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Capital controls checkup: Cases, customs, consequences

Stefan Goldbach

(Deutsche Bundesbank)

Volker Nitsch

(Technische Universität Darmstadt and CESifo)

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Deutsche Bundesbank, Wilhelm-Epstein-Straße 14, 60431 Frankfurt am Main,
Postfach 10 06 02, 60006 Frankfurt am Main

Tel +49 69 9566-0

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Non-technical summary

Research Question

In view of frequent and widespread government interventions to control capital flows, with administrative measures often varying in detail, scope and duration, there is considerable discussion about the effectiveness of such policies. In this paper, we ask: Are capital control measures successful in reducing cross-border financial transactions?

Contribution

We analyze this question by using highly disaggregated data on German financial flows from the German balance of payments statistics for the period from 1999 through 2017. Our data allows us to substantially deviate from existing work in this area. While some studies use large panel datasets, the results may suffer from aggregation bias. In our analysis, we identify the effect of (foreign) capital account restrictions by exploring the universe of entries in the capital account of the balance of payments statistics of a single, large open economy: Germany. Since each statistical entry contains information on a German declarant's monthly activities in a given country and asset category, we are able to differentiate between various types of (foreign) regulatory measures, allowing us to go beyond a single aggregate index variable. Moreover, another typical concern of empirical analyses of the effect of capital controls on economic outcomes is reverse causality. Such endogeneity problems, however, may be less of an issue when the capital flows of just a single (counterpart) country are analyzed.

Results

We find that capital control restrictions sizably reduce cross-border financial transactions, with particularly large effects for stricter measures. Moreover, control measures affect both the extensive and the intensive margin of bilateral financial relationships. Even episodic controls, which are transitory and usually targeted toward particular categories of assets, put measurable limits on a country's capital flows with Germany if they are imposed on a wide range of assets. Finally, capital controls have also been imposed by some European Union member countries (in Cyprus between March 2013 and April 2015; in Greece between June 2015 and September 2019) which significantly reduced bilateral cross-border financial flows with Germany.

Nichttechnische Zusammenfassung

Fragestellung

Angesichts häufiger und weitreichender staatlicher Interventionen bei grenzüberschreitenden Kapitalströmen gibt es eine intensive Diskussion über die Effektivität von solchen Politikmaßnahmen, deren administrative Ausgestaltung häufig in Detail, Reichweite und Länge variiert. In diesem Forschungspapier fragen wir: Reduzieren Kapitalverkehrskontrollen grenzüberschreitende Finanztransaktionen?

Beitrag

Wir untersuchen disaggregierte Finanztransaktionen für den Zeitraum von 1999 bis 2017 und legen dabei Angaben der deutschen Zahlungsbilanzstatistik zugrunde. Unsere Daten ermöglichen es, erheblich von der bestehenden Literatur abzuweichen. So könnten z.B. bei Arbeiten, die Paneldatensätze verwenden, die Ergebnisse durch das Aggregationsniveau der Analyse verzerrt sein. Wir identifizieren die Auswirkungen von Kapitalverkehrskontrollen, indem wir alle Meldungen der Kapitalbilanz einer einzelnen großen offenen Volkswirtschaft, Deutschland, untersuchen. Da der deutsche Melder die monatlichen Transaktionen innerhalb eines Landes und einer Vermögenskategorie angibt, können wir verschiedene Arten von Kapitalverkehrskontrollen untersuchen, welche über die Verwendung eines aggregierten Indexes hinausgehen. Ein weiteres Problem der empirischen Literatur ist die umgekehrte Kausalität. Solche Endogenitätsprobleme dürften bei einem einzelnen Partnerland bilateraler Finanztransaktionen weniger problematisch sein.

Ergebnisse

Wir zeigen, dass Kapitalverkehrskontrollen die grenzüberschreitenden Kapitalströme erheblich reduzieren, wobei striktere Regime stärker wirken. Darüber hinaus beeinflussen Kapitalverkehrskontrollen sowohl den extensiven als auch den intensiven Rand bilateraler Finanzbeziehungen. Selbst temporäre und gezielte Kontrollen reduzieren deutsche Kapitalströme, sobald genügend Vermögenskategorien betroffen sind. Auch einige Mitgliedstaaten der Europäischen Union haben während der Finanzkrise temporär Kapitalverkehrskontrollen eingeführt (in Zypern zwischen März 2013 und April 2015; in Griechenland zwischen Juni 2015 und September 2019), welche die bilateralen Kapitalströme mit Deutschland signifikant reduzierten.

Capital Controls Checkup: Cases, Customs, Consequences¹

Stefan Goldbach

Volker Nitsch

Deutsche Bundesbank

Technische Universität
Darmstadt and CESifo

Abstract

This paper examines the effect of administrative restrictions on cross-border capital transactions. Using highly disaggregated data from the German balance of payments statistics for the period from 1999 through 2017, we document several stylized facts about the effectiveness of such capital control policies introduced by other countries. Capital controls are associated with economically and statistically significant declines in capital flows; they affect bilateral financial relationships along both the extensive and the intensive margin. Capital controls have also been effective in reducing capital flows within the European Union.

JEL Classification Codes: F32; F36; F38

Keywords: restriction; capital flow; transaction

¹ Corresponding author. Contact address: Hochschulstrasse 1, 64289 Darmstadt. Phone : +49-6151-16 57261. E-Mail: nitsch@vwl.tu-darmstadt.de. We thank an anonymous referee, Ulrich Grosch, Stephan Jank, Axel Jochem and participants at the Bundesbank research seminar for helpful discussions and comments. Discussion Papers represent the authors' personal opinions and do not necessarily reflect the views of the Deutsche Bundesbank or the Eurosystem.

1. Introduction

Many countries impose restrictions on cross-border financial transactions. According to the Chinn-Ito index, a popular and regularly updated index of a country's degree of capital account openness, only 51 of 174 countries (~29%) allow for a free movement of capital and payments across its national borders.² Moreover, once restrictions are adopted, they often remain in place for longer periods of time. Eichengreen and Rose (2014), for instance, report that the majority of countries with capital controls in 1996 also had such controls, more than a decade later, in 2012.³

In view of frequent and widespread government interventions to control capital flows, with administrative measures often varying in detail, scope and duration, there is considerable discussion about the effectiveness of such policies. Since the ultimate aim of these restrictions is to improve a country's economic performance, studies typically focus on the effects of the country which imposes them. Klein (2012), for example, argues, from a macroeconomic perspective, that episodic capital controls do not sizably affect the growth of financial variables, the real exchange rate, or GDP growth. Other studies report more encouraging results. Erten and Ocampo (2016), for instance, find that capital account regulations help reduce macroeconomic instability and, therefore, may be useful countercyclical policy instruments. An excellent overview of the recent literature is available in Erten, Korinek and Ocampo (2020).

Our goal in this paper is more modest. In particular, we ask: Are capital control measures successful in reducing cross-border financial transactions? Specifically, we argue that findings of a measurable decline in the international flow of financial capital provide direct evidence that capital controls have indeed effects as intended, that is, disengaging a country from the world capital market.

We analyze this question by using highly disaggregated data on German financial flows from the German balance of payments statistics for the period from 1999 through 2017. Our data allows us to substantially deviate from existing work in this area. While some studies use large panel datasets, the results may suffer from aggregation bias. In practice, regulations of the capital account often vary considerably in detail, making the quantification of such policies difficult. In fact, Erten, Korinek and Ocampo (2020, p. 27) argue that “the

² The latest version of the dataset contains data up to the year 2017; see http://web.pdx.edu/~ito/Chinn-Ito_website.htm.

