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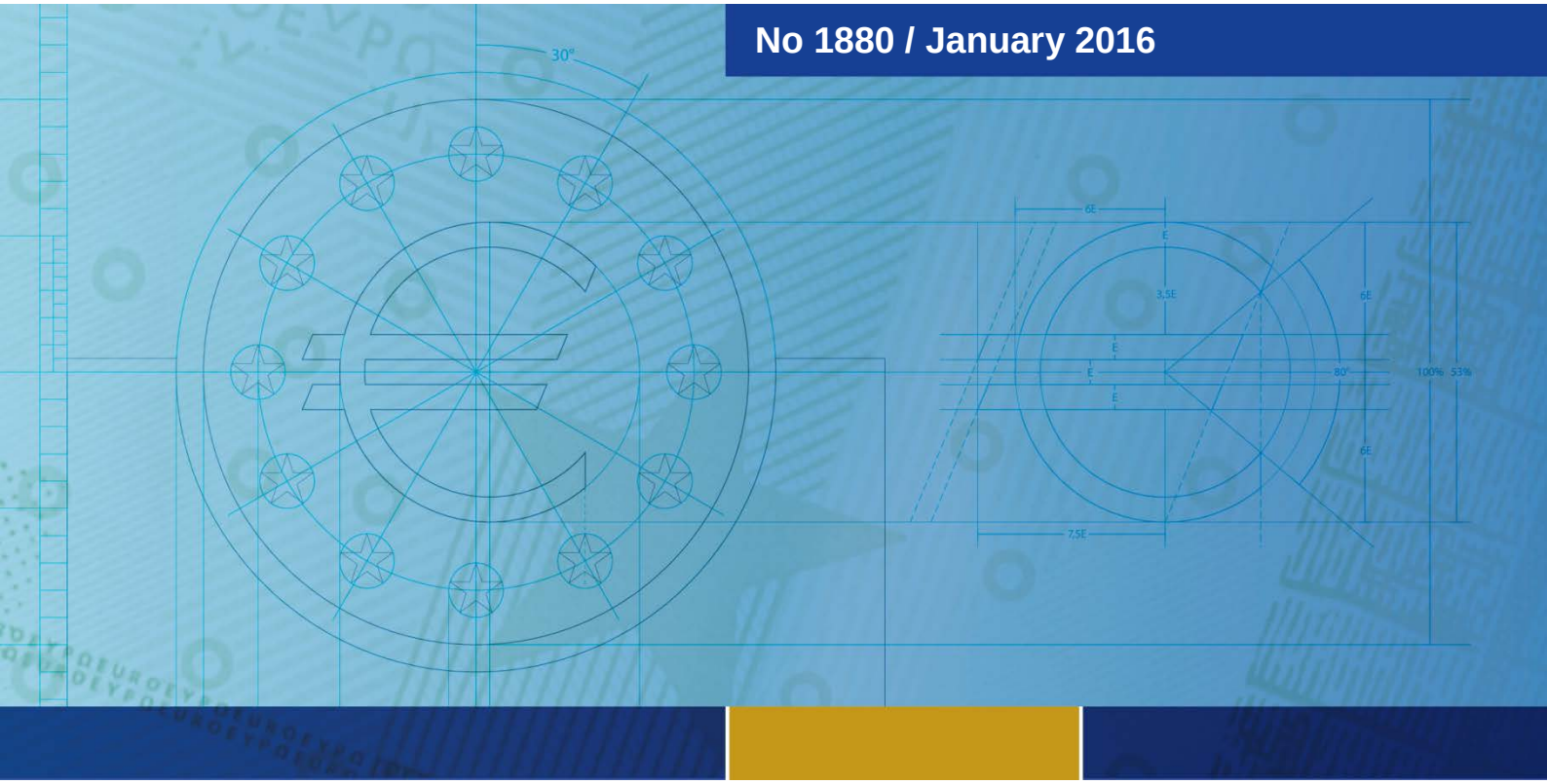
Reserve accumulation, inflation and moral hazard: Evidence from a natural experiment



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Abstract

This paper assesses whether international reserve accumulation can be inflationary because of moral hazard and incentive effects. It tests the hypothesis that an increase in international reserves may incentivise countries to become complacent and pursue less prudent policies due to the perceived safety provided by higher reserve holdings. The paper uses a unique natural experiment to solve the endogeneity problem between reserve accumulation and macroeconomic developments, namely the 2009 general allocation of Special Drawing Rights (SDR). This allocation –the first one in almost three decades– enables to cleanly trace the effect of an unanticipated, global exogenous shock to the reserve holdings of the 186 IMF member countries. Difference-in-differences and propensity score matching estimates suggest that inflation in countries receiving large SDR allocations was about half a percentage point higher in annual terms within the next two years following the allocation, controlling for the standard arguments of the Phillips curve and other determinants. This effect is commensurate to the size of discretionary fiscal deficits in these countries, which is also consistent with the hypothesis that reserve accumulation may be inflationary because of moral hazard and incentive effects.

Keyword: international reserves, moral hazard, special drawing right, natural experiment, difference-in-differences, propensity score matching estimates

JEL Classification: F30

Non-technical summary

The benefits and costs of reserve accumulation have been debated for many years. One potential cost remains poorly understood, however. To what extent does reserve accumulation give rise to macroeconomic costs at the country level? To what extent is it inflationary, in particular? These are long-standing questions, which emerged as early as the 1970s in the wake of the inflationary period that accompanied the advent of floating exchange rates.

That reserve accumulation may lead to higher domestic inflation is due to two main channels. The most well-known channel is imperfect sterilisation. An increase in reserve holdings leads to an increase in the monetary base which, if the latter is not (or not fully) sterilised, leads to higher inflation. Another channel which, hitherto, has received more limited attention, is moral hazard. To the extent that accumulating international reserves is a form of self-insurance, it may incentivise countries to become more complacent and to pursue more expansionary and less prudent policies due to the perceived safety provided by higher reserve holdings.

Identifying the inflationary effect of reserve accumulation raises conceptual and practical challenges. One challenge is to separate out the effect of the two channels. Another challenge is that reserve accumulation and macroeconomic developments are highly endogenous. The aim of this paper is to address these two challenges. It is to disentangle the effect of the moral hazard channel from that of the money supply/imperfect sterilisation channel and to identify the causal impact of reserve accumulation on inflation, as one facet of its potential macroeconomic costs.

To this end, the paper uses a unique natural experiment, namely the 2009 general allocation of Special Drawing Rights (SDRs), the synthetic currency issued by the International Monetary Fund (IMF). This allocation was the largest ever conducted by the IMF, and the first one in almost three decades. It resulted in an immediate increase in international reserves worldwide. It was truly global, insofar as all 186 member countries of IMF received SDRs in proportion of their IMF quotas. It was clearly unanticipated, as it was one of the cornerstones of the 2009 Summit of G20 Leaders in London, which surprised most observers to the upside by the boldness of its achievements. Importantly, the allocation enables to identify the moral hazard channel independently from the money supply/imperfect sterilisation channel. SDR allocations are indeed the sole example of an increase in a country's reserves which does *not* result in a simultaneous increase in the domestic monetary base. It helps solve the endogeneity problem, too, since the allocation of SDRs led to a simultaneous increase in countries' reserves which was independent from their business cycle conditions. Finally, it was also differentiated across countries, i.e. fairly large in some, and smaller in others. It is this source of heterogeneity which we also use for identification. In a nutshell, the 2009 general SDR allocation enables us to

cleanly trace the inflation effect of an unanticipated, global exogenous shock to the reserve holdings of the 186 IMF member countries.

In order to identify the incentive effect of an exogenous shock to reserves on inflation, we obtain difference-in-differences estimates and use the 2009 general SDR allocation as a treatment. We split our sample in two country groups, namely a treated group of countries for which the SDR allocation was "large" (i.e. accounting for more than 10% of their international reserves in our baseline case) and a control group of countries for which the SDR allocation was "small" (i.e. less than 10% of their international reserves). Insofar as these two groups did not have systematic differences in terms of inflation developments prior to 2009, we can identify the average "treatment" effect of the SDR allocation, i.e. the increase in inflation due to this exogenous reserve increase. In so doing, we also control for the standard Phillips curve variables, as well as for other relevant variables (e.g. sterilization operations, actual use of SDRs, existence of an IMF program, idiosyncrasies arising from euro area membership, etc.). We further control for other systematic differences between the treated and non-treated group by obtaining propensity score matching estimates.

Both our difference-in-differences and propensity score matching estimates suggest that inflation in countries receiving large SDR allocations was about half a percentage point higher in annual terms within the next two years following the allocation, controlling for the standard arguments of the Phillips curve. Moreover, this effect is commensurate to the size of discretionary fiscal deficits in these countries. Estimates conditional on the stance of fiscal policy suggest indeed that the effect is stronger in countries with large discretionary fiscal deficits and with elevated public debt levels. This is also consistent with the moral hazard channel and the hypothesis that reserve accumulation may be inflationary because of incentive effects that encourage countries to become more complacent and pursue more expansionary fiscal paths due to the perceived safety provided by higher reserves holdings.

I. Introduction

The benefits and costs of reserve accumulation have been debated for many years. The most obvious benefit, on the one hand, is that international reserves help countries self-insure against possible sudden stops in capital inflows (see e.g. Jeanne and Rancière, 2011). There is evidence, for instance, that countries with hefty war chests of reserves suffered less from the retrenchment in capital flows that accompanied the outbreak of the global financial crisis (Bussière et al., 2014).[1] Adequate reserve holdings are often seen by foreign investors as a positive signal of the soundness of fundamentals, too, with clear benefits in terms of market access and lower borrowing spreads (see e.g. Aizenman and Marion, 2004).

On the other hand, it has been argued that excessive reserve accumulation may create costs for reserve-hoarding countries, in terms of sterilisation (or opportunity) costs of holding reserves, as well as for the global economy at large.[2] This debate peaked before the 2008 global financial crisis. It was then claimed that large stockpiles of foreign reserves contributed to foster global imbalances.[3] But there is another potential cost which remains poorly understood. To what extent does reserve accumulation give rise to macroeconomic costs at the country level? To what extent is it inflationary, in particular? Admittedly, these are long-standing questions, which emerged in the early 1970s in the wake of the inflationary period that accompanied the advent of floating exchange rates (see e.g. Kelly, 1970; Williamson, 1970; Stein, 2009; Aizenman and Glick, 2009).

That reserve accumulation may lead to higher domestic inflation is due to two main channels. The most well-known channel is money supply/imperfect sterilisation. An increase in reserve holdings leads to an increase in the monetary base which, if the latter is not (or not fully) sterilised, leads to higher inflation (see e.g. Heller, 1976; Khan, 1979; Jones, 1983; Stein, 2009; Aizenman and Glick, 2009). Another channel – which, hitherto, has received more limited attention– is moral hazard. To the extent that accumulating international reserves is a form of self-insurance, it may incentivise countries to become complacent and to pursue more expansionary or unsustainable

[1] Other recent studies have looked at the role of foreign reserves as a determinant of country resilience in the wake of the Great Recession (e.g. Dominguez et al., 2012; Obstfeld et al., 2009; Frankel and Saravelos, 2010; Rose and Spiegel, 2009a, 2009b and 2011; Blanchard et al., 2010). Aizenman et al. (2014) investigate whether the global financial crisis has led to structural changes in reserve accumulation patterns.

[2] Aizenman and Marion (2003), Aizenman and Lee (2007), Cheung and Ito (2009) and Obstfeld et al. (2009, 2010) have studied the motives and consequences of "excessive" reserve accumulation since the Asian crisis in the run-up to the global financial crisis. On the costs of reserve accumulation, see also IMF (2010a) and Aizenman and Glick (2009).

[3] On this particular aspect see e.g. Caballero and Krishnamurthy (2009), Eichengreen (2009) and Portes (2009).

policies due to the perceived safety provided by higher reserve holdings (see e.g. Neumann, 1973; Mussa et al., 1996; Aizenmann and Marion, 2004).

Identifying the inflationary effect of reserve accumulation raises conceptual and practical challenges. One challenge is to separate out the effect of the two channels. Another challenge is that reserve accumulation and macroeconomic developments are highly endogenous.[4] The aim of this paper is to address these two challenges. It is to disentangle the effect of the moral hazard channel from that of the money supply channel and to identify the causal impact of reserve accumulation on inflation, as one facet of its potential macroeconomic costs.

