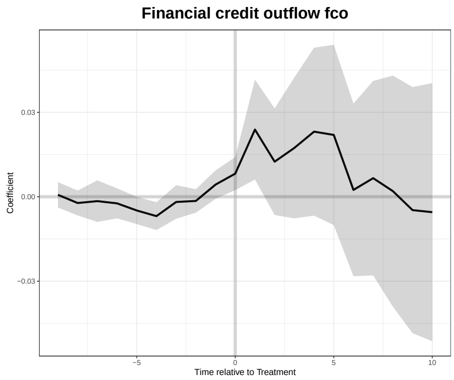
Indices	"eqo"	"boo"	"mmo"	"cio"	"deo"	"cco"	"fco"	"gso"	"dio"	"reo"
Number of treatment economies	11	10	9	14	16	3	14	5	5	15
Number of control economies	21	22	27	25	23	51	33	48	44	39
Results (↓ or ↑)	↑	↑	↓	↑	↓*	↓	↑*	↓	↑*	↑*

Note: The above table presents the cumulative average treatment effects of asset-specific outflow CFMs indices (quity "eqo", bond "boo", money market "mmo", collective investment "cio", derivative "deo", commercial credit "cco", financial credit "fco", guarantees and sureties "gso", direct investment "dio", real estate "reo") on the treated economies (ATT) after the adoption of outflow CFMs. The result with ↑ (↓) mark indicates that tightening the CFMs increases (decreases) wealth inequality. We also show the number of treatment and control economies used in the sample in the third and forth row. The arrow mark with asterisk means this result is statistically significant at the 10% level.

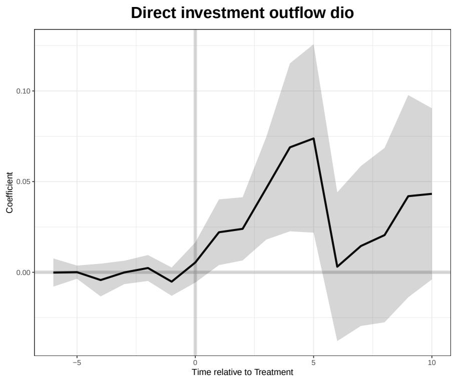
three policies ("fco", "dio", and "reo") materialize in the short term (within $T_0 + 5$) and have a shorter duration of significance. The adoption of "fco" increases the Gini coefficient of wealth concentration by 2.2 % points at $T_0 + 6$ (Figure 5.6b). The implementation of "dio" becomes significant from $T_0 + 1$ and reaches its maximum at $T_0 + 5$, with a 7.5% points increase (Figure 5.6c). The adoption of "reo" significantly raises wealth inequality at $T_0 + 1$, but the effect is relatively smaller, at 1.1% points (Figure 5.6d).



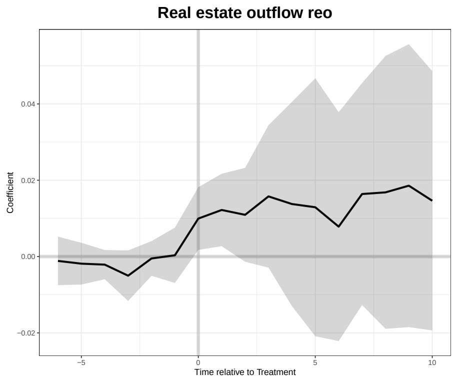
(a) Derivative outflow “deo”



(b) Financial credit outflow “fco”



(c) Direct investment outflow “dio”



(d) Real estate outflow “reo”

Figure 5.6: The dynamic cumulative average treatment effects (ATT) of asset-specific outflow CFMs on wealth inequality

Note: We show the statistically significant results on CFMs on (a) derivative outflow “deo”, (b), financial credit outflow “fco”, (c) direct investment outflow “dio”, and (d) real estate outflow “reo”. The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficient of the average effect ATT in sub-figure 5.6a is estimated using MC, and the coefficients of the average effect ATT in sub-figure 5.6b, 5.6c and 5.6d are estimated using IFEct.

On the whole, these results indicate that the granular CFMs exhibit high heterogeneity. Among the asset-specific CFMs cases, only the results of CFMs on “mm” and “de” are statistically significant and their implementation reduces wealth inequality (Figure 5.4). The outcomes for asset-specific inflow CFMs on “eqi” and “dii” indicate that these two inflow CFMs significantly reduce wealth inequality (Figure 5.5a and 5.5d), which contrasts with “eq” and “di” (Table 5.4). The results for asset-specific outflow CFMs shown in Table 5.5 are basically consistent with those for asset-specific CFMs shown in Table 5.3. “deo” significantly reduces wealth inequality (Figure 5.6a), while “fco”, “dio”, and “reo” significantly increase wealth inequality (Figure 5.6b, 5.6c, and 5.6d). A notable point is that the inflow CFMs on "dii" significantly reduces wealth inequality, whereas the outflow CFMs on "dio" significantly increases wealth inequality. This implies that even though they are related to the same asset market, the inflow and outflow CFMs may have the opposite effects on wealth inequality.