³ The percentage of 1996 controls persisting in 2012 varies by asset category, but is always sizably above 50%. For instance, for capital market securities, 127 countries had control measures in 1996, 116 (~91%) of which had such controls in 2012.

differences in measurement continue to play a major factor in explaining why studies reach different conclusions for testing the effectiveness of capital controls”. Other analyses provide case studies which face the problem of limited generalizability. Moreover, often the same well-known episodes are analyzed. As emphasized by Magud, Reinhart and Rogoff (2018), there seems to be an overweight of studies focusing, for instance, on evidence from Chile and Malaysia.

In our analysis, we identify the effect of capital account restrictions by exploring the universe of entries in the capital account of the balance of payments statistics of a single, large open economy, Germany. Unlike other country studies in the literature, which typically examine the effect of capital controls for countries that actively impose them, we do not emphasize a specific country experience. Rather, we analyze granular data on cross-border financial activities in a broad range of settings. Since each statistical entry contains information on a German declarant’s monthly activities in a given country and asset category, we are able, for instance, to differentiate between various types of (foreign) regulatory measures, allowing us to go beyond a single aggregate index variable. Moreover, another typical concern of empirical analyses of the effect of capital controls on economic outcomes is reverse causality, with financial openness measures being taken in response to capital flows. Such endogeneity problems, however, may be less of an issue when the capital flows of just a single (counterpart) country are analyzed.⁴ Overall, we examine the variation in German capital flows in different asset classes, both across countries and over time.

Previewing our results, we find that capital control restrictions sizably reduce cross-border financial transactions, with particularly large effects for stricter measures. Moreover, control measures affect both the extensive and the intensive margin of bilateral financial relationships. Even episodic controls, which are transitory and usually targeted toward particular categories of assets, put measurable limits on a country’s capital flows with Germany if they are imposed on a wide range of assets. Finally, capital controls have also been effectively enforced within the European Union. Membership in the European Union has been occasionally used in the literature as an instrument for the (non-)use of capital controls.

⁴ The assumption that there may be reverse causality from a country’s bilateral financial relationship with Germany to the country’s overall degree of financial openness does not seem to be particularly plausible to us. According to Davis, Valente and van Wincoop (2019), the correlation of German capital flows with world gross flows is substantial, but not exceptional and close to the median in their sample of advanced countries. Cerutti, Claessens, and Rose (2019) note that most of the variation in capital flows is unexplained by global factors; see also Barrot and Serven (2018).

Our results indicate, however, that this variable is, at best, only a weak instrument for free capital mobility in econometric analyses.⁵

The remainder of the paper is organized as follows. Section 2 describes our data and methodology. The empirical results are presented in Section 3. Section 4 briefly concludes.

2. Data and Methodology

In our empirical analysis, we mainly combine information from two different datasets: financial flows data from Deutsche Bundesbank and capital controls data using different sources.

2.1 Financial Flows

Our key source of data is the Deutsche Bundesbank's balance of payments statistics. Like other central banks, the Bundesbank regularly collects national data on cross-border financial activities, allowing us to make use of a register which contains detailed information on financial transactions between Germany and the rest of the world. In view of the sensitivity of the business information involved, the data are confidential and only accessible in anonymized form at the headquarters of the Bundesbank in Frankfurt, Germany.

For our purposes, this dataset has at least two notable advantages. First, transactions are recorded at granular detail. Each data entry contains an identifier of the reporting unit, the value and the partner country of the transaction as well as information on the type of asset that is transferred (bonds, commercial paper, stocks, investment certificate, equity capital, credit and other capital). The analysis covers purchases and sales of foreign assets by domestic residents as well purchases and sales of domestic assets by foreign residents.⁶ Second, the dataset is complete. All individuals, firms and financial institutions located in Germany are required to report cross-border payments in excess of 12,500 € to the Deutsche Bundesbank. While the raw data is reported at monthly frequency, covering the period from January 1999

⁵ More generally, Erten, Korinek, and Ocampo (2020) note that membership in international agreements and treaties which restrict the use of capital controls has low predictive power for the actual use of capital controls.

⁶ German outflows are defined as the purchases of foreign assets by domestic residents and the sales of German assets by foreign residents. German inflows are defined as the sales of foreign assets by German residents and the purchases of German assets by foreign residents. In our baseline analysis, therefore, we focus on the direction of the payment stream. However, in a robustness check, we also examine purchases of assets separately, thereby focusing on the instrument.

to December 2017, we compute annual aggregates to bring the data in line with the frequency of the capital controls measure.⁷

Table 1 provides a more detailed description of the data on financial flows and their characteristics. Summary statistics are presented at the country-year level, separately for the full sample and the (sub-)sample of non-European Union (EU) countries since EU membership typically allows for the free movement of capital between member states (Article 63 TFEU) (see, for instance, Ostry, Ghosh, Chamon, and Qureshi, 2012). As shown, German financial relationships are indeed more intense with European Union countries; transactions are, on average, larger, more numerous, and more diverse. Remarkably, however, differences in the composition of capital flows across the two country groups seem to be small. A typical bilateral relationship is characterized by transactions in a broad range of financial instruments, reported by a sizable number of German declarants, with most activities involving dealings in bonds and equities.

2.2 Capital Controls

We match German financial flows with data on capital control restrictions in the country of the counterparty, taken from Fernández, Klein, Rebucci, Schindler, and Uribe (2016). Using information from the International Monetary Fund's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER), Fernández, Klein, Rebucci, Schindler, and Uribe (2016) construct various indicators of de jure restrictions on international capital movements. Most notably, they provide measures of capital controls separately for inflows and outflows and disaggregated by asset category, which perfectly fit the granularity of our financial flows data. Measures for the presence or absence of capital controls are reported, on an annual basis, for 100 countries over the period from 1995 through 2017.⁸ In our analysis, we often use Fernández, Klein, Rebucci, Schindler, and Uribe's (2016) aggregate indices for administrative restrictions on outflows, inflows and both types of capital flows. These indices vary continuously between zero (no restrictions) and one (prohibitively high restrictions). For individual asset categories, the index values also vary between zero and one but are of categorical nature with discrete numbers.

⁷ We refrain from aggregating the monthly capital flows data in our analysis of the effects of capital controls within the European Union (in Section 3.3) for which we do not rely on the annual capital control restrictions measure of Fernández, Klein, Rebucci, Schindler, and Uribe (2016).

⁸ The data are available online at <http://www.columbia.edu/~mu2166/fkrsu/>.

For samples of financial flows with broad time and country coverage, like ours, various alternative indicators of restrictions on cross-border financial transactions are, in principle, available from the literature.⁹ Probably the most widely used measure is Chinn and Ito's (2006) index of financial openness.¹⁰ This index offers, for empirical research, at least two favorable features, since it is highly aggregated and available for a large sample. More specifically, Chinn and Ito (2006) compute a summary indicator, collapsing information on all types of regulatory restrictions on cross-border financial transactions into a single index measure, which is provided for a total of 182 countries over the period from 1970 through 2017.¹¹ Both features are, however, of limited value to us. Examining detailed micro data from one country, Germany, we are particularly interested in individual control measures (targeted, for instance, at specific types of transactions) rather than a country aggregate, while having data for a large panel of countries and years is of much less importance.