To this end, the paper uses a unique natural experiment, namely the 2009 general allocation of Special Drawing Rights (SDRs) –the synthetic currency issued by the International Monetary Fund (IMF). This allocation was the largest ever conducted by the IMF, and the first one in almost three decades. It resulted in an immediate increase in international reserves worldwide. It was truly global, insofar as all 186 member countries of IMF received SDRs in proportion of their IMF quotas. It was clearly unanticipated, as it was one of the cornerstones of the 2009 Summit of G20 Leaders in London, which surprised most observers to the upside by the boldness of its achievements. Importantly, the allocation enables to identify the moral hazard channel independently from the money supply/imperfect sterilisation channel. As we explain in more detail below, SDR allocations are indeed the sole example of an increase in a country's reserves which does *not* result in a simultaneous increase in the domestic monetary base. It helps solve the endogeneity problem, too, since the allocation of SDRs led to a simultaneous increase in countries' reserves which was independent from their business cycle conditions. In a nutshell, the 2009 general SDR allocation enables us to cleanly trace the inflation effect of an unanticipated, global exogenous shock to the reserve holdings of the 186 IMF member countries.

In order to identify the incentive effect of an exogenous shock to reserves on inflation, we obtain difference-in-differences estimates –in the spirit of e.g. Card and Krueger (1994) and (1998) and Angrist and Krueger (1998) – and use the 2009 general SDR allocation as treatment. We split our sample in two country groups, namely a treated group of countries for which the SDR allocation was "large" (i.e. accounting for more than 10% of their international reserves in our baseline case) and a control group of countries for which the SDR allocation was "small" (i.e. less than 10% of their international reserves).[5] Insofar as these two groups did not have systematic differences in terms of inflation developments prior to 2009, we can identify the average "treatment" effect of the SDR allocation, i.e. the increase in

[4] See e.g. the detailed discussion in Khan (1979), Heller and Kahn (1978) and Bussière et al. (2014). For instance, Kahn (1979) highlights that international reserves may simply respond to, and not cause, inflation and that they could be jointly determined by third variables.

[5] Obviously we vary this threshold in robustness checks and subject our results to a large array of sensitivity checks (see below).

inflation due to this exogenous reserve increase. In so doing, we also control for the standard Phillips curve variables, as well as for other relevant variables (e.g. sterilization operations, actual use of SDRs, existence of an IMF program, idiosyncrasies arising from euro area membership, etc.). We also control for other systematic differences between the treated and non-treated group by obtaining propensity score matching estimates.

Both our difference-in-differences and propensity score matching estimates suggest that inflation in countries receiving large SDR allocations was about half a percentage point higher in annual terms within the next two years following the allocation, controlling for the standard arguments of the Phillips curve. Moreover, this effect is commensurate to the size of discretionary fiscal deficits in these countries. Estimates conditional on the stance of fiscal policy suggest indeed that the effect is stronger in countries with large discretionary fiscal deficits and with elevated public debt levels. This is consistent with the moral hazard channel and the hypothesis that reserve accumulation may be inflationary because of incentive effects that encourage countries to become more complacent and pursue more expansionary fiscal policies due to the perceived safety provided by higher reserve holdings.

The rest of the paper is structured as follows. Section II presents the experiment in more detail. Section III discusses the theoretical framework. Section IV presents the methodology and the data. Section V reviews the empirical estimates, including the baseline difference-in-differences estimates, an array of robustness checks, the propensity score matching estimates as well as estimates conditional on the fiscal stance. Section VI concludes and draws policy implications.

II. The 2009 general SDR allocation: Some stylised facts

In the wake of discussions about the future of the Bretton Woods system of fixed exchange rates in which only the US dollar was convertible into gold, the IMF decided in 1969 to create the SDR to support the system by supplementing members' international reserves and meet "the legitimate liquidity requirements of an expanding world economy" (Triffin, 1960). The issue then was an increasing shortage of gold relative to growing amounts of US dollar claims on the United States.

The original purpose of SDR allocations was hence to increase global liquidity.[6] The SDR is neither a currency, nor a claim on the IMF. It is defined as a basket of five "freely usable" currencies (the euro, the Japanese yen, the pound sterling, and the US dollar), and a potential claim on the reserves held by the central

[6] Hence, the question as to whether SDR allocations are inflationary first arose at the very moment when SDRs were created and notably on the occasion of the first general allocation in 1970-1972. For a comprehensive discussion see e.g. Boughton (2001).

banks of IMF members in these five currencies.[7] SDRs can be converted into freely usable currencies through voluntary exchanges between members. In addition, the IMF can invite members with strong external positions to purchase SDRs from members with weak external positions. The second amendment of the IMF's Articles in 1978 included the ambitious goal of making the SDR the "principal reserve asset in the international monetary system". This idea was abandoned subsequently. But it timidly resurfaced more recently.[8]

According to its Articles of Agreement, the IMF may allocate SDRs to members in proportion of their IMF quotas, which provides each member with a costless asset.[9] IMF members benefiting from SDR allocations are also members of the SDR Department. General allocations of SDRs are based on long-term needs to supplement global reserve holdings. They can be created with an 85% majority of total IMF voting power.[10] So far, only three general allocations have been made. The first one took place in 1970-1972, for a total amount of SDR 9.3 billion. The second one was in 1979-1981, for a total amount of SDR 12.1 billion. And the third one was in 2009, for a total amount of SDR 161.2 billion. No allocation was made between 1981 and 2009 in the absence of a consensus on the existence of long-term global needs to supplement existing reserve assets and on the role that the SDR could play in the international monetary system.[11]

The 2009 general allocation is therefore the first one in almost three decades. It is also the largest allocation ever made. Importantly, it was completely unanticipated. The decision of G20 Leaders to "support a new SDR allocation of $250 billion" to help mitigate the effects of the global financial crisis, at the London Summit of 2 April 2009 was indeed one of the major unexpected breakthroughs of a

[7] The 2015 IMF review of the SDR basket considered and decided its broadening to the Chinese renminbi, which could have a significant impact on the composition and volatility of the basket (see e.g. Bénassy-Quéré and Cappelle, 2014).

[8] See, for instance, the G20 Leaders' Final declaration at the Cannes Summit, 3-4 November 2011 or the statement of People's Bank of China Governor Zhou Xiaochuan in March 2009 according to which ways should be found to "increasing SDR allocation to gradually replace existing reserve currencies with the SDR". See also e.g. Angeloni et al. (2011), Boughton (2014), Farhi, Gourinchas and Rey (2011) and Obstfeld (2011).

[9] If a member's SDR holdings rise above its allocation, it earns interest on the excess, while if it holds fewer SDRs than allocated, it pays interest on the shortfall.

[10] See Article XVIII, Sec. 1(a) of the IMF Articles of Agreement (http://www.imf.org/external/pubs/ft/aa/aa18.htm). For more background information on the IMF's procedures and criteria governing general SDR allocations, in particular the 2009 general allocation, see IMF (2009).

[11] In addition to these general allocations, there was also one-time special allocation of SDRs. The purpose of this special allocation was to enable all members of the IMF to participate in the SDR system on an equitable basis and correct for the fact that countries that joined the Fund after 1981 — more than one-fifth of the current IMF membership— had never received SDRs. The Executive Board agreed in 1997 to amend the IMF's Articles of Agreement to allow for a one-time, special allocation of SDRs of SDR 22 billion, which was implemented after US Congress approval on 9 September 2009.

meeting which surprised most observers to the upside.[12] The IMF Executive Board made a proposal for an SDR allocation only two months afterwards (in June 2009) and the allocation took effect on 28 August 2009. It was truly a global shock to international reserves, insofar as all the 186 members of the IMF received SDRs. The size of the shock yet differed across countries markedly. It was fully commensurate to each member's quota, which determines the amount of resources a member can draw from the IMF as well as its voting power within the Fund, but not necessarily commensurate to existing reserve holdings, money supplies or business cycle conditions in member countries.[13] It is this source of heterogeneity which we use for identification. Although the allocation took place in the midst of the most severe global crisis since the Great Depression, it was an exogenous shock, too. SDRs were allocated indeed solely on the basis of members' IMF quotas, which are meant to reflect countries' relative position in the world economy, not on the basis of reserve adequacy or business cycles considerations (more on this below).[14]

In a nutshell, the 2009 general SDR allocation enables us to cleanly trace the inflation effect of an unanticipated, global exogenous shock to the reserve holdings of the 186 IMF member countries. Figure 1 shows that the 2009 general SDR allocation is poorly correlated with countries' reserve holdings, insofar as it is based on countries' quotas which, as aforementioned, are defined using a much broader set of variables. Figure 2 plots a measure of the depth of the Great Recession, i.e. the output gap in 2008 (defined as the deviation of real GDP growth from a linear time trend) against the allocated SDRs (scaled by reserve holdings prior to the allocation). The figure clearly shows that the two variables are uncorrelated (the correlation coefficient is 0.04 and statistically insignificant).[15] This substantiates the point that IMF members received SDRs in proportion of their relative economic positions (i.e. quotas) and not according to their business cycle conditions or to reserve adequacy considerations, hence implying that the general allocation of 2009 was truly exogenous.

Next, Figure 3 shows the kernel distribution of the 2009 general SDR allocation, expressed as a percentage of countries' international reserves in the month just prior the allocation. The red line indicates the 10%-threshold for "large"

[12] For instance, one day after the Summit's conclusion, it was noted that G20 Leaders had made "real achievements" such as "new issuance of special drawing rights (see "The first bricks in a new world order", *Financial Times*, 3 April 2009). It was further noted that "markets surged yesterday [i.e. on 2 April 2009] as world leaders agreed a sweeping package of measures […], including a $250bn increase in the international money supply" (see "Markets surge on G20 accord", *Financial Times*, 3 April 2009). A further argument in support of the view that the outcome of the G20 Summit was a surprise is that April 2009 was the month showing the largest peak in internet searches of the word "SDR" according to *Google trends*.

[13] International reserves only account for a very limited weight in the calculation of quota shares. See e.g. IMF (2010b) and Skala, Thimann and Wölfinger (2007) for more information on the IMF quota formula.

[14] See also Bénassy-Quéré and Béreau (2011).

[15] Similar results hold when using the output gap in 2009 (with a – still insignificant –correlation coefficient of 0.07).

allocations in our baseline case (which corresponds to the 60-percentile of the distribution).[16] The SDR allocation resulted in sizeable increases in the reserve holdings of a large array of countries. In extreme cases, such as Greece, Ireland, or Tajikistan, holdings doubled or trebled.

Figures 4a and 4b provide a complementary perspective and scale the 2009 general SDR allocation by narrow and broad money, respectively. For half of the IMF membership, the allocation was larger than 5% of M1 and larger than 2% of M3. This suggests that, in many cases, the magnitude of the 2009 general allocation was economically sizeable.

SDRs are used by IMF members for several reasons. The latter include transactions with the IMF (borrowing and repayment of loans, payment of the reserve asset portion of quotas and quota increases);[17] managing the currency composition of international reserves (i.e. exchanging SDRs against e.g. US dollars and euros); balance-of-payments or budgetary support; or exchanging SDRs against major reserve currencies to intervene in the foreign exchange market.[18] That SDRs can be used for such a large array of reasons makes it sometimes difficult to explain changes in the level of IMF members' holdings over long periods of time.[19]

Table A1 in the Appendix reports the initial amount of SDRs that each IMF member country held before the general allocation of August 2009 and the amounts received on the occasion of the allocation. It also reports holdings in December 2011 to give an idea of the use of the SDRs in question. Significant SDR use was restricted to a handful of countries. Only 21 of the IMF members (out of 186) used half or more of their SDR holdings. In most cases, the underlying motivations were not made public. There were a few exceptions, however, such as Bosnia and Herzegovina, Moldova, Serbia and the Ukraine.[20].

Figure 5 shows the evolution of inflation in annual terms (and percentage points) in both the treated and control groups, before and after the treatment. The difference in inflation rates between countries receiving large allocations (treated

[16] This compares with a median allocation of 7.3% of the existing stock of international reserves.

[17] SDRs are also used as a means of payment of interest within the SDR Department. For example, members receive net interest from the IMF at the SDR interest rate on the amount of SDR holdings exceeding cumulative allocations, which leads to incremental increases in a member's SDR holdings over time. Conversely, members with SDR holdings below cumulative allocations incur net interest obligations to the IMF at the SDR rate, which leads to a decrease in its holdings over time.