5.5 Aggregate CFMs and wealth shares

The Gini coefficient provides an overall distribution structure, but does not indicate which part of the distribution is affected by CFMs, and is not sufficient to provide an adequate picture of wealth inequality (Piketty and Zucman, 2014). Besides, Piketty and Saez (2003) argue that an important trend in income inequality is that income is concentrated in very high income earners. In this section, we study the effects of CFMs on the wealth shares of the top 1%, top 10%, middle 40% and bottom 50% of the distribution.

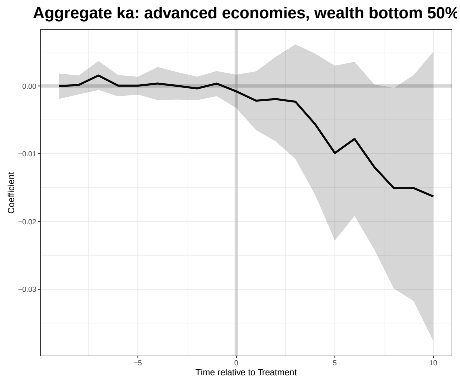
Table 5.6 summarizes the results for aggregate index "ka" and inflow/outflow management indices "kai" and "kao". For the aggregate index "ka" for the full sample and emerging economies (All and EMs), the implementation of this policy leads to an increase in the share of the top wealth level combined with a decrease in the proportion of the middle/bottom levels, but all the results are statistically insignificant. As shown in Figure 5.7a, however, in advanced economies (AEs), the implementation of "ka" causes a significant decline (by 1.5% points in 8 years after the adoption of "ka") in the share of the bottom 50% group. This decline in the share of the bottom 50% group explains the significant increase in the Gini coefficient in advanced economies shown in Figure 5.2. Similarly, in the "kai" (All) case, the policy's implementation significantly reduces the share of the bottom 50% group (Figure 5.7b). The decline in the share of the bottom 50% group accounts for the increase in the Gini coefficient in the full sample case (in Table 5.2). Moreover, the implementation of "kai" (AEs) significantly reduces the share of the top 1% in advanced economies by 7.5% points after 5 years as shown in Figure 5.7c. The decline in the share of the top 1% due to "kai" (AEs) explains the decrease in the Gini coefficient in advanced economies (in Table 5.2). Figure 5.7d shows that the implementation of "kao" (AEs) significantly increases the share of the middle 40% in advanced economies by 1.25% points after $T_0 + 4$. In spite of the increase in the share of the middle 40% due to "kao" (AEs), the Gini coefficient

Table 5.6: The effects of aggregate "ka", inflow "kai", outflow "kao" on different wealth shares

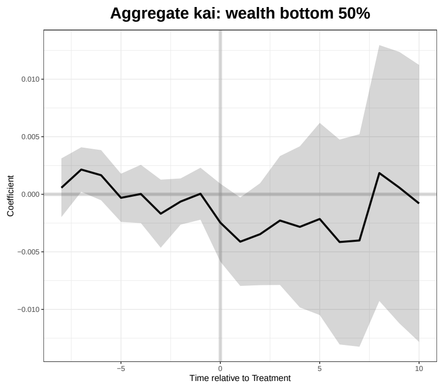
Indices	"ka"			"kai"			"kao"		
Income level	All	AEs	EMs	All	AEs	EMs	All	AEs	EMs
Number of treatment economies	18	12	6	12	5	7	21	14	7
Number of control economies	64	26	38	71	36	35	61	25	36
Average Top 1%	↑	↑	↑	↑	↓*	↑	↓	↓	↑
Average Top 10%	↑	↑	↑	↑	↓	↑	↑	↓	↑*
Average Middle 40%	↓	↑	↓	↓	↓	↓	↓	↑*	↓
Average Bottom 50%	↓	↓*	↓	↓*	↓	↓	↓	↓	↓
Gini coefficient (↓ or ↑)	↓	↑*	↓	↑	↓	↑	↓	↑	↑*

Note: The above table presents the cumulative average treatment effects of aggregate CFMs indices (aggregate "ka", inflow "kai", and outflow "kao") on the treated economies (ATT) for the full sample (All), advanced economies (AEs), and emerging markets (EMs) after the adoption of CFMs. The mark ↑ (↓) in the Gini coefficient indicates that tightening CFMs increases (decreases) wealth inequality (Tables 5.1 and 5.2). We also show the number of treatment and control economies used in the sample in the third and forth row. Row 4-7 show the wealth share for top 1%, 10%, middle 40%, and bottom 50%, respectively. The arrow mark with asterisk means this result is statistically significant at the 10% level.

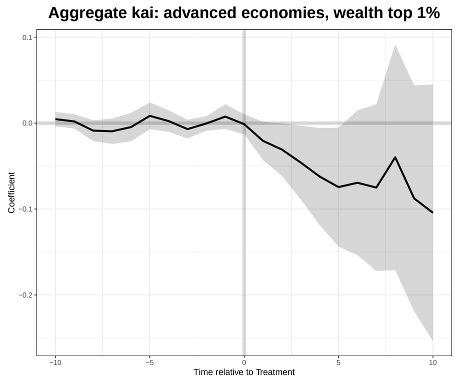
increases in Table 5.2, but the corresponding result is not statistically significant. Figure 5.7e shows that the implementation of "kao" (EMs) increases the share of the top 10% in emerging economies by 12.5% points in $T_0 + 7$. This increase in top 10% level share due to "kao" (EMs) accounts for the increase of the Gini coefficient in emerging economies shown in Figure 5.3.