Another alternative is the capital account openness index compiled by Jahan and Wang (2016). Similar to Fernandez, Klein, Rebucci, Schindler, and Uribe (2016), they use information from AREAER reports to compute separate indices for various categories of capital controls. However, while their measures are available for a larger sample of (164) countries (including many low-income developing countries), Jahan and Wang (2016) never updated their analysis such that their indices cover only a shorter period of time (1996-2013).¹²

In practice, apart from coverage and level of detail, differences between measures of capital controls are small. While each of the indicators seeks to quantify and aggregate very specific policy measures, with each approach having strengths and weaknesses, the raw data are obtained from the same source. Consequently, it is reassuring to note that measures of capital controls are typically highly correlated. Fernandez, Klein, Rebucci, Schindler, and Uribe (2016), for instance, compare various indicators, finding only moderate differences between them. At a more disaggregate level, Klein (2012, p. 327) reports a marked similarity in the incidence of capital controls across different asset categories.

⁹ Appendix Table 1 summarizes the key features of the coverage of selected capital controls indicators.

¹⁰ As of January 2020, the Google Scholar citations count of this article is in excess of 2,200.

¹¹ Chinn and Ito (2006) obtain their raw data from the AREAER. However, in contrast to Fernández, Klein, Rebucci, Schindler, and Uribe (2016), they use not only information on regulatory controls over current or capital account transactions, but apply a broader approach, which also takes information on the existence of multiple exchange rates and the requirements of surrendering export proceeds into account.

¹² Another notable difference is Jahan and Wang's (2016) exclusive focus on the binary (yes/no) coding for the existence of restrictions in the AREAER reports. Fernández, Klein, Rebucci, Schindler, and Uribe (2016), in contrast, code predominantly on the basis of the narrative information about controls contained in the AREAER.

2.3 Methodology

In our baseline empirical analysis, we examine the effect of a country's capital account restrictions on its cross-border capital flows with Germany. In particular, we estimate variants of the following fixed effects model:

$$(1) \quad \text{Log}(\text{Flow}_{\text{dact}}) = \alpha + \beta \text{CapitalControls}_{\text{ct-1}} \{+ \gamma X_{\text{ct-1}}\} + \eta_{\text{dac}} + \phi_t + \varepsilon_{\text{dact}}$$

where Flow_{ct} is a measure of the financial activity of German declarant d in asset a with country c at time t , $\text{CapitalControls}_{\text{ct}}$ is the capital controls index for country c from Fernandez, Klein, Rebucci, Schindler, and Uribe (2016), X is a country-specific vector of auxiliary control variables, and we include a full set of declarant-asset-country-specific (η_{dac}) and time-specific (ϕ_t) fixed effects. Following the literature (e.g., Klein, 2012), we use lagged values of capital controls to (at least partially) address the issue of possible endogeneity of capital control measures. Moreover, declarant-asset-country fixed effects control for all factors that affect a German declarant's financial transactions with a partner in a particular asset which are constant over time (including, for instance, cross-country differences in EU membership), while year fixed effects control for any variation in German financial flows that is common to all partners (including, for instance, the decline in cross-border financial activities during the global financial crisis). The coefficient of interest to us is β , the effect of (foreign) capital controls on cross-border financial activities; a negative and significant coefficient indicates that (foreign) capital controls are associated with fewer financial interactions between Germany and their foreign counterparts, holding other things constant.

In our estimations, we experiment, with moderate success, with the additional inclusion of various (lagged) macroeconomic controls. In particular, following Besedeš, Goldbach and Nitsch (2017), we use real GDP per capita, stock market capitalization, real GDP growth and public debt to GDP.

Throughout the analysis, we estimate regressions with OLS and apply cluster-robust Huber-White standard errors. Also, we analyze the data at the country-year level to reduce the amount of noise, and especially the number of zero observations (i.e., observations of no flows), in the raw data.

3. Empirical Results

3.1 Consequences: Do Capital Controls Reduce Capital Flows?

We begin our analysis by examining the effect of a country's overall level of capital account restrictions on its disaggregated bilateral capital flows with Germany. In particular, making use of the granular structure of our data, we analyze the full (unbalanced) sample of annual declarant-asset-country triplets, such that each observation contains the (non-zero) value of the financial flow between a German declarant and a foreign country in a given asset category in a given year.¹³

Table 2 reports our benchmark estimation results. In the first three columns of the table, the coefficient estimates of β are tabulated for specifications of equation (1) without additional macroeconomic controls (i.e., setting $\gamma=0$). The remaining three columns present the analogues when additional control variables are included. In practice, we have no preference for any particular specification, but interpret the results in combination.

In column (1), the dependent variable is the (log of the) total (or gross) value of a German declarant's financial activity in an asset category with a country, defined as the sum of inflows and outflows. Interestingly, and in contrast to conventional findings in the literature (e.g., Magud, Reinhart and Rogoff, 2018; Erten, Korinek and Ocampo, 2020), the effect of capital control restrictions turns out to be substantial. The effect is economically sizable; the point estimate of -0.39 implies that a 10 percentage point increase in a country's overall restrictions index yields a decrease in German financial flows to/from this country by about 3 percent ($\approx(\exp(-0.39)-1)*0,1$) in the following year, holding other things constant. The effect is also statistically significant; with a robust t-statistic of 3.6, the coefficient is different from zero at any conventional level of confidence.

Columns (2) and (3) tabulate the corresponding estimates of β when we analyze German capital outflows and inflows separately. For both specifications, the coefficients are qualitatively unchanged from the previous estimation model, although the point estimates are slightly smaller in magnitude. Consequently, our finding that capital controls are associated with a measurable decline in capital flows does not seem to be dependent on the direction of the flow. In the remaining three columns of Table 2, we augment our parsimonious specification of equation (1) by adding a host of other factors that might also affect the

¹³ Our data set contains information on the cross-border financial activities of 45,077 German declarants in 104 different types of assets with counterparts in 98 countries. For a period of 19 years, a balanced sample (including zeros) would yield $(45077*104*98*19=)$ 8,729,070,896 observations.

intensity of bilateral financial relationships. In particular, it may be argued that capital flows of German declarants respond to a counterpart country's economic conditions which are captured to a large extent, but certainly not completely, by the sets of declarant-asset-country fixed effects (which take account of any time-invariant characteristics of a country) and time-specific fixed effects (which absorb factors that are common to all countries). The coefficients on the macroeconomic controls are indeed generally sensible and consistent although not always statistically significant. For instance, bilateral financial flows to/from Germany tend to increase with per capita income, stock market capitalization and real GDP growth of a country. However, sample size is reduced with this extension because some data for the control variables (especially for stock market capitalization) are missing. More notably, the capital controls effect remains negative and significant; despite this more demanding specification of equation (1), the estimated β coefficient is only marginally smaller, in both economic and statistical terms.

To summarize, our estimates indicate that a tightening (lifting) of capital controls is associated with a decrease (increase) in German financial flows with a country. This finding holds for total financial flows as well as for outflows from Germany and inflows to Germany separately. Moreover, controlling for other determinants of bilateral financial flows leaves the results qualitatively unchanged.

We check the robustness of our results along various lines. We replicate the analysis for different levels of data aggregation. A major strength of our data set is the detailed information about various aspects of cross-border financial transactions, allowing us to finely dissect the bilateral capital flows of Germany. However, at this level of (dis)aggregation, we are only able to analyze an unbalanced panel (such that there are no observations of zero flows in our sample). More importantly, the finding that capital controls reduce the volume of capital flows may be dependent on our use of disaggregated data, partly explaining the difference in results compared with previous studies. Specifically, to the extent capital controls induce offsetting changes in capital flows at the transaction level (e.g., in different asset classes), the capital controls effect may not show up in the analysis of aggregate bilateral capital flows.