[18] Members may also voluntarily exchange (i.e. sell) all or part of their SDR holdings for freely usable currencies (such as euros or U.S. dollars) with other IMF members or prescribed holders. As the administrator of the SDR Department, the IMF manages the liquidity of the SDR system and facilitates these voluntary exchanges, which are considered confidential bilateral transactions between IMF members or prescribed holders. Participants in these exchanges may choose whether or not to publicly disclose the details of the transactions.

[19] SDR holdings of IMF members can be easily monitored on a monthly basis on the IMF's external website.

[20] For further details on how these countries used their SDRs, see e.g. US Congress Report (2010).

group) and other countries (non-treated group) *prior* to the 2009 general SDR allocation is small. A *t*-test does not reject the hypothesis of mean equality in this period (see Table 1). This is evidence in favour of the parallel trend assumption, which is crucial to identify the causal effect of the treatment. The gap between the treated and non-treated group widens visibly after the allocation. This provides the first suggestive evidence that the treatment contributed to foster inflation in the treated group, a hypothesis which we test formally below. The marked decline in inflation during the Great Recession of 2008-09 is also clearly apparent. This suggests that it is important to control for the effect of the business cycle, as we do with our measures of economic slack in the estimations.

Readers should be made aware that the case of the euro area is specific insofar as one has to distinguish between reserves held by the ECB itself and those held by the euro area National Central Banks (NCBs). SDRs are allocated to IMF member countries. Since the ECB is an observer and not an IMF member, it is therefore the Member States NCBs that receive the allocated SDRs[21]. However, according to article 31.2 of the Treaty on the Functioning of the European Union, euro area members have the obligation to obtain prior approval by the ECB of any reserve operation, including use of SDRs, involving amounts higher than a certain threshold, in order to ensure consistency with the exchange rate and monetary policies and not to interfere with liquidity operations. Hence they cannot bypass the ECB and affect monetary conditions.

III. Theoretical framework

Why may international reserve accumulation be inflationary? And how can our natural experiment be used to uncover the underlying transmission channels?

The most well-known channel through which reserve accumulation can lead to higher domestic inflation is imperfect sterilisation, as a large array of studies suggests (see e.g. Heller, 1976; Khan, 1979; Stein, 2009; Aizenman and Glick, 2009). An unsterilized increase in foreign reserve holdings (on the asset side of central bank's balance sheet) is matched by an increase in base money (on the liability side of the central bank's balance sheet). Coupled with the money multiplier, this initial increase leads to an expansion in the total quantity of money. According to the quantitative theory of money, this causes prices to rise, in turn. There is empirical support for this

[21] However, the ECB does hold SDRs. As the central bank issuing the euro, the ECB became an "SDR prescribed holder" in November 2000. As such, the ECB may acquire and use SDRs in exchange for an equivalent amount of monetary assets other than gold, in transactions and operations with any other prescribed holder and with any of the IMF's members.

effect. For instance, Khan (1979) finds that reserve growth Granger-causes inflation. Stein (2009) finds that reserve growth significantly raises the inflation rate with a lag of two years. Aizenman and Glick (2009) analyse sterilisation patterns associated with reserves accumulation and find similar evidence of inflationary effects of reserve inflows.

An SDR allocation is very different from other sources of reserve increases, however. It does lead to an expansion in reserve holdings.[22] But it has no direct monetary implications, as noted early on by e.g. Neumann (1973). On the asset side of the central bank's balance sheet, allocated SDRs are booked under the item "SDR holdings" (a sub-category of "international reserves").[23] On the liability side of the central bank's balance sheet, they are matched by an increase in "counterparts to SDRs", a purely bookkeeping entry, which is entirely distinct from base money. It is worth stressing that in the debate on their inflationary impact, SDR allocations have sometimes been compared to Milton Friedman's "helicopter money". That an SDR allocation has no implications for base money should now make clear that such comparisons are ill-founded.[24]

Another channel –which has hitherto received more limited attention– through which reserves accumulation may be inflationary is moral hazard. Like any insurance scheme, an increase in reserve holdings may incentivise countries to pursue more expansionary fiscal policies due to the perceived safety provided by higher reserve holdings. This incentive effect was identified as soon as SDRs were created in the late 1960s. Neumann was the first to propose a formal model showing that SDR allocations may have –what he colourfully called– a "liberalizing influence" on domestic policy.[25] They could induce countries to "use the additional margin to run

[22] SDR allocations are a form of unconditional liquidity. SDR Department participants do not have to meet any specific requirements for the receipt of their share in a general allocation. And following such allocation, they have a right to use their SDRs when they have balance-of-payments gaps to fill and to obtain freely usable currencies from members on the Fund's designation plan (or from other members in transactions by agreement).

[23] This is the standard gross basis presentation of how transactions with the IMF should be presented (also in line with the newly released BPM6 statistics standards). It is assumed, for the sake of simplicity, that the central bank is the sole agent that deals with IMF transactions.

[24] One might go as far as saying that "helicopter money" is possibly the opposite of an SDR allocation insofar as it consists in an increase in money supply (on the liability side) without genuine purchases of assets (on the asset side). An SDR allocation has no effect on the money supply; only use of allocated SDRs has, for instance when SDRs are used to intervene in the foreign exchange market. In most cases, this will lead to a reduction in base money, not to an increase, however. Typically SDRs are used when a country's currency is attacked, and the country in question is short of hard currency reserves. SDRs are converted into freely usable currencies and then used to purchase domestic currency, thereby resulting in a decline in base money –unless interventions are sterilised with security purchases– with obviously no inflationary consequences. In contrast, the reduction in base money may then be even *deflationary*.

[25] Although all the ingredients are present, Neumann does not use explicitly the concept of "moral hazard". But readers should note that his paper is posterior or contemporaneous to several papers that developed moral hazard theory (such as e.g. Arrow, 1963; Pauly, 1968; Grubel, 1971; Mirrlees, 1976; and Holmstrom, 1979).

home-made inflations for a longer period of time than would be possible" under no allocation (see Neumann, 1973, p. 244).[26] In his model of fixed nominal exchange rates, Neumann showed that this occurs if SDRs are used to cover for a country's trade deficit emerging from "home-made inflation" (i.e. an inflation shock resulting from exogenously-determined unsustainable policies). In the absence of SDRs, he shows that the country in question would quickly run out of foreign reserves and be forced to "end its inflationary policy", with adverse knock-on effects on growth. It would be forced into a severe adjustment path, in other words. SDR holdings, in contrast, enable the country to obtain additional hard currency reserves, as Neumann further observes. This enables the country in question to "prolong its home-made inflation for some additional period of time", as he puts it (Neumann, 1973, pp. 240-241).[27]

Neumann remained mute about what he meant by "inflationary policy". But fiscal profligacy was likely on his mind insofar as he was writing in a context when the viability of the Bretton Woods system was severely undermined by large US fiscal deficits and rampant inflation. The perceived safety provided by higher reserve holdings might hence incentivise authorities to postpone adjustment although the path of fiscal policy is unsustainable.[28] And unsustainable fiscal policy may be inflationary because it pushes the economy towards overheating or because it affects the price level directly. As proponents of the fiscal theory of the price level argue indeed, when the government is not able to pay off its future obligations out of tax revenues (i.e. when it runs persistent structural fiscal deficits) market participants may expect it to pay them off by inflating its debt away, hence leading to runaway inflation (see e.g. Sims, 2014).

It should be stressed that the 2009 general allocation was decided in parallel with an agreement by G20 members to expand the stance of fiscal policy at a time when the global economy was in a free fall. The potential inflationary outcome of such allocation was not necessarily perceived negatively, as a result.

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[26] In a related vein, Aizenman and Marion (2004) discuss the case of politically unstable economies with limited monitoring, and in which reserve hoarding encourages opportunistic spending. Mussa et al. (1996) further stress that the rapid accumulation of reserves as a result of a large allocation of SDRs might tempt authorities to monetise some of these gains to increase spending. This further suggests that an SDR allocation may distort incentives, with potential inflationary consequences down the line. In line with this, the IMF also notes that allocated SDRs may help relax budgetary constraints by increasing net central bank credit (directly or indirectly), if the central bank holds the SDRs, or by converting SDRs into usable currencies by the Treasury, if the fiscal authority is the holder of the SDRs (see IMF, 2009).

[27] As Neumann concludes, it is with regard to this incentive effect that the use of SDRs "can be viewed to be inflationary" although, while having a "permitting influence by no means [are they] the driving force of inflation" (ibid.).

[28] More recently it has also been argued that China's "massive cache of reserves should come to be seen as a moral hazard issue" (Holcombe, 2010). China's large reserve holdings would be "a cushion to fall back on" if some of their policies "go awry", e.g. if credit distributed by state-owned banks in the context of China's fiscal stimulus program launched after the collapse of Lehman Brothers lead to a surge in non-performing loans.

Finally, readers should note that a number of studies have endeavoured to estimate whether SDR allocations might have an inflationary impact abroad, instead of domestically. For instance, IMF staff analysed the potential macroeconomic impact of general SDR allocations (see e.g. IMF, 2009a, 2009b, 2011). They noted, for instance, that the actual use of SDRs (i.e. their conversion into freely usable currencies) might trigger inflationary pressures abroad through e.g. higher import demand or issuance of domestic currency by the major central banks.[29] We will control for such a channel in the empirical estimations below.

IV. Empirical methodology

Difference-in-differences estimates

Insofar as the 2009 general SDR allocation is an unanticipated, global exogenous shock to the reserve holdings of the 186 IMF member countries, we can analyse its causal effect on inflation through a difference-in-differences estimation in the spirit of Card and Krueger (1994, 1998) or, more recently, Agarwal and Qian (2014) and Koudijs and Voth (2014).

The methodology can be formalised as follows. Assume that a particular treatment D is given to randomly chosen individuals $i = 1, \ldots n$. Let treatment D_i be a dummy variable that equals one for treated individual i (and zero otherwise). Further assume that there can be two *potential* outcomes to the treatment, noted $Y_{1,i}$ if $D_i = 1$ and $Y_{0,i}$ if $D_i = 0$, respectively. The *observed* outcome Y_i can be written in terms of potential outcomes as follows:

$$Y_i = Y_{0,i} + (Y_{1,i} - Y_{0,i}) \times D_i \qquad (1)$$

where $Y_{1,i} - Y_{0,i}$ is the causal effect of treatment D on individual i. In our context, the treatment is the 2009 general SDR allocation. The sample of individuals $i = 1, \ldots N$, is the IMF membership of 186 countries. We aim to measure the treatment effect of the 2009 general SDR allocation on treated countries relative to non-treated countries.

The treatment period is September 2009 (i.e. the first month after the allocation was disbursed) to December 2011. It hence lasts about two years, which is

[29] Cooper (2009) argues –perhaps optimistically– that the potential global inflationary impact of increased import demand would be neutralised by the monetary policies of the major central banks and that any increase in their monetary base due to the conversion of SDRs would be small.

similar in duration to typical estimates in the literature of the transmission lag of a monetary policy shock. In robustness checks we consider alternative treatment periods such as September 2009-December 2013 (i.e. the full sample after the date of the start of the treatment) and April 2009-December 2011 (i.e. the announcement of the SDR allocation as an alternative date for the start of the treatment).

Crucial for the estimates is evidently the definition of the treated group. In our baseline estimates, treated countries are defined as those receiving a "large" allocation of SDRs, i.e. one in excess of 10% of their existing international reserve holdings, resulting therefore in a sizeable increase of their international reserve holdings.. This threshold corresponds to the top 60-percentile of the distribution of the SDR allocation.[30] A list of the treated countries, so defined, can be found in Table A2.

The comparison of average inflation rates conditional on treatment status is formally linked to the average causal effect as follows:

$$E[Y_i|\, D_i = 1] - E[Y_i|\, D_i = 0] = \tag{2}$$
$$E[Y_{1,i}|\, D_i = 1] - E[Y_{0,i}|\, D_i = 1] + E[Y_{0,i}|\, D_i = 1] - E[Y_{0,i}|\, D_i = 0].$$

As Angrist and Pischke (2008) stress, the term $E[Y_{1,i}|\, D_i = 1] - E[Y_{0,i}|\, D_i = 1] = E[Y_{1,i} - Y_{0,i}|\, D_i = 1]$ is the average causal treatment effect on the individuals which are treated. The term $E[Y_{0,i}|\, D_i = 1] - E[Y_{0,i}|\, D_i = 0]$ captures selection bias. The average treatment effect can hence be identified only in the absence of selection bias. Assigning treatment D randomly solves the selection bias problem insofar as it makes D independent of potential outcome Y.