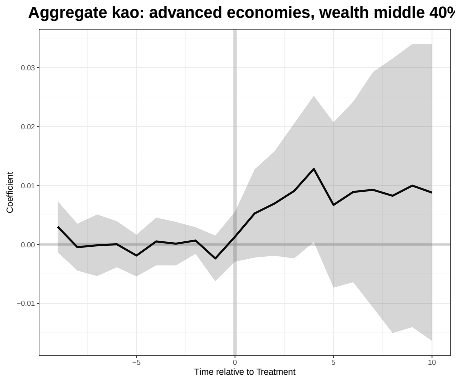
(a) Aggregate “ka” for advanced economies, wealth bottom 50%



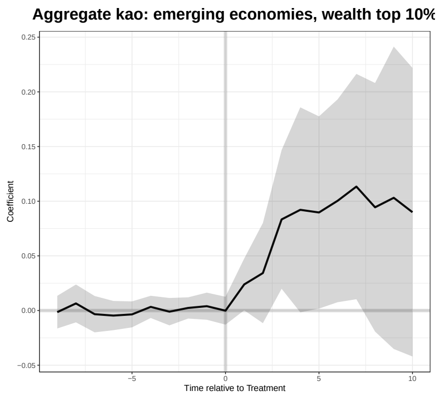
(b) Aggregate “kai”, wealth bottom 50%



(c) Aggregate “kai” for advanced economies, wealth top 1%



(d) Aggregate “kao” for advanced economies, wealth middle 40%



(e) Aggregate “kao” for emerging economies, wealth top 10%

Figure 5.7: The dynamic cumulative average treatment effects (ATT) of aggregate “ka”, inflow“kai”, outflow “kao” on different wealth shares

Note: We show the statistically significant results for (a) aggregate “ka” for advanced economies, (b) inflow “kai” for advanced economies, (c) outflow “kao” for advanced economies, (d) outflow “kao” for emerging economies, and (e) inflow “kai” for the full sample. The horizontal axis represents the relative time before and after implementing capital control at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT are estimated using IFEct.

5.6 Asset specific CFMs and wealth shares

Table 5.7 summarizes the results on the effects of asset specific CFMs on different wealth shares. Overall, the rise (fall) in the Gini coefficient is due to the rise (fall) in the wealth share of top 1% and (or) 10%, and the fall (rise) in the wealth share of middle 40% and (or) bottom 50%.

The typical case is that of CFMs in money market "mm". Figure 5.8 (row 1) shows that the wealth shares of top 1% and 10% fall and those of middle 40% and bottom 50% rise, which leads to the fall in the Gini coefficient shown in Figure 5.4a. The effect of CFMs in "mm" on wealth shares is persistently statistically significant. Similarly, Figure 5.8 (row 2) shows that CFMs in derivative "de" significantly reduce the wealth share of the top 10% and increases that of the bottom 50%, although the fall in the top 1% and the rise in the the middle 40% are not statistically significant. The fall in the top 10% and the rise in the bottom 50% due to "de" shown in the second row of Figure 5.8 are likely to cause the fall in the Gini coefficient due to "de" shown in Figure 5.4b. Figure 5.8 (row 3) shows that CFMs in guarantees and sureties "gs" significantly reduce the wealth share of the top 1% and increase those of the middle 40% and bottom 50%, although the rise in the top 10% is not statistically significant. The fall in the top 1% and the rise in the middle 40% and bottom 50% due to "gs" shown in the third row of Figure 5.8 are likely to cause the fall in the Gini coefficient due to "gs" (Table 5.3), although it is not statistically significant. These results on "mm", "de", and "gs" are consistent with those obtained by Li and Su (2021) and Teixeira (2023). Li and Su (2021) find that capital account liberalization is associated with a decrease in the income share of the poor and an increase in the income share of the rich. Teixeira (2023) shows that the increase in wealth inequality 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%. Unlike those in "mm", "de", and "gs", CFMs in "di" significantly reduce the share of the top 10% and decrease the share of the bottom 50% at the same time, which plausibly explains the statistically insignificant result on the Gini coefficient due to "di" (Table 5.3).

Table 5.7: The effects of asset-specific CFMs on different wealth shares

Indices	“eq”	“bo”	“mm”	“ci”	“de”	“cc”	“fc”	“gs”	“di”	“re”
Number of treatment economies	12	10	13	14	15	4	14	4	7	9
Number of control economies	18	20	22	24	22	44	29	47	25	16
Average Top 1%	↑	↑	↓*	↑	↓	↓*	↑	↓*	↓	↓
Average Top 10%	↑	↑	↓*	↑	↓*	↓	↑	↓	↓*	↑
Average Middle 40%	↓	↓	↑*	↓	↑	↑	↓	↑*	↑	↑
Average Bottom 50%	↓	↓	↑*	↓	↑*	↑	↓	↑*	↓*	↓
Gini coefficient (↓ or ↑)	↑	↑	↓*	↑	↓*	↓	↑	↓	↑	↑

Note: The above table presents the cumulative average treatment effects of asset-specific CFMs (quity “eq”, bond “bo”, money market “mm”, collective investment “ci”, derivative “de”, commercial credit “cc”, financial credit “fc”, guarantees and sureties “gs”, direct investment “di”, real estate “re”) on the treated economies (ATT) after the adoption of CFMs. The mark ↑ (↓) in Gini coefficient presents that CFMs increase (decrease) wealth inequality. We also show the number of treatment and control economies used in the sample in the third and forth row. Row 4-7 show the wealth shares of top 1%, 10%, middle 40%, and bottom 50%, respectively. The arrow mark with asterisk means this result is statistically significant at the 10% level.