Therefore, to deal with these issues, Table 3 reports estimates when we gradually reduce the number of dimensions of bilateral financial relationships in our sample. In particular, we analyze flows in (6) different asset classes instead of (>100) types of assets (declarant-asset class-country), we then aggregate flows across all assets (declarant-country), and, finally, we analyze total bilateral capital flows (country). At all levels of aggregation, we

find that capital control measures have a robust effect on reducing capital flows. The point estimates of β even tend to increase in magnitude at higher levels of aggregation (and, consequently, much fewer observations).

Table 4 tabulates results when alternative samples and/or estimation techniques are used. We begin by compiling a balanced sample at the level of country-year pairs (for 40 countries for which all data for the macroeconomic controls are available).¹⁴ Following conventional practice, then, we increase capital flows by a small value of one before taking logs which allows us to keep observations with missing or zero capital flows in our sample. While the results turn out to be weaker in this setting, this finding may be partly explained by the selection of the sample (which mainly comprises industrial countries with open capital accounts). We also experiment with Poisson pseudo-maximum likelihood (PPML) estimation.¹⁵ Using again the finely disaggregated sample of non-zero capital flows, the estimated β coefficients take consistently negative signs, although only the effects for German capital outflows are statistically significant at conventional levels.

Another estimation method which may be appropriate in our panel set-up is correlated random effects (CRE) estimation.¹⁶ Since our baseline model includes country fixed effects which account for all time-invariant factors related to a specific partner (destination/source) country, including the (average) level of the country's capital account openness over the sample period, the resulting estimates of β rely exclusively on time-series variation around dyadic means. However, in addition to such within estimation, it may also be useful to exploit variation across countries.¹⁷ Reassuringly, the cross-sectional (between) estimates obtained from a CRE estimator sensibly complement our baseline findings; the negative (although not always statistically significant) coefficients imply that Germany's cross-border financial linkages with economies closed to capital flows are less dense than those with countries with few capital control restrictions.

In Table 5, we report further robustness checks. For instance, we tabulate results for different measures of capital controls. In a comparison of their capital controls measure with other de jure indicators of aggregate capital controls, Fernández, Klein, Rebucci, Schindler,

¹⁴ The analysis of a balanced sample may be of particular relevance when capital controls are prohibitive and drive capital flows to zero.

¹⁵ PPML estimation deals with heteroskedasticity bias in multiplicative models. Often used in count models, it also easily accommodates observations of zero capital flows..

¹⁶ The CRE estimator is a general formulation of a panel regression estimator which considers the within variation and the between variation simultaneously. Under certain conditions, it transforms into the more convenient fixed effects estimator.

¹⁷ Appendix Figure 1 provides a scatter plot of the maximum value of a country's capital account restrictions index during the sample period against the corresponding minimum. The figure illustrates considerable variation in capital account openness across countries as well as over time.

and Uribe (2016) find a strong, but imperfect correlation. Therefore, to examine the sensitivity of our results to the actual quantification of capital account restrictions, we replace the capital controls measure from Fernández, Klein, Rebucci, Schindler, and Uribe (2016) (which we use in our default estimation) with the financial openness index from Chinn and Ito (2006) and the aggregate indicator from Jahan and Wang (2016). We also split our sample into subperiods and analyze the time intervals separately. None of these perturbations, however, alters the results substantially. Of the 30 reported coefficients, 27 are negative, with the large majority (18) of them being significantly different from zero. Moreover, to the extent the estimates are not significantly negative, the deviations appear reasonable and intuitive. The capital controls effect loses significance for the Chinn-Ito index (which is not disaggregated by the direction of flows) when macroeconomic controls are included and before/during the global financial crisis.

In another robustness check, we restrict our analysis to asset purchases only. For this type of cross-border financial activities, we are able to specifically explore the effects of capital control restrictions targeted on hot money flows (that is, purchases of foreign assets by German investors) and capital flight (that is, purchases of German assets by foreign residents). While the effect of capital controls on asset purchases is consistently negative, only for German capital inflows the estimates of β are statistically significant.¹⁸

Overall, we conclude that our baseline result is reasonably robust: capital account restrictions have, on average, a measurable dampening effect on the volume of capital flows.

3.2 Customs: How Do Capital Controls Work?

Interventions in the capital account can take many forms. Despite attempts of categorizations, capital control measures are typically complex in practice, and their implementation often depends on institutional details (Erten, Korinek, and Ocampo, 2020). At the same time, however, governments typically choose a combination of measures; they often pair controls. As a result, restrictions tend to apply on various types of capital flows in similar proportions. Klein (2012), for instance, finds that the share of country-year observations with capital controls is remarkably similar across asset categories; the incidence of controls ranges between 30 percent (for financial credits and collective investments) and 37 percent (for money market instruments). Moreover, inflow controls and outflow controls for a given asset are usually highly correlated (Fernandez, Klein, Rebucci, Schindler, and Uribe, 2016).

¹⁸ The results are unchanged when we analyze different subperiods.

Generally, a majority of countries has either almost no controls for any category or controls on virtually all assets—a choice which often turns out to be persistent; in Klein’s (2012) sample, 18 of 44 countries ($\approx 41\%$) have used capital controls temporarily.

In view of these different strategies, it may be argued that the sensitivity of capital flows to restrictive measures is dependent on the level of restrictions. In fact, Klein (2012) particularly emphasizes the distinction between long-term and widespread capital controls, erecting barriers like “walls”, and temporary, targeted controls, acting like “gates”, but finds that differences in the effects of long-standing and episodic capital controls are not robust.

To further analyze this issue, we follow Fernandez, Klein, Rebucci, Schindler, and Uribe (2016) and replace the capital controls index by two binary dummy variables, *lax* and *tight*.¹⁹ *Lax* takes the value of one when the index is larger than 0.1 (and zero otherwise), while *tight* takes the value of one when the index is equal to or larger than 0.7. With this definition, we are able to include both variables as controls in our analysis sequentially. The coefficient on *lax* can then be interpreted as the effect of having at least some capital controls while the coefficient on *tight* captures the (additional) effect of imposing strict measures on the cross-border flow of capital.

Table 6 presents the results. As before, we tabulate estimates of the effect of capital controls on capital flows for different directions of flows and for specifications with and without additional macroeconomic control variables. Reassuringly, the results turn out to be remarkably robust across the different specifications of equation (1). The consistently negative values of the β ’s indicate that official interventions in capital transactions by foreign countries are generally associated with a drop in German capital flows. However, while this finding holds for all types of restrictions, only coefficients on *tight* capital control measures are significantly different from zero. As a result, and in line with Klein (2012), measures imposed on a narrower set of assets, which fail to lift the average value of the restrictions index above 0.7, do not seem to be particularly effective. The results also indicate, however, that substantial changes in controls, especially if they are broadly implemented, measurably affect capital flows, questioning claims that temporary controls are likely to be ineffective (see, for instance, Klein, 2012).

¹⁹ We refrain from using the terms *gates* and *walls* because these terms implicitly link different dimensions of capital control restrictions (such as duration and coverage of asset categories), and consider them as largely synonymous, although they are not necessarily correlated with each other. More importantly, since our fixed effects estimator exploits variation over time, we ignore several types of policies (such as long-standing controls) by definition.