To operationalize equation (2), we estimate the following model:

$$y_{i,t} = \alpha_i + \lambda_t + \beta_1(1^{treated} \times 1^{after}) + \boldsymbol{\gamma}'\mathbf{X}_{i,t} + \varepsilon_{i,t} \tag{3}$$

where i = 1, … 186 (all IMF country members); t = 1…108 (all months between January 2005 and December 2013) and y is the monthly inflation rate (on a year-on-year basis) in country i at time t; α denotes a vector of country fixed effects and λ a vector of time (monthly) effects.

We define a treatment period dummy, denoted 1^{after}, which equals one from August 2009 to December 2011 (and zero otherwise), and a treated country dummy,

[30] We vary this ad-hoc threshold in robustness checks (more on this below). As aforementioned, it is also important to remember that for the euro area SDRs are allocated to Member States and that the ECB's international reserves represent only a fraction of the total reserve holdings of the Eurosystem.

denoted $1^{treated}$, which equals one for treated countries (and zero otherwise; see below for details as to how we define treated countries). We include in Equation (3) an interaction term, denoted ($1^{after} \times 1^{treated}$), which is the product of the first two dummies.

It is the coefficient on the interaction, β_1, which is of particular interest in our context. It provides us with an estimate of the average causal effect of the 2009 general SDR allocation on inflation in the treated countries after the implementation of the latter (relative to not being treated). As Angrist and Pischke stress, the interaction coefficient (the "DD" estimator) captures the average causal effect of the treatment, i.e. the difference between the treated and control group, before and after treatment (see in particular the corollary to Eq. 5.2.2. p. 170 in Angrist and Pischke, 2008). In our context, this means that inflation was β_1 percentage points higher in treated countries relative to non-treated countries due to the SDR allocation.

Readers should note that the inclusion in the regression of the treatment period dummy (i.e. 1^{after}) and of the treated country dummy (i.e. $1^{treated}$) is not needed to the extent that their effects is already subsumed in the country and time fixed effects α and λ, respectively (see e.g. Popov and Rocholl, 2015, for further details).[31]

In obtaining the estimates, we control for the standard arguments of the Phillips curve, which are included in vector **X**, namely persistence in inflation arising from e.g. nominal rigidities and inflation expectations (as captured by the lagged dependant variable) and the output gap, as a measure of economic slack (as proxied by the deviation of GDP growth from a linear time trend).[32] Conceptually, Equation (3) is a standard Phillips curve augmented with an international reserves shock (i.e. the 2009 general SDR allocation).

Although we already control for a large amount of unobserved heterogeneity through the sets of country and time fixed effects, we seek to control for omitted variables. Vector **X** hence contains additional control variables in several specifications. One of these variables is the *actual* use of the allocated SDRs, which is defined as the percentage change between SDR holdings at the end of our sample (in December 2013) relative to the holdings by the time of the allocation (in August 2009). This is intended to control for the effect of potential inflationary pressures

[31] In robustness checks, we also obtained estimates with the treatment period and treated country dummies. Their inclusion made no economically meaningful difference on the estimate of the treatment effect. These estimates are not reported here to save space but are available from the author upon request.

[32] In robustness checks, we also use IMF measures of the output gap (available for a more restricted set of economies, however) and the unemployment rate, as further measures of economic slack. We also removed observations above +/-1% of the distribution of the output gap. This specification is in the spirit of the traditional Phillips Curve, while recent hybrid models of the Phillips curve (e.g. Galí and Gertler, 1999) relate current inflation to both currently expected future inflation and lagged inflation. The new-keynesian Phillips curve has been used in most recent state-of-the-art new-keynesian DSGE models, such as the model of Clarida, Galí, and Gertler (2000). For a recent survey of the literature on the Phillips curve, see e.g. Gordon (1997, 2011, 2013).

abroad arising from higher import demand if SDRs were converted into freely usable currencies (as discussed in e.g. IMF, 2009, 2011). Another additional control is the extent of sterilisation operations, which we proxy as the non-monetary liabilities of the central bank scaled by the size of the general SDR allocation, as in Cook and Yetman (2012). This is intended to capture the effect on inflation of imperfectly sterilised accumulation of reserves originating from sources other than the SDR allocation.[33] Other controls include dummies for the existence of an IMF program and for euro area membership.

We obtain estimates of the coefficient of Equation (3) by pooled OLS in our baseline specification. We report standard errors corrected for heteroskedasticity and autocorrelation (insofar as the dependant variable is the year-on-year inflation rate at the monthly frequency, which creates overlapping observations). In robustness checks, we consider alternative estimators, including fixed effects, random effects (both with standard errors corrected for clustered heterogeneity), GMM and other estimators tuned to dynamic specifications.

Propensity score matching estimates

One potential estimation challenge is selection bias. If certain countries are part of the treated group because they have specific characteristics, treatment *D* is not given randomly. This would bias our estimate of its effect.[34] It is therefore important to check that the treated and non-treated groups have broadly similar characteristics. A casual look at the countries in the two groups also suggests that assignment might be somewhat random, with the treated group including countries as different as Germany and Moldova (see Table A2), and the non-treated group including countries as diverse as Denmark and Armenia.

More formally, Table 1 reports standard *t*-tests of the differences in the mean and variance of inflation between the two groups during the non-treatment period (as discussed earlier in Section II). The hypothesis of no significant differences between the two groups is not rejected, which suggest that there is no selection bias for this particular variable. However, there are systematic differences along several other dimensions between the two groups prior to the treatment. For instance, it appears that treated countries had more positive output-gaps, lower unemployment, larger current account deficits, higher quota shares and higher public debt levels than non-treated countries. In robustness checks, we explicitly control for these differences, and for the potential non-random selection of countries into the treatment group related to the differences in question.

[33] As explained above, this channel is orthogonal to the SDR allocation, insofar as the latter has no monetary implication. The (imperfect) sterilisation channel could yet be operative if reserve accumulation during the treatment period rose because of e.g. capital inflows.

[34] See e.g. Angrist and Pischke (2008) for a thorough discussion, including of the so-called "Ashenfelter's dip".

To that end, we will also obtain propensity score matching estimates, a methodology that matches countries that undertake the treatment to a subset of countries that do not, based on a subset of observable characteristics (see below). The methodology controls for systematic differences between the treated and non-treated group that affect outcomes, such that selection bias is removed.[35]

Estimates conditional on the stance of fiscal policy

As explained in Section III, a further testable prediction of the moral hazard channel of reserve accumulation on inflation is that the inflationary effect of an increase in reserves should be stronger in countries with more expansionary –notably fiscal– policies. The perceived safety provided by higher reserve holdings would incentivize authorities to pursue more expansionary policy paths, with potential inflationary implications, in line with Neumann's original study or with modern fiscal theories of the price level. To test this prediction, we estimate the interacted effect of the treatment with the stance of fiscal policy:

$$y_{i,t} = \alpha_i + \lambda_t + \beta_1 1^{fiscal} + \beta_2\left(1^{treated} \times 1^{fiscal}\right) + \beta_3\left(1^{after} \times 1^{fiscal}\right) + \beta_4\left(1^{treated} \times 1^{after} \times 1^{fiscal}\right) + \varepsilon_{i,t} \qquad (4)$$

where the dummy 1^{fiscal} equals one in countries with large discretionary fiscal deficits during the treatment period (and zero otherwise). Countries with large discretionary fiscal deficits are defined as those countries within the bottom quartile of the distribution of cyclically-adjusted fiscal balances between 2010 and 2011.[36] A list of these countries can be found in Table A3. It is the coefficient on the triple interaction, β_4, which is of particular interest in our context.[37] It provides us with an estimate of

[35] In practice, it is sufficient to match treated and control observations based on a propensity score, which is a scalar variable that is the probability that country *i* receives the treatment. The propensity scores are typically estimated using a logit or probit regression. There are then several algorithms that can be used to match treated and non-treated observations, such as nearest neighbour(s) (i.e. the country which has the closes propensity score) with or without replacement, radius, kernel, local linear, etc. (see e.g. Forbes, Fratzscher and Straub, 2013, for further details).

[36] Data on cyclically-adjusted fiscal balances are available from the IMF for a restricted subset of countries. To obtain estimates of cyclically-adjusted balances over the treatment period, we regressed the fiscal balance as a share of GDP on the output gap measure discussed above, time effects and country fixed effects and used the residual as our measure of structural (or cyclically-adjusted) fiscal balance (i.e. the balance orthogonal to the business cycle, country-specific factors and global factors). In robustness check we also use as alternative threshold the bottom 10% of the distribution of cyclically-adjusted fiscal balances between 2010 and 2011 as well as the level of public debt as a metric of fiscal vulnerability (see below).

[37] This triple interaction is akin to a triple difference estimator ("DDD", or Difference-in-Difference-in-Differences), which helps to difference out trends that may differentially affect the treated and control group countries in DD estimates.

the average causal effect of the 2009 general SDR allocation on inflation in treated countries which have large fiscal deficits. Finding $\widehat{\beta_4} > 0$ is supportive evidence of the hypothesis that reserve accumulation is inflationary because of moral hazard and incentive effects, insofar as these effects are commensurate to the propensity of these countries to pursue more expansionary fiscal policies.

V. Empirical estimates

Difference-in-differences estimates

Table 2 reports the simple arithmetic difference in the differences between the average inflation rate in the treated group and the non-treated group, before and after the treatment, namely 0.44. This suggests that inflation was about half a percentage point higher in the treated group relative to the non-treated group after the 2009 general SDR allocation, in line with the hypothesis that it was an inflationary shock. Of course, this still says nothing on the statistical significance of this effect.

This is taken up in Table 3, which reports pooled OLS estimates of Equation (3). Estimates without controls are reported in column (1). They suggest that the about half a percentage point increase in inflation in the treated group relative to the non-treated group due to the treatment is statistically significant. Adding dummies to control for countries under an IMF program over the treatment period and for euro area membership (as in column 2) as well as for use of SDRs (as in column 3) does not alter the results. Adding the arguments of the Phillips curve as control variables, as in columns (5) to (8) further supports this finding. The estimates suggest that the 2009 general SDR allocation led to an increase in inflation of about 0.2-0.3 percentage point in the short run in the treated group relative to the non-treated group, against an increase of about 5 percentage points in the long run (consistently with inflation's estimated strong persistence).[38] We also obtained estimates controlling for deviation from the central banks' respective inflation target, when this information was available (i.e. for 55 of the 186 countries in our sample), with broadly similar results (which are not reported to save space but available upon request).[39]

[38] This is calculated as the ratio of 0.20 and (1 – 0.96).

[39] The treatment effect is also more imprecisely estimated, which reflects the smaller sample of available observations. Note that the treatment effect loses its statistical significance when sterilisation is added as a control in the static specification of column (4) of Table 3, but not in the dynamic specification of column (8).

Robustness

Next we consider alternative estimation techniques in Table 4. Panel fixed effect estimates are reported in columns (1) to (3). Random effect estimates are reported in columns (4) to (6). In addition, the standard errors are robust to both heteroskedasticity and autocorrelation (as in the baseline specification) and are now also clustered by country. The estimates remain very similar to those obtained in the baseline specification in terms of sign, economic magnitude and statistical significance. In the dynamic estimates, in particular, the coefficient on the interacted dummy continues to point to a short-run effect of the treatment on inflation on the order of 0.2-0.3 percentage point.

Although we resort to a natural experiment, which should enable us to cleanly trace the causal effect of an increase in international reserves on inflation, sceptical readers could still argue that the introduction of a lagged dependant variable, which control for one of the arguments of the Phillips curve, could give rise to endogeneity. We address the issue in Table 5, which reports estimates obtained with alternative dynamic estimation techniques, including the Griliches-Liviatan estimator (in column 1), the Hatanaka estimator (in column 2) and difference-GMM (in columns 3 and 4). The results remain again broadly unchanged, further buttressing the validity of our experiment.[40]

We also varied the definition of the treatment period and took the full sample of observations after the treatment, namely the above-four-year period from August 2009 to December 2013.[41] The results remained broadly in terms of sign, economic magnitude and statistical significance relative to those obtained under the baseline definition. The coefficient of the interaction term was slightly smaller, at 0.3 percentage point in the static specification and 0.1-0.15 percentage point in the dynamic specification. Readers should yet note that such a longer treatment period may be subject to contamination bias and to the possible occurrence of treatments other than the 2009 general SDR allocation. We hence prefer to use a shorter treatment period in the baseline estimation to mitigate such potential bias.