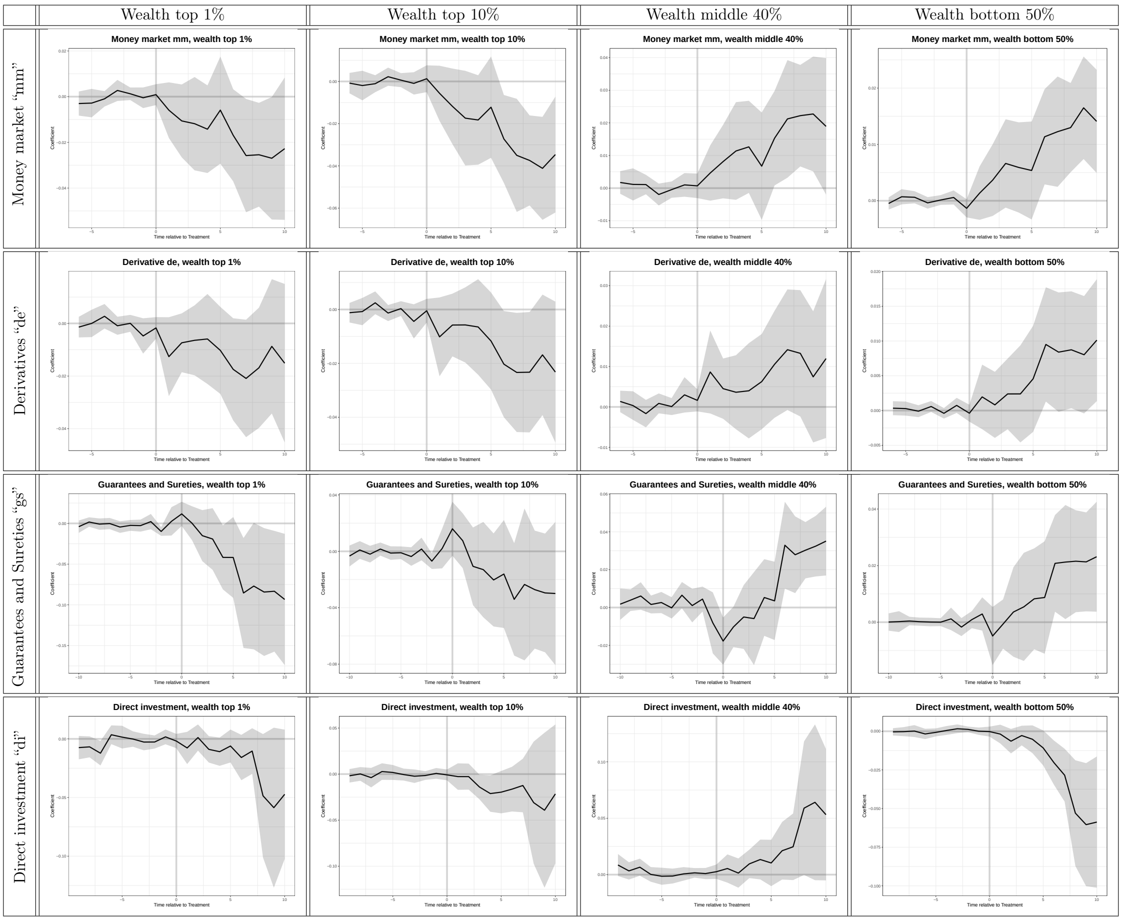


Figure 5.8: The dynamic cumulative average treatment effects (ATT) of asset-specific CFMs on different wealth shares

Note: We show the results for CFMs on money market "mm", derivatives "de", guarantees and sureties "gs", and direct investment "di". The four column shows the wealth shares of top 1%, 10%, middle 40%, and bottom 50%, respectively. The horizontal axis represents the relative time before and after the CFMs' implementation at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT for money market "mm", derivatives "de" are estimated using MC, and the coefficients of the average effect ATT for guarantees and sureties "gs", and direct investment "di" are estimated using IFEct.

6 Robustness Checks

In this section, we perform various robustness checks on the relationship between CFMs and wealth inequality. We provide a series of robustness exercises and we find that our analysis results are basically robust. We test if the results are robust when we use the alternative measures of CFMs, alternative control variables, and alternative pre-treatment periods. We also test the robustness in the case where we include the other possible factors such as fiscal and monetary policies.