In a next step, we examine the effects of capital controls for different types of financial assets. Although restrictive measures affect asset categories, on average, in similar proportions, there is also considerable variation across countries and across time (Klein, 2012). Therefore, we replicate our benchmark analysis for subsamples of different asset categories in a balanced panel, making direct use of the disaggregated information on capital controls restrictions in Fernandez, Klein, Rebucci, Schindler, and Uribe (2016).²⁰

The results are reported in Table 7. Each cell in this table represents a different regression with varying combinations of regressands (direction of capital flows), samples (asset categories) and specifications (macro controls). As before, a restrictive policy stance is associated with smaller flows; 25 of the 30 tabulated coefficients take a negative sign. However, for most specifications, the estimate of β is indistinguishable from zero at any level of statistical significance. Still, it seems reassuring that a measurable effect of capital controls on flows is observed, if anything, for the two largest asset categories in German cross-border financial activities (in terms of both values and the number of entries in the balance of payments statistics), bonds and equities, as well as foreign direct investments.²¹

Our micro data also allows us to examine the mechanisms behind the capital controls effect on the volume of bilateral financial flows in more detail. In particular, we replace the value of inflows/outflows with other measures of cross-border financial interactions as regressands, analyzing the contribution of intensive and extensive margins to the aggregate effect of capital controls on financial flows separately. Specifically, following common practice (see, for instance, Besedeš, Goldbach and Nitsch, 2017), we decompose the aggregate value of German capital flows with a partner country into various factors, including the number of reporting units in Germany that declare financial transactions with that country

²⁰ From our micro data we are able to identify and match transactions in five of Fernandez, Klein, Rebucci, Schindler, and Uribe's (2016) ten asset categories. An asset category which may be of particular interest because of its growing relevance in international transactions, but is excluded from our analysis, is financial derivatives. For financial instruments in this asset category, it may not be feasible to measure transactions on a gross basis such that only net transactions are recorded (see paragraph 6.60 in International Monetary Fund, 2009). In Appendix Table 2, we assess the sensitivity of our results to this (omitted) asset category. The table reports the benchmark estimates (analogous to Table 2) when financial derivatives are additionally included in the sample; the results are, if anything, stronger with this extension. Similar to other asset categories, we also examine transactions in financial derivatives only and tabulate the results as a memo item in Table 7. For this sample, the β estimates are negative but statistically indifferent from zero.

²¹ Rather than reducing the volume of capital flows, capital controls may change the composition of cross-border financial transactions. In unreported robustness checks, however, our results remain essentially unchanged when we analyze the effectiveness of capital controls on the shares of different asset types (instead of their volumes).

(declarants) and the average value of ‘transactions’ which are defined as the number of declarant-asset-month triplets (or entries).²²

Table 8 presents the estimation results. Similar to our previous checks and exercises, the β coefficient is consistently negative. This finding indicates that capital controls tend to affect all margins of a bilateral financial relationship. However, the coefficients are not always precisely estimated; about one half of the coefficients is statistically indifferent from zero. The strongest effects, both in economic and statistical terms, are observed for capital inflows, for which the effect also survives the additional inclusion of macroeconomic control variables. Generally, the results imply that capital controls have a widespread effect on financial flows.

3.3 Cases: Capital Controls in a Currency Union

Empirical work on the effects of capital controls typically faces a number of challenges. Potential issues include, as highlighted in Erten, Korinek and Ocampo (2020), mismeasurement of capital flow restrictions due to the use of annual indices, masking short-term adjustments of capital control measures, and the endogeneity of capital controls to capital flows. Therefore, to (partially) address these issues, higher frequency data is used and an instrumental variables strategy is applied. Ostry, Ghosh, Chamon, and Qureshi (2012) argue, for instance, that membership in the European Union is a useful instrument to predict whether countries use capital controls because membership is expected to constrain (or perhaps even prohibit) the use of capital account restrictions.²³

In practice, however, two member countries of the European Union have temporarily imposed restrictive measures on capital flows during times of crisis. Cyprus adopted, in a state of emergency, capital controls legislation in March 2013, with the last remaining restrictions being lifted in April 2015. Greece imposed, in similar fashion, barriers to the movement of capital in June 2015; the final capital controls were removed in September 2019.

²² Germany’s financial relationship with a country can be described along various dimensions, in addition to the volume of capital flows, each of which may be affected differently by capital control measures. Variations in cross-border financial flows along the extensive margin include, in our sample, variations in the number of statistical entries in the German balance of payments, variations in the number of German declarants, and variations in the number of traded asset classes and asset categories. Variations along the intensive margin are variations in the capital flow per entry and variations in the average capital flow per asset class per declarant.

²³ Ostry, Ghosh, Chamon, and Qureshi (2012) use another instrument along similar lines, the existence of a bilateral investment treaty (BIT) with the United States.

In Cyprus, the government introduced severe capital controls in order to prevent a bank run when the country's banking system reopened after a lengthy period of closure. The measures included limits on daily withdrawals, credit card transactions, money transfers abroad and the cashing of cheques. Initially designed as temporary measures, to expire after seven days, the measures were continuously renewed over the next months and only gradually eased. Greece imposed capital controls in order to stem a massive capital outflow, partly driven by mistrust in the government's ability to regain solvency.²⁴ When the Governing Council of the European Central Bank finally decided not to further increase the level ("maintain the ceiling") of its Emergency Liquidity Assistance for Greek banks²⁵, the Greek government faced the serious risk of a collapse of the domestic banking system, forcing it to close banks for 20 days and to implement controls.

In the following, we use our empirical framework to analyze the effect of these capital controls on German capital flows. To do so, we modify our baseline empirical setting as follows. We analyze monthly data, allowing us to identify country-month pairs with capital controls in place. For simplicity, we use a simple capital controls dummy. Finally, we construct a balanced sample which only includes European Union member countries. For these countries, Article 63 of the Treaty on the Functioning of the European Union holds which states that: "all restrictions on the movement of capital between Member States and between Member States and third countries shall be prohibited".²⁶ Overall, despite substantial changes in the composition of our sample, sample size is only moderately reduced.

Our main results are presented in Table 9. This table is analogous to Table 2, although we now use a different set of macroeconomic control variables (available at monthly frequency).²⁷ As before, however, we tabulate estimates of β for different directions of flows and for specifications of equation (1) with and without macroeconomic controls. Still, since the capital controls have been implemented in turbulent times, we have more confidence in the estimates derived from regressions which include additional macroeconomic control variables.

²⁴ For instance, the Governing Council of the European Central Bank lifted the waiver of minimum credit rating requirements for marketable instruments issued or guaranteed by the Hellenic Republic..

<https://www.ecb.europa.eu/press/pr/date/2015/html/pr150204.en.html>

²⁵ <https://www.ecb.europa.eu/press/pr/date/2015/html/pr150628.en.html>

²⁶ See https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=uriserv:OJ.C_.2016.202.01.0001.01.ENG#C_2016202EN.01004701.

²⁷ Cyprus and Greece imposed capital controls as emergency measures in times of serious economic crisis. The effect of crisis-related (and other) factors on capital flows is, at least partly, captured by sets of time-specific fixed effects, country-specific fixed effects and country-specific macroeconomic variables.

The results strongly confirm our previous findings. German financial transactions with Cyprus and Greece, two countries with which Germany shares a common currency, the euro, fall substantially during months in which capital controls have been imposed; the effect of capital account restrictions is statistically and economically significant. More notably, the effect appears to be even considerably larger in magnitude; the point estimate of about -0.6 indicates a drop in German financial flows with these countries by almost one half (~45 percent).