In a related vein, given that the announcement of the 2009 general SDR allocation by the G20 (in April 2009) differed from its implementation by the IMF (in August 2009), we could also investigate the announcement effect instead of the implementation effect. We again obtained broadly similar results, once we controlled for the standard arguments of the Phillips curve, although the estimates were smaller, at about 0.1-0.2 percentage point for the coefficient on the interacted dummy in the

[40] The effect of the treatment loses statistical significance in column (4), however. But the GMM estimates should be taken with a pinch of salt insofar as the long time dimension *T* of our panel relative to its cross-sectional dimension *N* leads to an instrument proliferation problem. In other words, the number of instruments in the estimation (i.e. over 2,000) far exceeds the number of cross-sections (i.e. 170), which makes estimation biased (and borderline feasible), as observed by Roodman (2009).

[41] The following robustness checks are not shown to save space but are available from the author upon request.

dynamic estimations. One interpretation of this result is that the implementation of G20 Leaders' decision to allocate new SDRs remained somewhat uncertain until the summer of 2009, because it had to be endorsed by the full IMF membership.[42] That the availability of insurance was uncertain contributed to dampen moral hazard, and hence the inflationary effect of an increase in reserves.

As a further robustness check, we changed the definition of our metric of economic slack and took the output gap and unemployment rate estimates from the IMF (available for a restricted subset of our sample). We obtained similar results.[43]

As yet another robustness check, we varied the definition of the threshold used to define "large" SDR allocations. Instead of 10%, we used 8%, 12%, 14% and 16% of prior reserve holdings as alternative thresholds. The results are shown in Table 6. In the case of the thresholds above 12%, we obtained stronger results in the dynamic specifications relative to the baseline estimates, with coefficient estimates of 0.3 to 0.5 on the interacted dummy. In the case of the lower 8%-threshold, the coefficient was statistically insignificant (and wrongly-signed) in the static specification of column (1). This suggests that an increase in reserves must be sufficiently larger to have discernible inflationary effects.

Next we considered the effect of lagged money creation and alternative treatment scaling. Table 7 reports the estimates when lagged inflation is replaced with lagged money creation (in column 3). The baseline results remained unchanged in terms of sign and statistical significance. In terms of economic magnitude, the estimates suggested that the effect of the moral hazard channel is even stronger (and trebles from 0.2 to 0.7). Table 7 reports additional estimates using an alternative definition of treatment where the SDR allocation is scaled by the long-term average of international reserves defined from January 2005 to July 2009 (see columns 4 and 5).[44] The results remained broadly unaltered in the dynamic specification, in terms of sign, significance and economic magnitude. As still another alternative, we scaled the SDR allocation by money base (in lieu of international reserves) as of July 2009, and as of 2008 (annual average); see columns 6-9.[45] The interacted coefficient remained

[42] That the decisions by G20 Leaders are not always implemented by the IMF membership, and hence subject to uncertainty are epitomized by the 2010 Quota and Governance reform decided by G20 Leaders at the Seoul Summit which is still not ratified by all IMF members, including the US.

[43] As aforementioned given that output gap data are readily available for only a restricted number of countries, we used as proxy the deviation of GDP growth from a linear time trend to obtain our baseline estimates. To ensure that our estimated output gaps did not deviate too much from e.g. IMF estimates, we calculated the correlation between our own estimates and those readily available from the Fund for the restricted sample of countries for which both measures were available. We found that our measure was a satisfactory proxy insofar as both measures were positively correlated (with a correlation coefficient on the order of 0.4 significant at the 1% level).

[44] In this case, the threshold for large allocation is set to 15%, given that the median is also higher than in the baseline case (at almost 10% vs. from around 7.3% in the baseline).

[45] The threshold of the treatment is set at 5% (in line with the respective treatment medians).

positive and significant (although slightly different in magnitude than in the baseline), hence providing further support for the moral hazard channel.[46]

Sceptical readers could remain concerned by the existence of a potential inflation expectation channel, alongside the imperfect sterilisation and moral hazard channels which we have considered so far. To the extent that markets expect allocated SDRs to be used in the future with potential inflationary consequences, they would argue, expectations of future inflation could feed into current inflation. To address this point, we used proxies of changes to inflation expectations one-year and two-year ahead by the time of the treatment.[47] We included these proxies as additional controls in our baseline specification and tested whether the effect of the treatment (hence the moral hazard channel) survives. The estimates are reported in Table 8 (where column 1 shows our baseline estimates pro memoria). Not only does the effect of the treatment survive, but it turns out to be even slightly stronger (see columns 2 and 3). This suggests that expectations of future inflation, which could feed into current inflation, do not annihilate the effect of the moral hazard channel.

Propensity score matching estimates

One concern with our baseline estimates could be that the results are not driven by the effect of the treatment itself but by systematic differences between countries in the control and treated groups. This non-random assignment would make it difficult to assess whether differences in inflation developments after the 2009 general SDR allocation arose because of the existence of a moral hazard channel or simply because of underlying differences between the two sets of countries. As we discussed in section IV, there are systematic differences indeed prior to the treatment insofar as treated countries had more positive output-gaps, lower unemployment, larger current account deficits, higher quota shares and higher public debt levels than non-treated countries.

We address this challenge by obtaining propensity score matching estimates. We select a subsample of control (non-treated) countries that are as close as possible a match for the sample of treated countries based on a set of observable characteristics

[46] This said, the mean equality test does not reject that the pre-treatment average inflation rates are significantly different for the two groups. In other words, there may be selection bias if one uses this alternative treatment metric (unlike the baseline metric). These results should hence be interpreted with caution.

[47] The change in inflation expectations one-year ahead is defined as the difference between the IMF forecast for CPI inflation for 2010 published in the WEO of October 2009 and that published in the WEO of April 2009 for each of the 186 countries of our sample. The change in inflation expectations two years ahead is defined as the difference between the IMF forecast for CPI inflation for 2011 published in the WEO of October 2009 and that published in the WEO of April 2009 for each of the 186 countries of our sample. The two sets of forecasts nicely straddle around the treatment (which was disbursed in September 2009).

(see e.g. Forbes, Fratzscher and Straub, 2013, for a discussion of the methodology in relation to the international macro-finance literature).

The estimates are obtained in two steps. In a first step, we obtain propensity scores i.e. the conditional probability of a country being treated (in other words, of having a large SDR allocation) conditional on a set of observables prior to the treatment. We use logit regressions to estimate the conditional probabilities as well as the variables listed in Table 1 that pointed to systematic differences between our baseline treated and non-treated groups as observables.[48] In a second step, we use five distinct matching algorithms to match treated observations with control observations. In so doing, we subject the estimates to two specification tests. One is the Common Support Condition which requires that for each set of observables there is a positive probability that a country-observation is treated and untreated (countries are known to be "on-support" if this condition is met). The second specification test is the Balancing Test (or Independence Assumption) which consists in checking that the matching procedure does help remove significant systematic differences between the treated and matched non-treated group.[49]

Table 9 reports the first-stage logit regression estimates used to calculate the propensity scores. The table also reports the means for treated and control groups using different matching algorithms for the set of observables, in line with the Balancing Test (or Independence Assumption). All the observables contribute to the propensity scores and enter significantly in the regression. Importantly, there are significant differences in the means of several of these observables between the treated and unmatched control groups, but they narrow considerably (and, in some cases, almost vanish) once the matching methodology is applied, which hence suggests that it is valid.

Table 10 next reports the estimated average treatment effect obtained from the propensity score matching estimates calculated as the difference in inflation between the treated and matched control groups according to the five different matching algorithms. The table further reports the number of "off-support" and "on-support" observations for both groups. The algorithms generate only 60 treated observations that are "off-support"; this remains very small insofar as the total number of treated observations is almost 3,500. Moreover, depending on the algorithm, the estimates suggest that inflation is about half a percentage point higher in the treated group relative to the matched control groups.[50] Therefore the estimations confirm that the

[48] One rationale for using these variables is that they capture both countries' relative economic positions (through their quota shares) as well as their macroeconomic situations (through the other variables) prior to the treatment.

[49] This is achieved by comparing means between the matched and unmatched samples across the range of observables.

[50] The estimated effect is not statistically significant in the case of the local-linear matching algorithm, however. The estimates were obtained with STATA's psmatch2 code. The standard errors do not take into account the fact that the propensity scores are estimated.

2009 SDR general allocation had a causal effect on inflation when it was relatively large. And they point to a similarly strong moral hazard channel as suggested by our baseline difference-in-differences estimates, insofar as the propensity score matching estimate is of similar economic magnitude.

Estimates conditional on the stance of fiscal policy

What is the evidence that the prudence of the fiscal policy stance is affected by the perceived safety provided by higher reserve holdings?

This is addressed in Table 11, which reports pooled OLS estimates of Equation (4). The main coefficient of interest is the one on the triple interaction, which provides us with an estimate of the average causal effect of the 2009 general SDR allocation on inflation in treated countries with large discretionary fiscal deficits. As already explained in Section IV, large" refers to the bottom quartile of the distribution of cyclically-adjusted fiscal balances between 2010 and 2011.[51] The coefficient on the triple interaction is positive, statistically significant and larger than the coefficient obtained in the baseline estimates, at about 1.2. In other words, inflation was about 1.2 percentage points higher in treated countries with large fiscal deficits, compared with other countries. This result suggests that treated countries may have pursued more expansionary fiscal policies due to the perceived safety of higher reserve holdings. This, in turn, it is consistent with the hypothesis that reserve accumulation may be inflationary because of moral hazard and incentive effects. In robustness checks, we used other metrics of fiscal vulnerability, including one in which we used real GDP growth (rather than the output-gap) as a proxy of the business cycle to estimate cyclically-adjusted fiscal balances; the bottom decile of the distribution as an alternative definition of large structural fiscal deficits;[52] as well the level of public debt. The results, reported in columns 2 to 4 of Table 11, point to similar conclusions, with coefficient estimates on the triple interaction between 1.5 and 3.4 for fiscal deficit measures and 1.2 for the public debt level (in other words, inflation was about 1.2 percentage points higher due to the treatment in countries with large public debts). This further buttresses evidence for the moral hazard channel and for the hypothesis that reserve accumulation may be inflationary because of incentive effects.

[51] Specifically, -0.8% of GDP or lower.

[52] Specifically, -2% of GDP or lower.

VI. Conclusion and policy implications

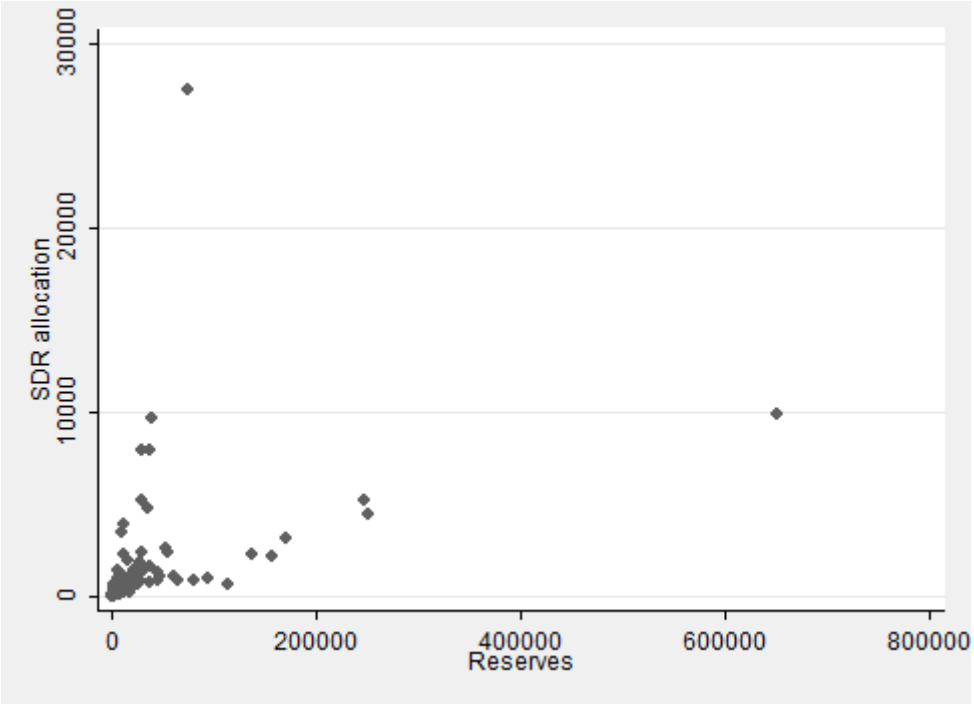
This paper has aimed to assess whether reserve accumulation can be inflationary because of moral hazard and incentive effects. It has used the 2009 general SDR allocation as a unique natural experiment that enables to trace the effect of an unanticipated, global exogenous shock to the foreign reserve holdings of the 186 IMF member countries.