6.1 Different measures of capital flow managements

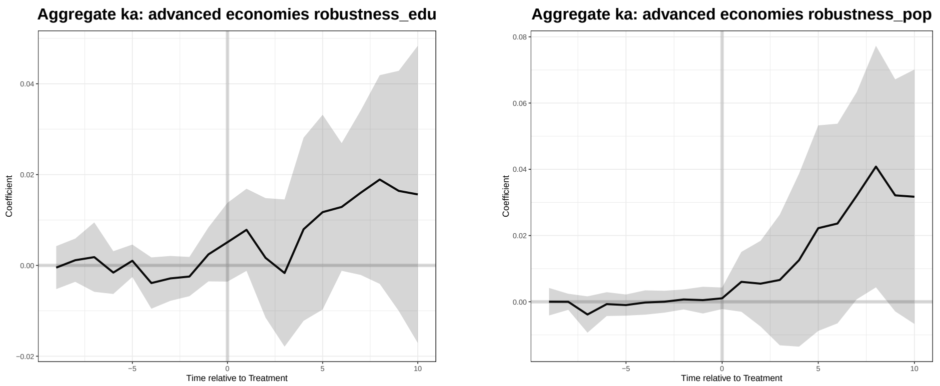
We first test if the results about the effects of aggregate CFMs on wealth inequality are robust to the use of alternative measures of CFMs. We re-estimate Eq.(1) using the Chinn and Ito (2006, 2008) "KAOPEN" indicator of synthetic capital account liberalization. Since the index is available for an unbalanced panel of 182 countries from 1970 to 2020, ranging from -1.9 (more restricted capital account) to 2.3 (less restricted), we need to define aggregate CFMs episodes in a way that allows the index to be classified as a 0-1 dummy. Following the method of Furceri and Loungani (2018), we identify CFMs episodes as follows. We assume that a CFM occurs when, for a given country at a given time, the annual change in the "KAOPEN" indicator is *below* the average annual change observed across all observations by more than two standard deviations.[18] Using the alternative measure, we obtain the consistent results to Section 5.1. Similarly, as in the main analysis, the robustness check also yields the similar result for aggregate CFMs in the full sample case. However, the robustness check for advanced economies does not yield a statistically significant result. The different outcome for advanced economies may be due to the construction method of CFMs episodes in the robustness check, in which rare outcomes are classified as having implemented CFMs. Additionally, whereas in Fernández et al. (2016b)'s database, twelve advanced economies are selected as treatment economies, the CFMs episodes here turn out to include only two advanced economies as treatment economies. This could explain the different result from the main analysis in the robustness check.

6.2 Different conditioning factors

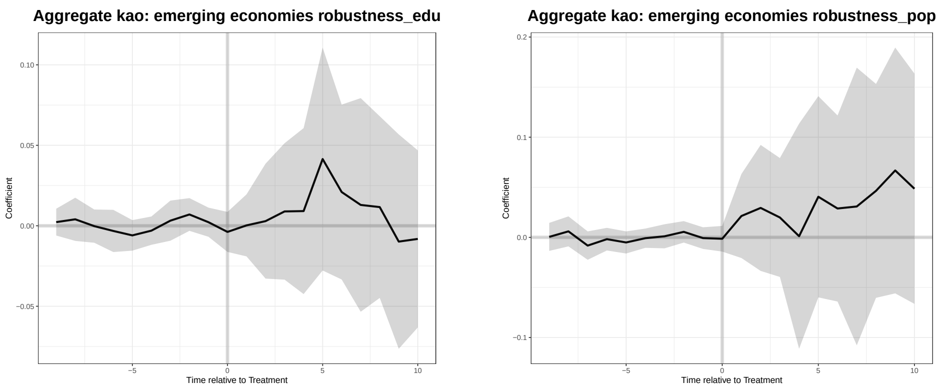
Following Teixeira (2023), we re-estimate Eq.(1) using the alternative measures of education and population. We use "average of education" instead of "Government expenditure on education (%GDP)," and " population growth" instead of "population." As Figures 6.1a are broadly consistent with Figure 5.2 in Section 5.1, we confirm that the implementation of aggregate CFMs increases wealth inequality in advanced economies. However, unlike in Figure 5.3, the effects on aggregate outflow CFMs in emerging economies are not statisti-

[18] Unlike FKRSU, the "KAOPEN" index indicates that a higher value means a more open capital account.

cally significant, which implies that we should be cautious in interpreting our result (Figure 6.1b). Figures 6.2a and 6.2b also provide robust evidences showing that the imposition of asset-specific CFMs on money market "mm" and derivative "de" are significantly associated with a decrease in wealth inequality, which is similar to Figure 5.4 in Section 5.3 in the main analysis.