Overall, capital account restrictions have also been effectively implemented within the European Union. These official interventions sizably reduced the volume of cross-border financial flows. As a result, it is questionable whether membership in the European Union generates variation in the use of capital controls that is plausibly exogenous, and its use as an instrument for free capital mobility in econometric analyses is, therefore, likely to be misguided

4. Conclusions

In this paper, we examine the effects of capital controls on cross-border financial flows. In particular, we use detailed data from the German balance of payments statistics to analyze the effects of a counterpart's capital account openness over the period from 1999 through 2017.

Our analysis focuses on various features of capital controls. Examining the consequences of such policy interventions, we find that capital controls are generally successful in limiting cross-border capital flows; stricter regulations are associated with economically and statistically significant declines in German financial activities with a country. We next review different customs and procedures of implementing controls. In line with intuition, for instance, we find that capital control measures are more effective if they are implemented on a wide range of assets. Finally, we analyze the implementation of temporary capital controls within the European Union. Finding that national legislation, justified in exceptional circumstances, restricted the free movement of capital, we argue that membership in the European Union serves, at best, only as a weak instrument for the (non-)use of capital controls in econometric analyses.

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Table 1: Descriptive Statistics

	Full Sample			Non-EU		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Total Flows (Bn. €)	1,861	107.0	409.0	1,443	26.5	138.0
Entries (Number)	1,861	597.4	1028.0	1,443	353.3	729.9
Avg. Flow per Entry (Mn. €)	1,861	46.38	109.4	1,443	18.03	43.8
Declarants (Number)	1,861	291.2	473.2	1,443	176.6	345.8
Avg. Number of Entries per Declarant	1,861	1.94	0.37	1,443	1.92	0.39
Assets (Number)	1,861	29.90	18.36	1,443	24.34	14.66
Asset Classes (Number)	1,861	5.65	0.82	1,443	5.56	0.90
Avg. Flow per Asset Class per Declarant (Mn. €)	1,861	2.08	5.04	1,443	1.15	4.13
Assets Total Flows (Bn. €)						
– Bond	1,817	48.40	166.0	1,399	10.70	54.90
– Money Market	1,609	18.30	73.30	1,197	5.67	49.40
– Equity	1,821	35.60	189.0	1,403	9.27	51.40
– Collective Investment	1,849	3.65	21.80	1,431	0.76	6.62
– Foreign Direct Investment	1,662	4.13	18.80	1,252	1.48	7.82
– Other	1,754	1.93	9.08	1,337	0.33	1.71
Assets Entries (Number)						
– Bond	1,817	229.5	407.7	1,399	130.1	241.9
– Money Market	1,609	25.86	50.98	1,197	13.36	27.45
– Equity	1,821	125.4	209.8	1,403	83.90	174.7
– Collective Investment	1,849	79.46	135.8	1,431	50.85	101.0
– Foreign Direct Investment	1,662	99.42	182.8	1,252	61.13	153.6
– Other	1,754	64.29	152.0	1,337	33.49	107.2
Assets Declarants (Number)						
– Bond	1,817	122.3	202.1	1,399	71.24	119.1
– Money Market	1,609	18.72	32.58	1,197	10.52	19.25
– Equity	1,821	88.05	137.2	1,403	61.37	116.5
– Collective Investment	1,849	47.75	75.93	1,431	31.90	58.16
– Foreign Direct Investment	1,662	87.04	158.0	1,252	54.04	133.6
– Other	1,754	59.04	136.7	1,337	31.37	98.29

Notes: The unit of observation is a country-year pair. Total flows refer to the sum of inflows and outflows.

Table 2: The Effect of Capital Controls on Cross-Border Financial Flows

	Baseline			With Macro Controls		
	Total Flows	Outflows	Inflows	Total Flows	Outflows	Inflows
Capital Control Restrictions	-0.386*** (0.107)			-0.300*** (0.110)		
Restrictions on Capital Inflows		-0.341*** (0.119)			-0.200** (0.086)	
Restrictions on Capital Outflows			-0.264*** (0.086)			-0.226** (0.088)
Log GDP per Capita				0.190*** (0.071)	0.325*** (0.064)	0.124 (0.086)
Stock Market Capitalization				0.001* (0.001)	0.002*** (0.001)	0.001 (0.001)
Real GDP Growth				0.005* (0.003)	0.007** (0.003)	0.004 (0.003)
Public Debt				-0.001 (0.001)	-0.001 (0.002)	-0.001 (0.002)
Observations	1,067,969	783,020	719,432	975,559	718,066	653,574
Adj. R2	0.01	0.01	0.01	0.01	0.01	0.01

Notes: OLS estimation. The dependent variable is specified at the top of each column (in logs). The unit of observation is a (non-zero) declarant-asset-country triplet in annual frequency. The sample period is from 1999 to 2017. Declarant-asset-country fixed effects and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Table 3: Aggregation

Baseline	Total Flows			Outflows			Inflows		
	Declarant-Asset class-Country	Declarant-Country	Country	Declarant-Asset class-Country	Declarant-Country	Country	Declarant-Asset class-Country	Declarant-Country	Country
Capital Control Restrictions	-0.450*** (0.133)	-0.538*** (0.198)	-1.229*** (0.468)	-0.374*** (0.140)	-0.384** (0.159)	-0.730 (0.516)	-0.309*** (0.111)	-0.402** (0.175)	-1.178*** (0.396)
Observations	719,735	523,210	1,762	526,331	383,666	1,757	476,854	338,643	1,759
Adj. R2	0.01	0.03	0.18	0.01	0.02	0.17	0.02	0.05	0.16

With Macro Controls	Total Flows			Outflows			Inflows		
	Declarant-Asset class-Country	Declarant-Country	Country	Declarant-Asset class-Country	Declarant-Country	Country	Declarant-Asset class-Country	Declarant-Country	Country
Capital Control Restrictions	-0.379*** (0.142)	-0.495** (0.213)	-0.952** (0.391)	-0.249** (0.103)	-0.212* (0.126)	-0.569 (0.444)	-0.278** (0.117)	-0.389** (0.183)	-0.861*** (0.316)
Observations	657,745	477,545	1,344	483,452	352,086	1,343	432,375	305,812	1,343
Adj. R2	0.01	0.03	0.17	0.01	0.02	0.17	0.02	0.05	0.14

Notes: OLS estimation. The dependent variable (in logs) and the unit of observation are specified at the top of each column. The sample period is from 1999 to 2017 in annual frequency. Relevant fixed effects (dependent on the level of aggregation) and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Table 4: Alternative Estimation Techniques

	OLS (Balanced Sample)					
	Baseline			With Macro Controls		
	Total Flows	Outflows	Inflows	Total Flows	Outflows	Inflows
Capital Control Restrictions	-0.964* (0.494)	-0.643 (0.655)	-0.565 (0.494)	-0.676 (0.440)	-0.441 (0.586)	-0.216 (0.496)
Observations	720	720	720	720	720	720
Adj. R2	0.06	0.05	0.06	0.12	0.11	0.06

	Poisson					
	Baseline			With Macro Controls		
	Total Flows	Outflows	Inflows	Total Flows	Outflows	Inflows
Capital Control Restrictions	-0.695 (0.568)	-1.556*** (0.502)	-0.218 (0.384)	-0.540 (0.535)	-1.306* (0.749)	-0.186 (0.363)
Observations	890,734	831,451	815,274	808,654	755,531	737,599