Difference-in-differences and propensity score matching estimates suggest that inflation in countries receiving large SDR allocations was about half a percentage point higher in annual terms within the next two years following the allocation, controlling for the standard arguments of the Phillips curve. These estimates are robust to an array of sensitivity checks, including different estimation techniques; changes to the definition of the treatment period; changes to the threshold used to define large allocations; to using the announcement (in lieu of the implementation) date of the SDR allocation to define the treatment; to the inclusion of a host of controls to mitigate omitted variable bias; to controlling for the inflation expectation channel; and to various scaling of the treatment.

Moreover, this effect is commensurate to the size of these countries' discretionary fiscal deficits. According to our conditional estimates, inflation in countries receiving large SDR allocations and with large discretionary fiscal deficits was more than one percentage point higher within the next two years following the allocation. This is consistent with the moral hazard channel and the hypothesis that reserve accumulation may be inflationary because of incentive effects which encourage countries to pursue more expansionary fiscal policy paths due to the perceived safety provided by higher reserve holdings.

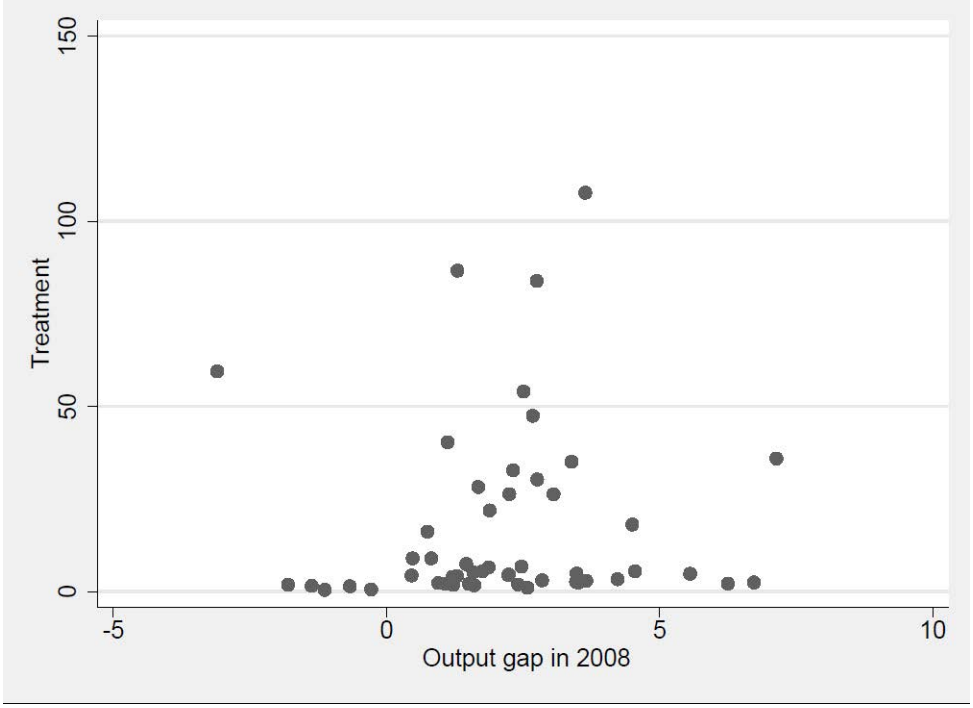
Optimal insurance in the form of precautionary reserves or firewalls –be they at the country, regional or global level– has been widely discussed since the onset of the global financial crisis. One question is what the adequate mix of safety nets should be, i.e. self-insurance, through reserve accumulation, or collective insurance, through IMF lending instruments, regional financing arrangements or currency swap agreements. The findings presented in the paper gives empirical support to the view that reserve accumulation can have inflationary costs through the moral hazard channel. From a policy perspective, they hence tilt the balance of benefits and costs against self-insurance. In discussions about the adequate mix of instruments for the global safety net, they therefore also tend to be more supportive of collective insurance.

Figure 1: Evidence of orthogonality between SDR allocation and foreign reserves



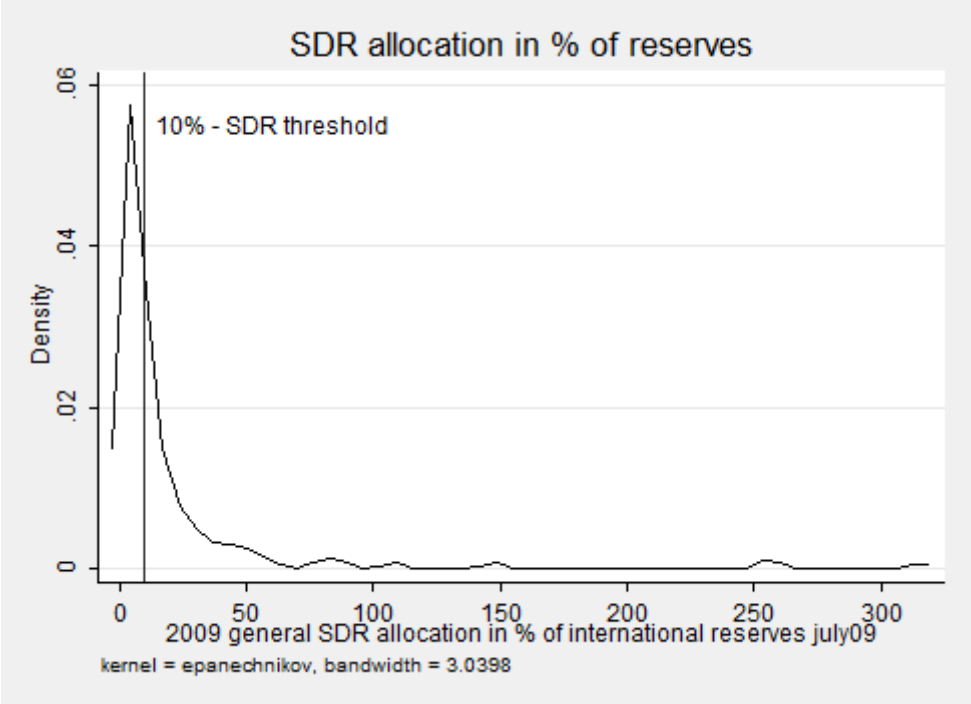
Note: The figure plots the 2009 general SDR allocation (in SDR million) across countries against their international reserve holdings (in SDR million) as of July 2009. China is not shown (due to its exceptionally large reserve holdings) to make the figure more readable.

Figure 2: Evidence of treatment exogeneity



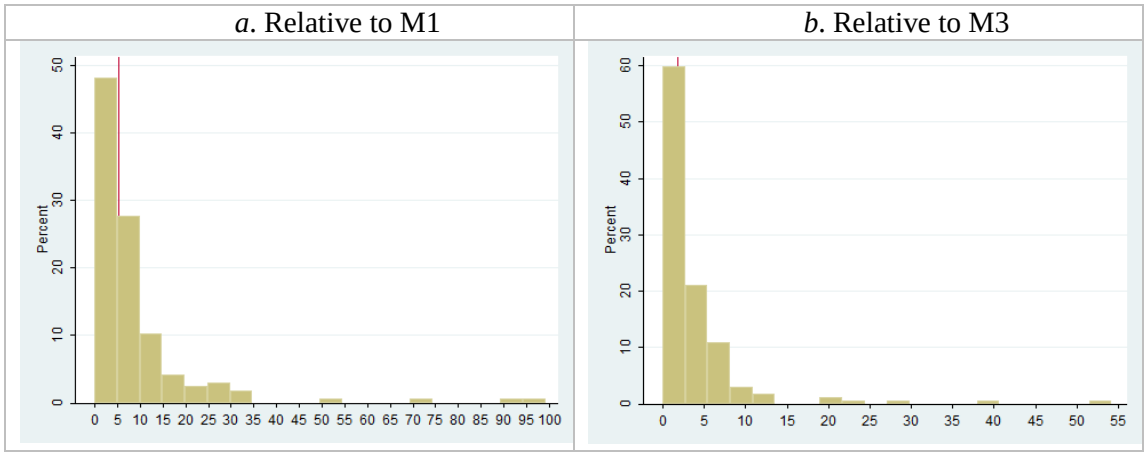
Note: The figure plots the output gap in 2008 (defined as the deviation of real GDP growth from a linear time trend) against the treatment (i.e. SDR allocated in 2009 scaled by international reserve holdings prior to the allocation).

Figure 3: Kernel distribution of the 2009 general SDR allocation



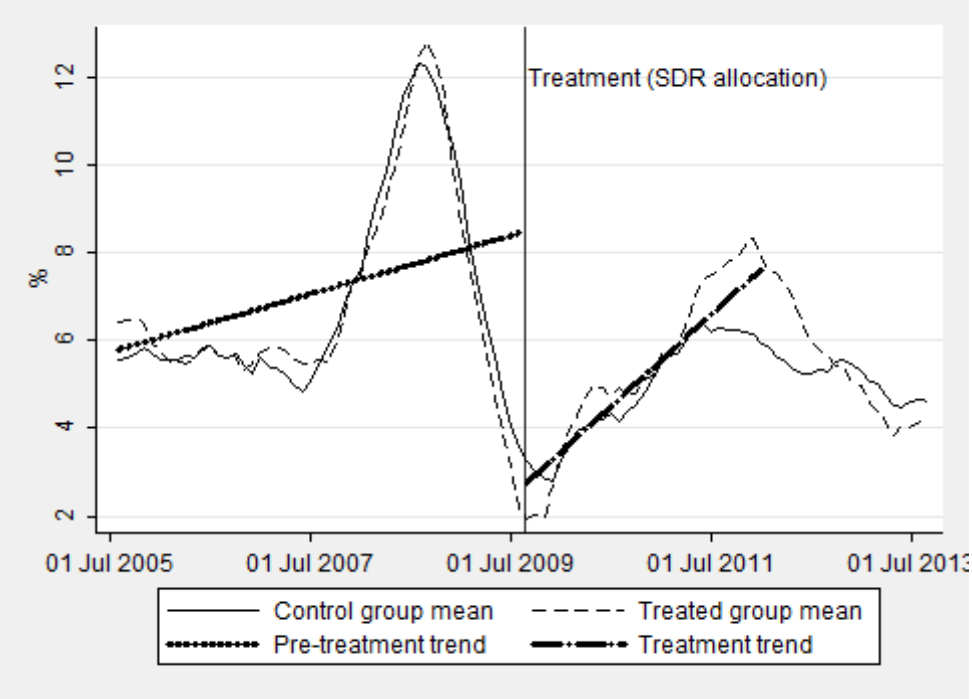
Note: The figure shows the kernel distribution of the 2009 general SDR allocation (expressed as a percentage of countries' international reserves in the month prior to the allocation) obtained with an Epanechnikov kernel. The vertical line indicates the 10%-threshold for "large" allocations (the 60-percentile of the distribution). The median allocation stands at 7.3%.

Figure 4: Size of the 2009 general SDR allocation relative to money



Note: The figure shows the histogram of the 2009 general SDR allocation scaled by narrow money (panel *a*) and broad money (panel *b*). The red lines indicate the median allocations. The *x*-axis is in percentage.

Figure 5: Inflation – Treated vs. control groups pre- and post-treatment



Note: The figure shows the evolution of inflation year-on-year (in percentage points) in the treated and control groups before and after treatment. The treatment refers to the IMF general SDR allocation of 28 August 2009. Treated countries are defined as those for which SDR received exceeded 10% of their international reserve holdings prior to the allocation.

Table 1: Pre-treatment systematic differences across groups

Variable	Non-treated	Treated	Difference
Inflation	6.798	6.872	-0.074
Output-gap	0.967	1.502	-0.535 ***
Real GDP growth	5.306	3.787	1.519 ***
Unemployment rate	7.814	6.963	0.851 ***
Current account balance	-0.393	-6.723	6.33 ***
IMF quota share	0.436	0.756	-0.32 ***
Public debt/GDP	42.479	61.639	-19.16 ***

Notes: The table shows a *t*-test of the difference in mean between the treated and non-treated group prior to the treatment for selected variables. *** $p< 0.01$.