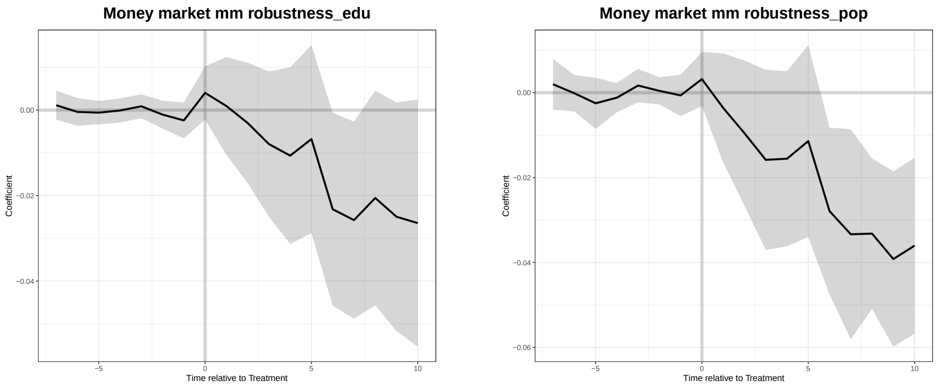
(a) Aggregate "ka" for advanced economies



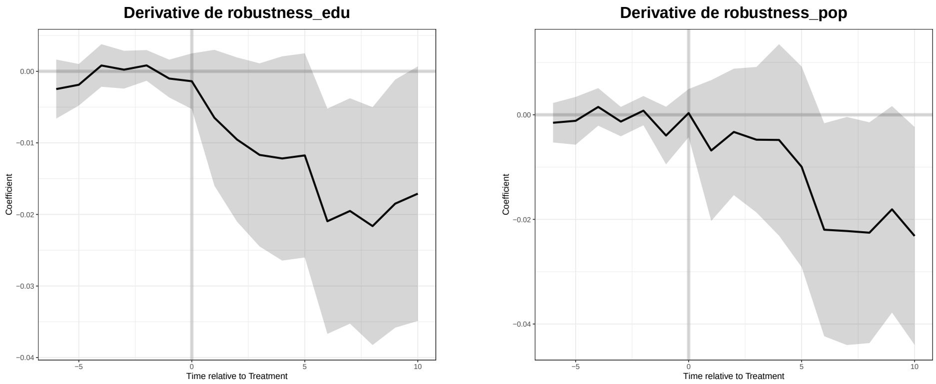
(b) Aggregate "kao" for emerging economies

Figure 6.1: Robustness check: the dynamic cumulative average treatment effects (ATT) of aggregate CFMs on wealth inequality; different conditioning factors

Note: We show the results on (a) aggregate CFMs "ka" for advanced economies using average education (left), and population growth (right), (b) aggregate outflow CFM "kao" for emerging economies using average education (left), and population growth (right). The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT are estimated using IFEct.



(a) Money market “mm”



(b) Derivative “de”

Figure 6.2: Robustness check: the dynamic cumulative average treatment effects (ATT) of asset-specific CFMs on wealth inequality; different conditioning factors

Note: We show the results of CFMs on (a) money market “mm” using average education (left), and population growth (right), and (b) derivatives “de” using average education (left), and population growth (right). The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT are estimated using MC.

6.3 Other possible factors driving wealth inequality

Teixeira (2023) and Furceri et al. (2018) include fiscal policy and monetary policy in their analyses. We do not include the two additional variables in our main analysis because it drastically reduces our sample size. In this section, however, we use government subsidies as a proxy for fiscal policy following Teixeira (2023) and broad money as a proxy for conventional monetary policy based on McPhail (2000).[19]

[19] Broad money may not be an ideal indicator for describing monetary policy. However, considering the difficulty of obtaining the policy rate for 100 economies, we still choose broad money as our policy variable. Moreover, McPhail (2000) contends that broad money is a useful indicator of inflation over long horizons

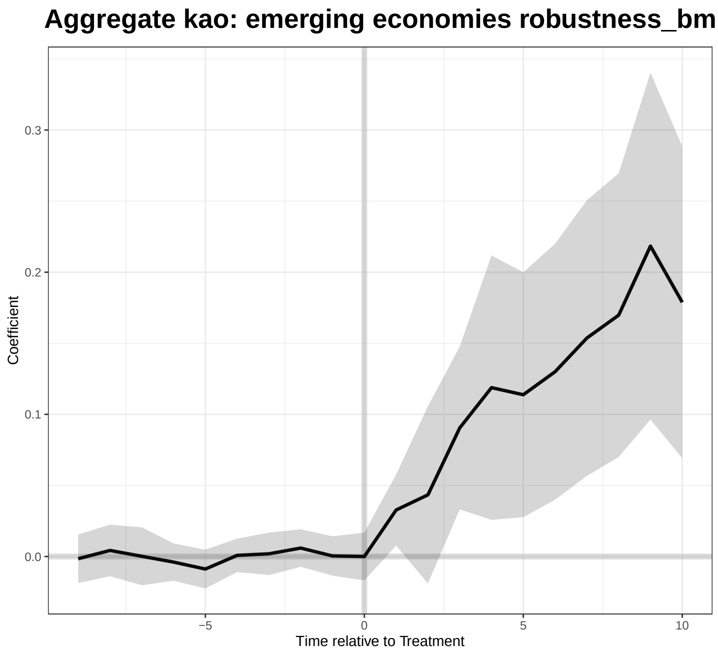
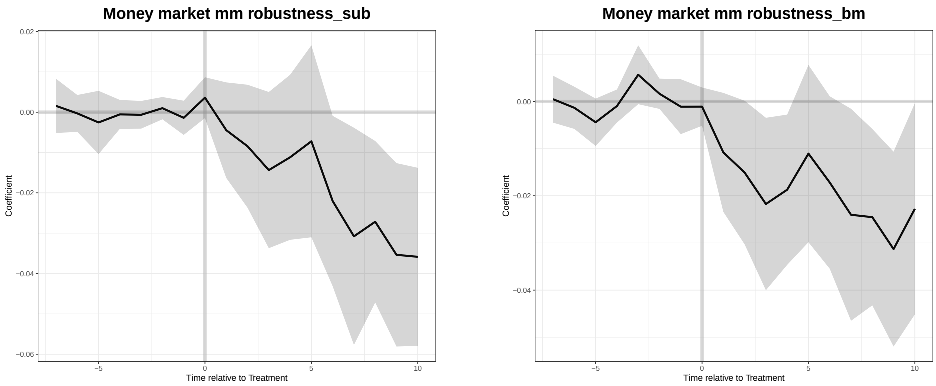