	Correlated Random Effects					
	Baseline			With Macro Controls		
	Total Flows	Outflows	Inflows	Total Flows	Outflows	Inflows
Capital Control Restrict. (Within)	-0.392*** (0.129)	-0.301** (0.147)	-0.286*** (0.088)	-0.327** (0.136)	-0.225* (0.127)	-0.233** (0.094)
Capital Control Restrict. (Between)	-1.079*** (0.325)	-0.944*** (0.326)	-1.276*** (0.284)	-0.351 (0.370)	-0.370 (0.319)	-0.350 (0.372)
Observations	1,067,969	783,020	719,432	975,559	718,066	653,574

Notes: The estimation technique and the dependent variable (in logs, except for Poisson) are specified at the top of each column. The sample period is from 1999 to 2017 in annual frequency. Relevant fixed effects (dependent on the level of aggregation) and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Table 5: Further Robustness

Baseline	Total Flows	Outflows	Inflows
Chinn-Ito	-0.286*** (0.089)	-0.281*** (0.087)	-0.291*** (0.097)
Jahan-Wang	-0.294** (0.121)	-0.377*** (0.103)	-0.183** (0.075)
1999-2004	-0.538*** (0.177)	-0.434** (0.167)	-0.516** (0.230)
2005-2010	-0.013 (0.135)	-0.065 (0.145)	0.079 (0.098)
2011-2017	-0.341*** (0.117)	-0.084 (0.185)	-0.245*** (0.073)
Purchases of domestic residents (outflows)/purchases of foreign residents (inflows)		-0.068 (0.088)	-0.069** (0.027)

With Macro Controls	Total Flows	Outflows	Inflows
Chinn-Ito	-0.157 (0.102)	-0.108 (0.102)	-0.173 (0.109)
Jahan-Wang	-0.184 (0.118)	-0.248*** (0.088)	-0.155** (0.078)
1999-2004	-0.346*** (0.098)	-0.329*** (0.116)	-0.286* (0.165)
2005-2010	-0.036 (0.123)	-0.040 (0.124)	0.008 (0.095)
2011-2017	-0.272* (0.153)	0.168 (0.193)	-0.241*** (0.088)
Purchases of domestic residents (outflows)/purchases of foreign residents (inflows)		-0.048 (0.090)	-0.075*** (0.025)

Notes: OLS estimation. Each cell contains the coefficient from a separate regression; the regression specification is similar to the corresponding column in Table 2. The dependent variable is specified at the top of each column (in logs); the capital control restrictions measure/sample is listed in the first column. The unit of observation is a (non-zero) declarant-asset-country triplet. The sample period is from 1999 to 2017. Declarant-asset-country fixed effects and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Table 6: Lax vs. Tight

Baseline	Total Flows			Outflows			Inflows		
Lax	-0.066 (0.040)		-0.063 (0.041)	-0.050 (0.054)		-0.049 (0.054)	-0.043 (0.050)		-0.035 (0.049)
Tight		-0.103** (0.047)	-0.096** (0.046)		-0.083** (0.039)	-0.082** (0.039)		-0.196** (0.085)	-0.191** (0.083)
Observations	1,067,969	1,067,969	1,067,969	783,020	783,020	783,020	719,432	719,432	719,432
Adj. R2	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01

With Macro Controls	Total Flows			Outflows			Inflows		
Lax	-0.059 (0.038)		-0.057 (0.039)	-0.037 (0.067)		-0.037 (0.067)	-0.037 (0.052)		-0.033 (0.051)
Tight		-0.057** (0.028)	-0.052* (0.029)		-0.052*** (0.018)	-0.051*** (0.018)		-0.115 (0.083)	-0.111 (0.082)
Observations	975,559	975,559	975,559	718,066	718,066	718,066	653,574	653,574	653,574
Adj. R2	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01

Notes: OLS estimation. The dependent variable is specified at the top of each column (in logs). The unit of observation is a (non-zero) declarant-asset-country triplet in annual frequency. The sample period is from 1999 to 2017. Declarant-asset-country fixed effects and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Table 7: Results by Asset Type

Baseline	Total Flows	Outflows	Inflows
Bond	-0.051 (0.038)	-0.016 (0.012)	-0.037 (0.030)
Money Market	-0.004 (0.010)	-0.008 (0.008)	0.004 (0.007)
Equity	-0.013** (0.007)	-0.009** (0.004)	-0.006 (0.005)
Collective Investment	0.001 (0.005)	0.001 (0.004)	-0.001 (0.004)
Foreign Direct Investment	-0.003** (0.001)	-0.001 (0.001)	-0.001** (0.000)
Memo item: Financial Derivatives	-0.002 (0.003)	-0.001 (0.003)	-0.002 (0.002)

With Macro Controls	Total Flows	Outflows	Inflows
Bond	-0.094* (0.056)	-0.016 (0.018)	-0.067* (0.039)
Money Market	-0.003 (0.013)	-0.008 (0.010)	0.003 (0.009)
Equity	-0.012 (0.010)	-0.007 (0.006)	-0.006 (0.006)
Collective Investment	-0.006 (0.008)	-0.005 (0.005)	-0.005 (0.006)
Foreign Direct Investment	-0.000 (0.001)	0.001 (0.001)	-0.001 (0.001)
Memo item: Financial Derivatives	-0.003 (0.005)	-0.004 (0.004)	-0.001 (0.003)

Notes: OLS estimation. Each cell contains the coefficient from a separate regression; the regression specification is similar to the corresponding column in Table 2. The dependent variable is specified at the top of each column (in logs); the sample is listed in the first column. The unit of observation is a declarant-asset-country category triplet (balanced sample). The sample period is from 1999 to 2017. Country-declarant fixed effects and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Table 8: Extensive and Intensive Margin

Baseline	Total Flows	Outflows	Inflows
Log Total Value	-1.229*** (0.468)	-0.730 (0.516)	-1.178*** (0.396)
Log Average Value per Entry	-0.908** (0.401)	-0.578 (0.431)	-0.864** (0.341)
Log Number of Entries	-0.320** (0.124)	-0.105 (0.145)	-0.386*** (0.101)
Log Number of Declarants	-0.321** (0.127)	-0.300* (0.157)	-0.397*** (0.099)
Log Number of Asset Classes	-0.140* (0.073)	-0.061 (0.084)	-0.178** (0.077)
Log Number of Asset Categories	0.011 (0.021)	-0.007 (0.032)	-0.024 (0.028)
Log Average Value per Asset Class per Declarant	-0.918** (0.380)	-0.612 (0.418)	-0.757** (0.319)

With Macro Controls	Total Flows	Outflows	Inflows
Log Total Value	-0.952** (0.391)	-0.569 (0.444)	-0.861*** (0.316)
Log Average Value per Entry	-0.737** (0.317)	-0.474 (0.340)	-0.634** (0.266)
Log Number of Entries	-0.215 (0.149)	-0.055 (0.169)	-0.283*** (0.101)
Log Number of Declarants	-0.216 (0.154)	-0.217 (0.198)	-0.317*** (0.108)
Log Number of Asset Classes	-0.097 (0.063)	-0.056 (0.074)	-0.097* (0.052)
Log Number of Asset Categories	-0.006 (0.018)	-0.007 (0.031)	-0.012 (0.023)
Log Average Value per Asset Class per Declarant	-0.730** (0.306)	-0.526 (0.338)	-0.532** (0.245)