Table 2: Arithmetic difference-in-differences

	Non-treatment period	Treatment period	Difference
Treated group	6.396	5.446	-0.950
Non-treated group	6.268	4.874	-1.394
Difference	0.128	0.572	*0.445*

Notes: The table shows the simple arithmetic difference-in-differences in average inflation rates between the treated and non-treated groups, both for the treatment and placebo periods. The treatment period is August 2009-December 2011.

Table 3*:* Baseline difference-in-differences estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$1^{treated} \times 1^{after}$	0.473***	0.487***	0.473***	0.262	0.203***	0.207***	0.203***	0.267***
	(0.159)	(0.160)	(0.159)	(0.287)	(0.046)	(0.047)	(0.046)	(0.081)
IMF program dummy		-0.481***				-0.092**		
		(0.151)				(0.046)		
Euro area member dummy		-0.356***				-0.094*		
		(0.134)				(0.053)		
Use of SDRs			0.002				0.000	
			(0.002)				(0.001)	
Sterilisation				-0.005**				0.000
				(0.002)				(0.000)
Lagged inflation					0.954***	0.954***	0.954***	0.955***
					(0.009)	(0.009)	(0.009)	(0.012)
Output gap					0.003	0.002	0.003	0.007
					(0.006)	(0.006)	(0.006)	(0.008)
Constant	-0.656	-0.851	2.587***	23.410***	0.213	0.216	0.000	1.648***
	(0.807)	(0.805)	(0.385)	(1.830)	(0.354)	(0.354)	(0.157)	(0.527)
Observations	17,647	17,647	17,647	10,036	17,152	17,152	17,152	9,755
Adjusted R^2	0.520	0.520	0.520	0.473	0.958	0.958	0.958	0.953
F-stat	95.97	97.39	95.97	70.97	1271	1265	1271	873.9
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Time effects	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1, + p<0.15

Note: The table reports pooled OLS estimates of Equation (3). Estimates without Phillips curve controls are reported in columns (1) to (4) and estimates with such controls are reported in columns (5) to (8). IMF program and euro area member dummies are included as additional controls in columns (2) and (6), and sterilisation and use of SDRs in columns (3), (4), (7) and (8). The treatment period is August 2009-December 2011.

Table 4: Estimates with alternative panel estimation techniques

	(1)	(2)	(3)	(4)	(5)	(6)
$1^{treated} \times 1^{after}$	0.282***	0.203**	0.267+	0.239***	0.131*	0.274**
	(0.085)	(0.085)	(0.175)	(0.070)	(0.069)	(0.129)
Lagged inflation	0.961***	0.954***	0.955***	0.979***	0.977***	0.975***
	(0.006)	(0.008)	(0.011)	(0.004)	(0.005)	(0.006)
Output gap	0.035***	0.003	0.007	0.035***	0.004	0.007
	(0.005)	(0.007)	(0.010)	(0.005)	(0.006)	(0.008)
Sterilisation			0.000			0.000**
			(0.001)			(0.000)
Use of SDRs						0.000
						(0.000)
Constant	0.192***	0.174***	0.203*	0.090***	0.055	0.088
	(0.043)	(0.065)	(0.116)	(0.024)	(0.055)	(0.091)
Observations	17,152	17,152	9,755	17,152	17,152	9,755
No. countries	170	170	99	170	170	99
Overall R^2	0.954	0.958	0.953	0.954	0.958	0.953
ρ	0.0189	0.0259	0.0274	0	0	0
Country fixed effects	YES	YES	YES	YES	YES	YES
Time effects	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1, + p<0.15

Note: The table reports panel fixed effect estimates of Equation (3) in columns (1) to (3) as well as random effects estimates in columns (4) to (6), controlling for the arguments of the Phillips curve. The standard errors reported are robust to heteroskedasticity, autocorrelation and clustered heterogeneity. The treatment period is August 2009-December 2011.

Table 5*:* Alternative dynamic estimates

	(1) Griliches-Liviatan	(2) Hatanaka	(3) Difference GMM	(4) Difference GMM robust
$1^{treated} \times 1^{after}$	0.192***	0.206***	0.180*	0.180
	(0.074)	(0.045)	(0.100)	(0.207)
Lagged inflation			0.944***	0.944***
			(0.004)	(0.011)
Output gap	-0.004	0.001	0.002	0.002
	(0.008)	(0.006)	(0.008)	(0.015)
Fitted inflation	0.933***	0.933***		
	(0.016)	(0.008)		
Residual		1.158***		
		(0.022)		
Constant	0.288	0.281	0.306***	0.306***
	(0.437)	(0.378)	(0.075)	(0.105)
Observations	16,961	16,961	9,829	9,829
Adjusted R^2	0.903	0.960		
F-stat	488.3	1436		
No. countries	Pooled	Pooled	170	170
Country fixed effects	YES	YES	YES	YES
Time effects	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports alternative dynamic estimates, including estimates obtained with the Griliches-Liviatan estimator (in column 1), the Hatanaka estimator (in column 2) and difference-GMM estimates (in columns 3 and 4). The treatment period is August 2009-December 2011.

Table 6*:* Estimates with alternative thresholds to define treatment

	(1) >=8	(2) >=12	(3) >=14	(4) >=16	(5) >=8	(6) >=12	(7) >=14	(8) >=16
$1^{treated} \times 1^{after}$	-0.047	0.336**	0.408**	0.507***	0.123*	0.244**	0.275**	0.366***
	(0.149)	(0.172)	(0.177)	(0.193)	(0.074)	(0.098)	(0.108)	(0.122)
Lagged inflation					0.955***	0.955***	0.955***	0.955***
					(0.012)	(0.012)	(0.012)	(0.012)
Output gap					0.007	0.008	0.008	0.007
					(0.008)	(0.008)	(0.008)	(0.008)
Sterilisation					0.000	0.000	0.000	0.000
					(0.000)	(0.000)	(0.000)	(0.000)
Use of SDRs					-0.002	-0.002	-0.002	-0.002
					(0.002)	(0.002)	(0.002)	(0.002)
Constant	-0.544	-0.511	-0.505	-0.501	0.273	0.287	0.285	0.287
	(0.790)	(0.786)	(0.786)	(0.785)	(0.256)	(0.254)	(0.255)	(0.255)
Observations	17,647	17,647	17,647	17,647	9,755	9,755	9,755	9,755
Adjusted R^2	0.519	0.520	0.520	0.520	0.953	0.953	0.953	0.953
F-stat	95.74	95.98	96.05	96.20	878.6	872.9	872.1	871.3
Country fixed effects	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*
Time effects	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports estimates with alternative treatment thresholds including 8% of prior holdings of international reserve holdings (in columns 1 and 5); 12% (in columns 2 and 6); 14% (in columns 3 and 7) and 16% (in columns 4 and 8). The treatment period is August 2009-December 2011.

Table 7: Estimates with money base growth and alternative treatment scaling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$1^{treated} \times 1^{after}$	0.473***	0.203***	0.698***	0.183***	0.368***	0.171***	0.276***	0.089*	0.135**
	(0.159)	(0.046)	(0.176)	(0.054)	(0.103)	(0.049)	(0.067)	(0.046)	(0.061)
Lagged inflation		0.954***		0.954***	0.955***	0.954***	0.955***	0.954***	0.955***
		(0.009)		(0.009)	(0.012)	(0.009)	(0.012)	(0.009)	(0.012)
Output gap		0.003	-0.051***	0.002	0.006	0.003	0.008	0.002	0.006
		(0.006)	(0.018)	(0.006)	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)
Lagged money creation			0.012***						
			(0.003)						
Sterilisation					0.000		0.000		0.000
					(0.000)		(0.000)		(0.000)
Use of SDRs					0.000		0.000		0.000
					(0.001)		(0.001)		(0.001)
IMF program dummy					-0.031		-0.053		-0.048
					(0.063)		(0.063)		(0.062)
Constant	-0.656	0.213	8.323***	0.212	0.095	0.222	0.140	0.242	0.106
	(0.807)	(0.354)	(0.721)	(0.353)	(0.178)	(0.355)	(0.179)	(0.356)	(0.177)
Observations	17,647	17,152	11,145	17,152	9,755	17,152	9,755	17,152	9,755
Adjusted R^2	0.520	0.958	0.655	0.958	0.953	0.958	0.953	0.958	0.953
F-stat	95.97	1271	96.51	1271	868.1	1275	879.2	1275	878.9
Country fixed effects	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*
Time effects	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*	*YES*

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports pooled OLS estimates of Equation (3). Baseline estimates are reported pro memoria in columns (1) and (2). Lagged money creation is used instead of lagged inflation in column (3). Alternative scaling for the treatment are used in columns (4) to (9). The SDR allocation is scaled by long-term international reserve holdings in columns (4) and (5); by the money base in July 2009 in columns (6) and (7); and by the money base in 2008 in columns (8) and (9). The estimates control for both fixed effects and time effects. The treatment period is August 2009-December 2011.

Table 8: Controlling for the inflation expectations channel

	(1)	(2)	(3)
$1^{treated} \times 1^{after}$	0.473***	0.516***	0.516***
	(0.159)	(0.160)	(0.160)
1-year ahead expected inflation change		0.037	
		(0.052)	
2-year ahead expected inflation change			-0.450***
			(0.039)
Constant	-0.656	13.263***	16.932***
	(0.807)	(0.602)	(0.532)
Observations	17,647	17,437	17,437
Adjusted R^2	0.520	0.519	0.519
Country fixed effects	*YES*	*YES*	*YES*
Time effects	*YES*	*YES*	*YES*

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports pooled OLS estimates of Equation (3) controlling for changes to inflation expectations one-year ahead and two-year ahead by the time of the treatment. The change in inflation expectations one-year ahead is defined as the difference between the IMF forecast for CPI inflation for 2010 published in the WEO of October 2009 and that published in the WEO of April 2009 for each of the 186 countries of our sample. The change in inflation expectations two years ahead is defined as the difference between the IMF forecast for CPI inflation for 2011 published in the WEO of October 2009 and that published in the WEO of April 2009 for each of the 186 countries of our sample. The treatment period is August 2009-December 2011.

Table 9: First-stage logit regression for propensity scores and means of treated and matched control groups

	1st-stage logit regression results			Means of treated and control group using different matching algorithms											
							% reduc.		% reduc.		% reduc.		% reduc.		% reduc.
	Coef.	*Std.*			Treated	Control	|bias|	Control	|bias|	Control	|bias|	Control	|bias|	Control	|bias|
						Nearest neighbour		*5-nearest*		*Kernel*		*Radius*		*Local-linear*	
Quota share	0.177	*0.020*	***	Unmatched	0.891	0.473		0.473		0.473		0.473		0.473	
				Matched	0.595	0.464	68.5	0.301	29.5	0.347	40.6	0.354	42.4	0.301	29.5
Real GDP growth	-0.016	*0.004*	***	Unmatched	2.233	3.138		3.138		3.138		3.138		3.138	
				Matched	2.252	2.518	70.6	2.263	98.8	2.345	89.7	2.331	91.3	2.264	98.6
Current account/GDP	-0.061	*0.028*	***	Unmatched	-7.214	-1.574		-1.574		-1.574		-1.574		-1.574	
				Matched	-7.292	-6.149	79.7	-8.761	73.9	-8.408	80.2	-8.287	82.3	-8.767	73.8
Public debt/GDP	0.010	*0.000*	***	Unmatched	60.460	43.298		43.298		43.298		43.298		43.298	
				Matched	59.810	51.662	52.5	56.251	79.3	56.885	83.0	56.963	83.4	56.220	79.1
Constant	-1.351	*0.050*	***												

Note: The table reports the first-stage logit regression results predicting the probability of receiving a large SDR allocation and used to calculate the propensity scores as well as well as the means for treated and control groups using different matching algorithms for the set of observables.

Table 10: PSME estimates - Average treatment effect on the treated

Sample	Treated group	Control group	Difference	S.E.	*t*-stat
Unmatched obs.	5.406	4.782	0.624	0.134	4.657
Matched obs.					
Nearest neighbour	5.473	5.062	0.411	0.162	2.537
Off support obs.	*60*	*0*			
On support obs.	*3404*	*5443*			
5-nearest neighbours	5.473	4.930	0.543	0.219	2.479
Off support obs.	*60*	*0*			
On support obs.	*3404*	*5443*			
Kernel	5.473	5.019	0.454	0.164	2.768
Off support obs.	*60*	*0*			
On support obs.	*3404*	*5443*			
Radius with caliper	5.473	5.036	0.437	0.164	2.665
Off support obs.	*60*	*0*			
On support obs.	*3404*	*5443*			
Local linear	5.473	5.012	0.461	0.425	1.085
Off support obs.	*60*	*0*			
On support obs.	*3404*	*5443*			

Note: The table reports the average treatment effect on the treated obtained from the propensity score matching estimates calculated as the difference in inflation between the treated and the matched control groups according to five different matching algorithms. The table also reports the number of "off-support" and "on-support" observations for the treated and matched control groups.