Figure 6.3: Robustness check: The dynamic cumulative average treatment effects (ATT) of outflow CFMs "kao" in emerging economies; the additional conditioning variable (broad money) case

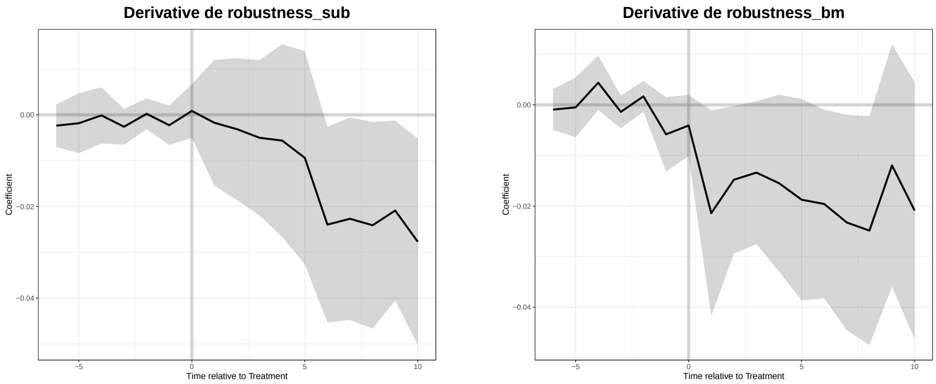
Note: We show the statistically significant results for outflow CFMs "kao" in emerging economies using additional conditioning variable (broad money). The horizontal axis represents the relative time before and after implementing a CFM at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficient of the average effect ATT is estimated using IFEct.

Although we include the broad money, as shown in Figure 6.3, the outflow CFMs "kao" in emerging economies exhibit strong and significant consistency with the results shown in Figure 5.3 in Section 5.1. Similarly, as shown in Figure 6.4a and 6.4b, the asset-specific CFMs on money market "mm" and derivative "de" also exhibit strong consistency with Figures in 5.4a and 5.4b in Section 5.3.

and thus serves as a practical monetary policy tool.



(a) Money market "mm"



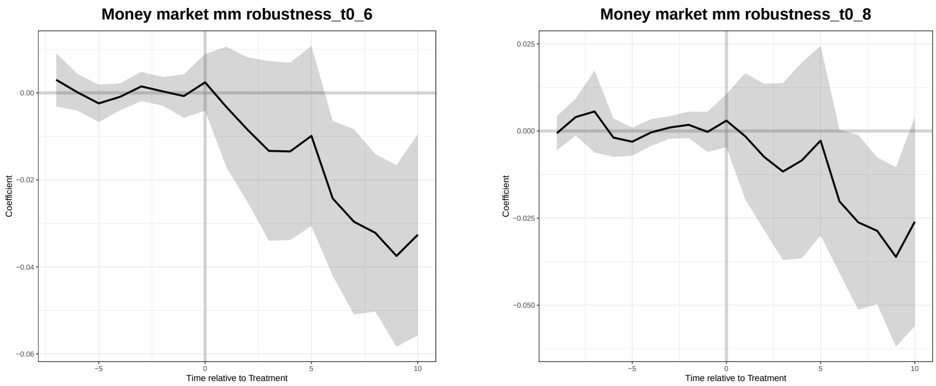
(b) Derivative "de"

Figure 6.4: Robustness check: the dynamic cumulative average treatment effects (ATT) of asset-specific CFMs; the additional conditioning variables

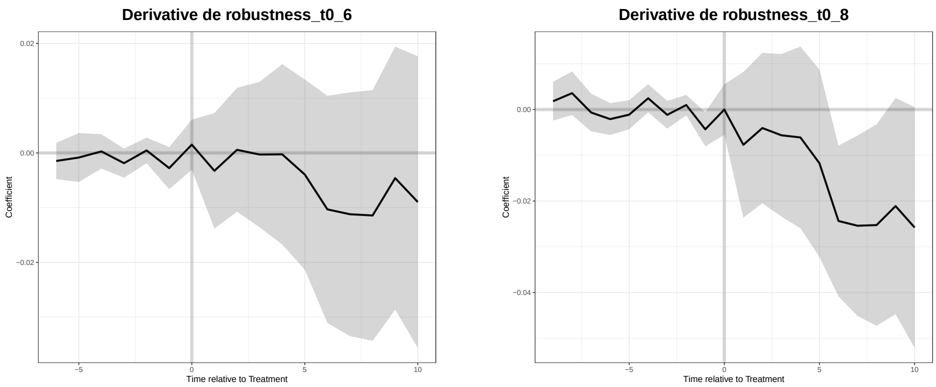
Note: We show the results on CFMs on (a) money market "mm" using additional government subsidies (left) and broad money (right) and (b) derivatives "de" using additional government subsidies (left) and broad money (right). The horizontal axis represents the relative time before and after implementing capital control at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT are estimated using MC.