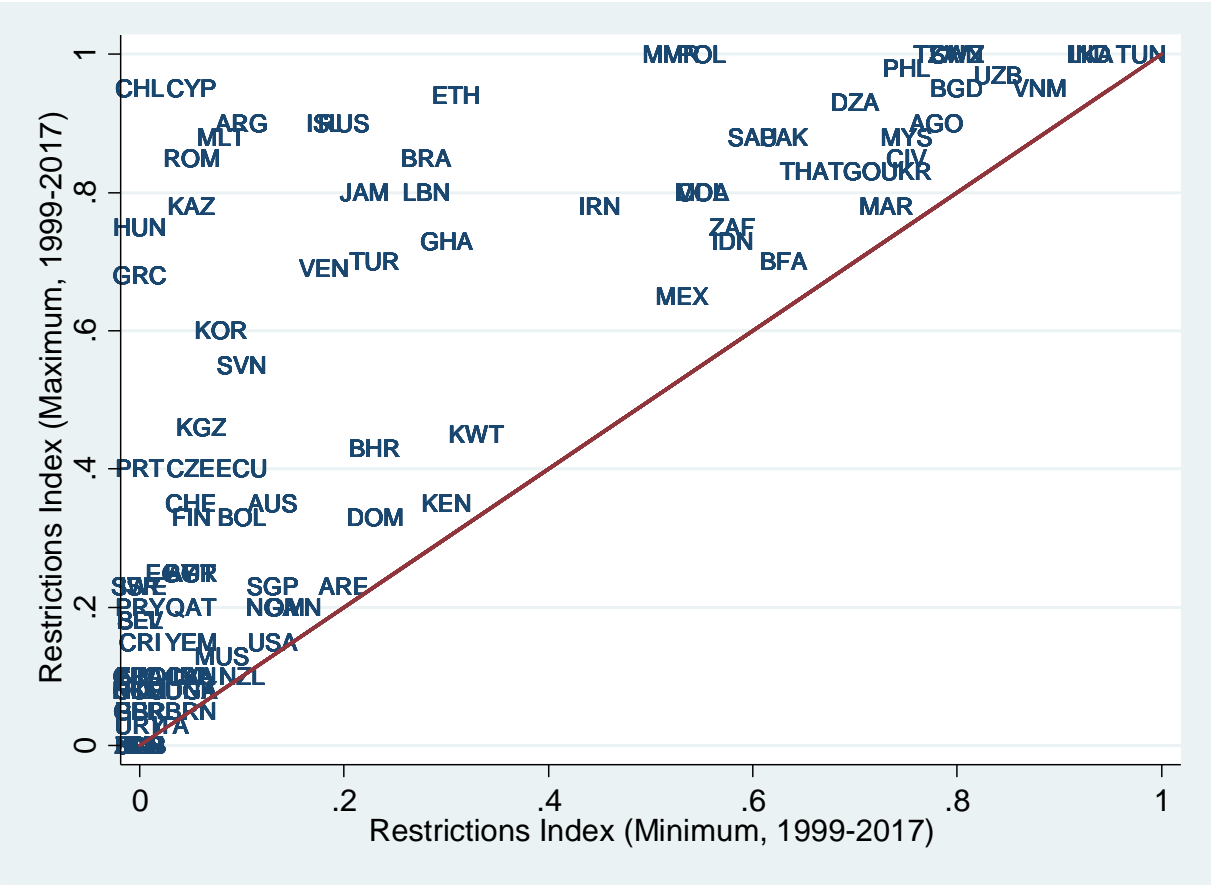
Notes: OLS estimation. Each cell contains the coefficient from a separate regression; the regression specification is similar to the corresponding column in Table 2. The dependent variable is listed in the first column; the sample is specified at the top of each column. The unit of observation is a (non-zero) country-year pair. The sample period is from 1999 to 2017. Country-specific fixed effects and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Table 9: The Effect of Capital Controls on Cross-Border Financial Flows Within the European Union

	Baseline			With Macro Controls		
	Total Flows	Outflows	Inflows	Total Flows	Outflows	Inflows
Capital Control Restrictions	-0.393* (0.211)			-0.636*** (0.074)		
Restrictions on Capital Inflows		-0.374** (0.140)			-0.503*** (0.071)	
Restrictions on Capital Outflows			-0.374* (0.200)			-0.617*** (0.075)
Real Effective Exchange Rate				-0.010* (0.006)	-0.012** (0.005)	-0.011 (0.007)
Inflation				1.343 (1.414)	1.239 (1.215)	1.042 (1.186)
Reserves				0.083 (0.098)	0.067 (0.091)	0.075 (0.088)
Long-term Bond Yield				-0.024** (0.009)	-0.023** (0.010)	-0.022** (0.009)
Asset Returns				0.321** (0.125)	0.258** (0.116)	0.173 (0.110)
Observations	640,224	640,224	640,224	397,176	397,176	397,176
Adj. R2	0.04	0.03	0.03	0.05	0.04	0.04

Notes: OLS estimation. The dependent variable is specified at the top of each column (in logs). The unit of observation is a (non-zero) declarant-asset-country triplet in monthly frequency. The sample period is from 1999 to 2017. Declarant-asset-country fixed effects and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.

Appendix Figure 1: Overall Capital Controls Restrictions Index, 1999-2017



Source: Updated data compiled from Fernández, Klein, Rebucci, Schindler, and Uribe (2016).

Appendix Table 1: Capital Control Measures

	Chinn-Ito	Jahan-Wang	Fernández, Klein, Rebucci, Schindler, and Uribe
Period	1970-2017	1996-2013	1995-2017
Countries	182	164	100
Assets	Total	Equity, Bond, Money market, Collective investment, Derivatives, Commercial credit, Financial credit, Guarantee, Direct investment, Real estate	Equity, Bond, Money market, Collective investment, Derivatives, Commercial credit, Financial credit, Guarantee, Direct investment, Real estate
Indicators	2	47	57

Source: Own compilation.

Appendix Table 2: The Effect of Capital Controls on Cross-Border Financial Flows (including Financial Derivatives)

	Baseline			With Macro Controls		
	Total Flows	Outflows	Inflows	Total Flows	Outflows	Inflows
Capital Control Restrictions	-0.402*** (0.102)			-0.313*** (0.104)		
Restrictions on Capital Inflows		-0.353*** (0.116)			-0.221*** (0.083)	
Restrictions on Capital Outflows			-0.269*** (0.078)			-0.224*** (0.080)
Log GDP per Capita				0.212*** (0.068)	0.340*** (0.064)	0.161* (0.082)
Stock Market Capitalization				0.001* (0.001)	0.001*** (0.001)	0.001 (0.001)
Real GDP Growth				0.003 (0.003)	0.005* (0.003)	0.002 (0.003)
Public Debt				-0.001 (0.001)	-0.001 (0.002)	-0.001 (0.002)
Observations	1,146,363	849,684	783,779	1,047,078	778,742	712,167
Adj. R2	0.01	0.01	0.01	0.01	0.01	0.01

Notes: OLS estimation. The dependent variable is specified at the top of each column (in logs). The unit of observation is a (non-zero) declarant-asset-country triplet in annual frequency. The sample period is from 1999 to 2017. Declarant-asset-country fixed effects and time-specific fixed effects are included but not reported. Robust standard errors (clustered by country) are in parentheses. ***, ** and * denote significant at the 1%, 5% and 10% level, respectively.