Table 11: Estimates conditional on the stance of fiscal policy

	(1) Bottom quartile & output -gap	(2) Bottom quartile & growth	(3) Bottom decile & output -gap	(4) Upper decile public debt
1^{fiscal}	0.432***	0.375***	0.420	7.115***
	(0.141)	(0.141)	(0.276)	(2.533)
$1^{treated} \times 1^{fiscal}$	-1.320***	-1.398***	-2.884***	-5.562**
	(0.320)	(0.331)	(0.489)	(2.563)
$1^{after} \times 1^{fiscal}$	-0.501***	-0.651***	-1.250***	-0.876
	(0.179)	(0.174)	(0.318)	(0.588)
$1^{treated} \times 1^{after \times} 1^{fiscal}$	1.212***	1.509***	3.400***	1.206**
	(0.353)	(0.364)	(0.567)	(0.614)
Constant	0.371	0.462*	0.487*	-5.617**
	(0.256)	(0.264)	(0.261)	(2.510)
Observations	5,067	5,079	5,067	8,907
Adjusted R^2	0.749	0.749	0.753	0.556
F-stat	127.8	129.5	124.7	93.11
Country fixed effects	*YES*	*YES*	*YES*	*YES*
Time effects	*YES*	*YES*	*YES*	*YES*

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports pooled OLS estimates of Equation (4). Countries with large fiscal deficits are defined as countries in the bottom quartile of the distribution of cyclically-adjusted fiscal balances between 2010 and 2011 (in columns 1 and 2) as well as within the bottom decile of the distribution (in column 3). The output gap is used to adjust fiscal balances in columns 1 and 3, against GDP growth in column 2. Column (4) reports the results where the upper decile of the distribution of the level of public debt between 2009 and 2011 is used a metric of fiscal policy stance. The treatment period is August 2009-December 2011.

Appendices

Table A1: Country details on SDR holdings and 2009 allocations

	Pre-treatment (Jul/09)	General alloc. (Aug/09)	Special alloc. (Sep/09)	As of Dec/11	% change Sep/09-Dec/11
Afghanistan	0	120	9	128	0
Albania *	6	36	10	50	-2
Algeria	6	930	139	1,074	0
Angola *	0	212	61	256	-6
Antigua *	0	10	3	0	-96
Argentina	321	1,569	132	2,053	2
Armenia *	2	68	20	3	-96
Australia	112	2,399	214	3,012	6
Austria	193	1,388	169	1,690	-3
Azerbaijan *	2	119	34	155	0
Bahamas, The	0	97	18	114	0
Bahrain	9	100	18	128	0
Bangladesh	1	395	68	463	0
Barbados	0	50	6	56	0
Belarus *	1	286	82	373	1
Belgium	367	3,414	424	4,234	1
Belize *	2	14	4	20	0
Benin	0	46	4	50	0
Bhutan *	0	5	1	6	0
Bolivia	27	127	10	165	0
Bosnia-Herzegovina	0	125	15	0	-97
Botswana	40	47	6	87	-6
Brazil	359	2,251	278	2,591	-10
Brunei Darussalam *	13	160	44	216	0
Bulgaria *	4	475	136	611	-1
Burkina Faso	0	45	4	48	0
Burundi	7	57	3	85	27
Cambodia	0	65	4	68	0
Cameroon	3	138	15	16	-90
Canada	645	4,722	487	5,840	0
Cape Verde	0	7	1	5	-38
Central African Rep.	0	41	3	3	-94
Chad	0	42	3	0	-100
Chile	37	635	60	791	8
China	792	5,997	756	7,806	-1
Colombia	131	574	50	743	-2
Comoros	0	7	1	10	47
Congo, Dem. Rep.	0	395	29	353	-16
Congo, Republic of	0	63	7	70	0
Costa Rica	0	122	11	133	0
Cote d'Ivoire	0	241	32	273	0
Croatia	0	271	33	303	0
Cyprus	2	104	10	140	21
Czech Republic *	14	607	173	750	-6
Denmark	170	1,218	135	1,462	-3
Djibouti	0	12	2	10	-29
Dominica	0	6	1	5	-37
Dominican Republic	0	162	15	22	-88
Ecuador	17	224	31	15	-11
Egypt	68	700	63	819	-1
El Salvador	25	127	12	164	0
Equatorial Guinea	0	24	1	21	-19
Eritrea *	0	12	3	4	-73
Estonia *	0	48	14	62	0
Ethiopia	0	99	18	97	450
Fiji	7	52	8	51	-24
Finland	155	937	110	1,119	-7
France	629	7,961	1,094	9,594	-1
Gabon	0	114	18	133	0
Gambia, The	0	23	2	25	0
Georgia *	5	111	33	144	1
Germany	1,332	9,643	1,205	11,897	-2
Ghana	0	274	17	286	-2
Greece	15	610	69	554	-20
Grenada	0	9	2	10	-3
Guatemala	1	156	17	174	0
Guinea	0	79	6	61	-28
Guinea-Bissau	0	11	2	12	0
Guyana	0	67	5	2	-7
Haiti	4	61	4	69	-1
Honduras	0	96	9	100	-4
Hungary *	0	770	221	549	-45
Iceland	5	87	9	463	376
India	0	3,083	215	2,885	-13
Indonesia	21	1,541	200	1,762	0
Iran	283	1,110	72	1,536	5
Iraq	94	881	185	1,136	-2
Ireland	64	622	67	632	-16
Israel	8	688	89	827	5
Italy	127	5,230	643	6,018	0
Jamaica	0	203	18	206	-7
Japan	1,914	9,869	1,524	12,861	-3
Jordan	2	126	19	146	0
Kazakhstan *	1	271	73	345	0
Kenya	8	201	22	14	-94
Kiribati *	0	4	1	5	0
Korea	57	2,170	162	2,246	-6
Kosovo *	0	44	12	55	0
Kuwait	153	1,024	265	1,445	0
Kyrgyz Republic *	25	66	19	107	0
Lao, PDR	10	39	2	51	0
Latvia *	0	94	27	94	-22
Lebanon	21	151	38	193	-8
Lesotho	4	26	3	36	13
Liberia	21	96	7	141	14
Libya	588	833	181	1,609	0
Lithuania *	0	107	30	137	0
Luxembourg	14	207	23	244	0
Macedonia, FYR	1	51	6	0	-99
Madagascar	0	91	7	94	-4
Malawi	0	51	4	94	-4
Malaysia	148	1,102	105	1,285	-5
Maldives	0	6	1	7	-10
Mali	0	69	4	73	0
Malta	12	76	9	91	-5
Marshall Islands *	0	3	1	3	0
Mauritania	0	48	4	1	-98
Mauritius	19	75	6	100	0
Mexico	293	2,337	224	2,649	-7
Micronesia *	1	4	1	6	0
Moldova *	0	91	26	1	-83
Mongolia *	0	38	11	45	-7
Montenegro *	0	20	5	26	0
Morocco	12	436	40	444	-9
Mozambique *	0	84	25	108	-1
Myanmar	0	192	11	1	-100
Namibia *	0	101	29	5	-96
Nepal	5	53	7	58	-10
Netherlands	578	3,827	479	4,739	-3
New Zealand	14	663	49	828	14
Nicaragua	0	96	9	114	9
Niger	1	49	5	54	0
Nigeria	0	1,300	219	1,675	10
Norway	279	1,239	156	1,523	-9
Oman	13	144	29	175	-6
Pakistan	98	766	52	686	-24
Palau, Republic of *	0	2	1	3	0
Panama	0	153	18	171	0
Papua New Guinea	0	98	19	10	-92
Paraguay	29	74	7	111	0
Peru	6	473	45	524	0
Philippines	7	652	69	728	0
Poland *	34	1,015	290	1,171	-13
Portugal	80	643	110	793	-5
Qatar	29	196	43	269	0
Romania	45	764	145	384	-59
Russia *	2	4,407	1,264	5,684	0
Rwanda	20	59	4	83	0
Samoa	3	9	1	13	0
San Marino *	1	13	3	15	-8
Sao Tome	0	6	1	4	-42
Saudi Arabia	482	5,178	1,309	6,725	-4
Senegal	0	120	10	130	0
Serbia	0	347	42	2	-100
Seychelles	0	7	1	7	-14
Sierra Leone	26	77	5	118	-3
Singapore	241	639	88	868	-11
Slovak Republic *	1	265	76	325	-5
Slovenia	8	172	19	209	5
Solomon Islands	0	8	2	9	0
Somalia		33	4	0	0
South Africa	223	1,385	180	1,788	11
Spain	46	2,260	269	2,656	15
Sri Lanka	0	307	18	3	-99
St. Kitts and Nevis *	0	7	2	8	14
St. Lucia	2	11	3	15	19
St. Vincent	0	6	1	1	-88
Sudan	0	126	16	125	0
Suriname	0	68	12	81	18
Swaziland	3	38	4	44	11
Sweden	189	1,776	227	2,203	12
Switzerland *	156	2,564	724	3,204	18
Syrian Arab Republic	37	218	25	279	10
Tajikistan *	0	65	18	70	8
Tanzania	0	147	12	157	6
Thailand	86	802	84	973	10
Timor-Leste *	0	6	2	8	33
Togo	0	54	5	59	9
Tonga *	0	5	2	7	27
Trinidad and Tobago	1	249	26	276	11
Tunisia	3	212	26	242	12
Turkey	13	883	76	971	9
Turkmenistan *	0	56	14	70	25
Uganda	0	134	10	142	6
Ukraine *	3	1,017	292	12	-99
United Arab Emirates	11	454	76	542	17
United Kingdom	261	7,961	261	9,404	14
United States	6,080	27,539	2,877	35,795	6
Uruguay	2	227	16	246	7
Uzbekistan *	0	204	59	263	29
Vanuatu *	1	13	4	1	-89
Venezuela	14	1,971	255	2,239	13
Vietnam	1	244	23	272	11
Yemen, Republic of	6	181	23	166	-11
Zambia	6	363	38	402	9

Note: SDR holdings in SDR mln. Data obtained from the IMF Finance Department. *Countries that received SDRs for the first time as a result of both the General and Special SDR Allocation.

Table A2: Treated countries

Antigua Barbuda	Fiji	Liberia	Sierra Leone
Austria	Finland	Luxembourg	Slovak Rep.
Bahamas	France	Madagascar	Slovenia
Barbados	Gambia	Malta	Solomon Islands
Belarus	Georgia	Mauritania	Spain
Belgium	Germany	Moldova	Sri Lanka
Brunei Darussalam	Ghana	Netherlands	St Vincent
Burundi	Greece	Nicaragua	Sudan
Canada	Grenada	Niger	Suriname
Central African Rep.	Guinea Bissau	Pakistan	Tajikistan
Cote d'Ivoire	Guyana	Panama	Togo
Cyprus	Haiti	Portugal	Tonga
Dominica	Ireland	Rwanda	United Kingdom
Dominican Rep.	Italy	Samoa	United States
Ecuador	Jamaica	Sao Tome Principe	Venezuela
Ethiopia	Laos	Senegal	Zambia

Note: In the baseline estimates treated countries are defined as those receiving a large allocation of SDRs, i.e. one in excess of 10% of their existing international reserve holdings. This threshold corresponds to the top 60-percentile of the distribution of the SDR allocation. It is varied in the robustness checks (see Table 6 for further details).

Table A3: Countries with large fiscal deficits

Austria	Lithuania
Bahrain	Myanmar
Botswana	New Zealand
Bulgaria	Portugal
Colombia	Qatar
France	Romania
Georgia	Russia
Germany	Saudi Arabia
Guinea	Slovak Republic
Hungary	St Kitts
Iceland	Swaziland
Irak	Turkey
Ireland	U.A.E.
Kazakhstan	United Kingdom
Kuwait	United States

Note: In the baseline estimates countries with large fiscal deficits are defined as those countries within the bottom quartile of the distribution of cyclically-adjusted fiscal balances between 2010 and 2011.

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