6.4 Different pre-treatment periods

As mentioned in Section 4.3, the pre-treatment periods are crucial for estimating the counterfactual effects. Too few pre-treatment periods might prevent us from determining whether the specific estimator can pass the equivalence test, and if it is the case, it is not appropriate to use them for estimation. On the other hand, setting too long pre-treatment periods may limit the number of treated economies in the sample, because the treated units with fewer pre-treatment periods than specified are excluded in estimations. Therefore, selecting an appropriate number of pre-treatment periods is essential for accurate estimations. As a robustness check, we choose pre-treatment periods of 6 or 8 close to the setting of 7 in the main analysis. We confirm that asset-specific CFMs on the money market "mm" and derivatives "de" tend to decrease wealth inequality across different pre-treatment periods as shown in Figure 6.5a and 6.5b.



(a) Money market "mm"



(b) Derivative "de"

Figure 6.5: Robustness check: the dynamic cumulative average treatment effects (ATT) of asset-specific CFMs; different pre-treatment periods

Note: We show the results on (a) money market "mm" using pre-treatment period 6 (left) and 8 (right) years and (b) derivatives "de" using pre-treatment period 6 (left) and 8 (right) years. The horizontal axis represents the relative time before and after implementing CFMs at T_0; the vertical axis represents the estimated coefficient of the average effect ATT. The solid black line represents the average treatment effect; the gray area shows the 90% confidence interval. Standard errors are based on 1000 parametric bootstraps at the country level. The coefficients of the average effect ATT are estimated using MC.

7 Concluding Remarks

In this study, we have examined the effects of various CFMs on wealth equality. Firstly, we examine the effects of aggregate CFMs on wealth equality using the full sample. Although the result suggests that tightening CFMs reduces wealth inequality, we can not obtain a statistically significant result in this case (Figure 5.1). We next separate the full sample into advanced and emerging economies, and then examine the effects of aggregate CFMs on wealth equality in each case. In this case, as for advanced economies, we obtain a statistically significant result indicating that tightening CFMs increases wealth inequality (Figure 5.2). We further differentiate between capital inflows and outflows, and examine the effects of inflow and outflow CFMs on wealth inequality in each case. In this case, as for emerging economies, we obtain a statistically significant result indicating that tightening outflow CFMs increases wealth inequality (Figure 5.3). Using the more granular indices of CFMs by Fernández et al. (2016b), we examine the effects of asset-specific CFMs on wealth inequality and find that tightening CFMs in the asset markets of money market and derivative reduce wealth inequality, which are statistically significant (Figure 5.4). Differentiating between inflow and outflow further, we find that tightening asset-specific inflow CFMs significantly reduce wealth inequality in the assets of equity, money market, and direct investment, while they significantly increase that in the bond market (Figure 5.5). As for outflows, we obtain statistical significant results indicating that tightening CFMs in the asset markets of financial credit, direct investment, and real estate increase wealth inequality, whereas that in the derivative market reduces wealth inequality (Figure 5.6).

We next examine how CFMs change the wealth distribution in order to fully comprehend the linkage between CFMs and the wealth inequality (i.e., Gini coefficients). We obtain a statistically significant result showing that tightening CFMs reduces the share of the bottom 50% group in advance economies (Figure 5.7a), which explains the rise in the wealth inequality in advance economies shown in Figure 5.2. We also find a statistically significant result indicating that tightening outflow CFMs increases the wealth share of the top 10% group in emerging economies (Figure 5.7e), which explains the rise in the wealth inequality in emerging economies (Figure 5.3). Furthermore, we find a statistically significant result showing that tightening CFMs in the money market reduces the wealth shares of the top 1% and 10% groups, but increases those of middle 40% and bottom 50% groups (the first row of Figure 5.8), which explains the fall in the wealth inequality shown in Figure 5.4a. We also find a statistically significant result showing that tightening CFMs in the derivative market reduces the wealth shares of the top 10% group, but increases that of the bottom 50% group (the second row of Figure 5.8), which explains the fall in the wealth inequality shown in Figure 5.4b.

Overall, our results imply that the effects of CFMs on wealth equality and distribution are quite heterogeneous: they depend on income levels, capital flow directions, and asset categories. Our heterogeneous results are consistent with Kitano and Zhou (2022) and Zhou (2024)'s finding that the estimated effects of CFMs are distinct for different capital flow types and flow directions.

Despite the robust analysis and significant findings, our study has a certain limitation. Although we show that the effects of CFMs on wealth equality and distribution depend on income levels, capital flow directions, and asset categories, we do not have fully convincing explanations for the heterogeneous results. Future research should aim to address the transmission mechanism in each specific case.

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