Local Ownership of IMF Conditionality Programs: Conceptualization, Measurement, and Validation*

Nikitas Konstantinidis

IE University

School of Global & Public Affairs

nikitas.konstantinidis@ie.edu

Bernhard Reinsberg

University of Glasgow

School of Social & Political Sciences

bernhard.reinsberg@glasgow.ac.uk

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Abstract

The shifting emphasis on performance evaluation and accountability in the context of lending and/or foreign-aid arrangements sponsored by international organizations (IOs) has brought

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to the fore the question of local ownership of reforms. While the concept of ownership has featured highly in recent academic and policy debates over the effects of IO conditionality, it still remains elusive, ill-identified, and under-specified. In this paper, we focus on International Monetary Fund (IMF) programs and define local ownership with respect to the counterfactual level of *de jure* structural reforms that would be achieved in the absence of IMF conditionality. We then use the synthetic control method (SCM) to identify continuous levels of ownership as a function of a treatment effect on treated compliers (TETC) and operationalize the concept of ownership over external- and financial-sector conditionality across a restricted sample of uninterrupted IMF arrangements (1980-2014). Furthermore, we probe the criterion and construct validity of our measure with respect to known determinants, proxies, and outcomes of ownership. We argue that ours is a reliable, replicable, valid, robust, and systematic measure of ownership that can help better identify and estimate the indirect relationship between program design and policy implementation.

Keywords: IMF conditionality; ownership; reforms; synthetic control method

> *When a country borrows from the IMF, its government agrees to adjust its economic policies to address the macroeconomic imbalances that led it to seek financial aid. These policy adjustments are conditions for IMF loans and serve to ensure that the country will be able to repay the IMF. This system of conditionality is designed to promote national ownership of strong and effective policies (International Monetary Fund 2019).*

"Ownership" is one of those buzzwords that features prominently in the policy jargon surrounding the conditionality programs of major international organizations (IOs)—namely, international development organizations (IDOs) such as the World Bank (WB) and the United Nations Development Program (UNDP), and international financial institutions (IFIs) such as the International Monetary Fund (IMF) or even the European Union (EU)—as well as national foreign-aid agencies. Local (or else government)[1] ownership of IO-sponsored financial arrangements is regarded as an essential ingredient for the *de facto* on-the-ground implementation of adjustment reforms (Henisz and Mansfield 2019), the successful completion of lending and/or foreign-aid programs, the credibility of such international financial assistance schemes, the enhancement of borrowing countries' creditworthiness in global capital markets (International Monetary Fund 2001), as well as the improvement of foreign-aid effectiveness (Dornan 2017; Keijzer et al. 2020). By contrast, once such programs are in place, the absence of ownership may bring about their interruption or utter failure, which bodes ill for the country's short-term financing needs and the overall welfare of the population.

As a result, the enhancement of program ownership has featured among the main objectives in the design of IO conditionality policies. And yet, while ownership is intuitively understood as buy-in, political will, and the assumption of responsibility on the part of a borrower government for taking certain necessary adjustment measures, the concept remains theoretically elusive and

[1] Henceforth, we use these two terms interchangeably.

empirically ill-identified. Hence, in this paper we propose a systematic approach toward measuring program ownership using the International Monetary Fund (IMF) as our main point of reference.

International financial assistance programs generally comprise some form of conditionality, i.e., a corpus of conditions attached to the granting of financial assistance in the form of (concessional or non-concessional) loans or grants in pursuit of goals deemed desirable by the donor organization itself and/or the target government. In the case of the IMF, the fallout from the Asian and emerging-market programs of the 1990s and the "relevance crisis" of the 2000s heightened demands for a structural overhaul of the Fund's conditionality policies both in terms of streamlining the scope of conditionality and adopting a more flexible outcomes-based approach to program design (Independent Evaluation Office of the International Monetary Fund 2018; International Monetary Fund 2009b; Khan and Sharma 2003). Ever since, IMF program design has been informed by the official view of ownership as the "willing assumption of responsibility for an agreed program of policies, by officials in a borrowing country who have the responsibility to formulate and carry out those policies, based on an understanding that the program is achievable and is in the country's own interest" (International Monetary Fund 2001, 6). This definition is predicated on a strong set of assumptions, namely that (i) there is no conflict of interest between the IMF and the borrowing government, (ii) the government "shares with the IMF both the objectives of the program and an understanding of the appropriate economic model linking those objectives to economic policy" (Khan and Sharma 2003, 235), and (iii) the IMF trusts in the target government's willingness and/or ability to comply, reform, and repay its loans. According to the official IMF view, conditionality and ownership should hence be viewed as complementary insofar as borrowers are more likely to gain ownership of conditions that make payback more likely and continued lending more readily available (International Monetary Fund 2001).

Yet, a contrapositive interpretation of this line of reasoning would imply that once an official arrangement has been agreed, approved, and launched, program failure can only be attributed to lack of government ownership, thereby introducing a dubious sense of infallibility in IMF program

design. Moreover, the conceptualization of ownership in an idealized environment of common beliefs, shared (*ex ante* and *ex post*) preferences, and perfect and complete information gives rise to the following conundrum: If it is in a country's best interest to implement a certain program in question, then why make loan disbursements explicitly and irrevocably conditional on a required (and avowedly desired) set of reforms (Drazen 2002)? In other words, the official definition of ownership appears oversimplified, tautological, and fallacious insofar as it presumes such "loans-for-reforms" contracts as complete; as such it may not credibly inform effective policy design.

To avoid such logical pitfalls, one may simply justify the need for conditionality on the basis of some type of informational asymmetry, preference heterogeneity, lack of administrative capacity, or a non-zero probability of program failure. In other words, we consider the attachment of conditions (both hard and soft)[2] to an IO-sponsored lending arrangement as a necessary consequence of some type of *ex ante* asymmetry or incompleteness of these contracts (Dixit 2000; Hart and Holmström 1987), where such imperfections may either stem from informational or commitment problems. In that light, the various types of conditions can serve as (i) confidence-building measures against preference heterogeneity between a mistrustful IO and a restive crisis-ridden government over the means and/or the ends of a stabilization and adjustment program (e.g., Beazer and Woo 2016), (ii) commitment devices against diverging *ex post* interests and time-inconsistent preferences on the part of the contracting government (e.g., Candel-Sánchez 2021; Diwan and Rodrik 1992), (iii) costly signals of government resolve to restore national debt sustainability and creditworthiness in the eyes of international capital markets (e.g., International Monetary Fund 2001), (iv) domestic agenda-setting bargaining tools vis-à-vis recalcitrant special interests (e.g., Drazen 2002), (v) expert policy recommendations to national delegations short on technical capacity (e.g., Drazen and Isard 2004), or, finally, (vi) rhetorical ploys of cheap talk employed by reform-minded

[2] *Hard* conditions are those whose implementation is a prerequisite for the successful completion of a program review and the disbursement of funds, whereas *soft* conditions refer to broad targets and policy benchmarks meant to assess the actual progress of an adjustment program.

governments seeking to deflect the blame for painful reforms to external actors and thus win over the acquiescence of a skeptical public opinion (e.g., Vreeland 2003).

Accordingly, the design of country-specific conditionality—in terms of its focus (policy- vs. outcome-based conditions), size (number of conditions), scope (number of policy areas covered), quantifiability (quantitative vs. structural conditions), timing (prior vs. posterior actions), and enforceability (hard performance criteria vs. soft indicative targets and benchmarks)—should optimally depend on which of the above informational asymmetries or conflicts of interest between the creditor organization ("principal"), the debtor government ("agent"), domestic actors (special interests, public opinion), and/or international capital markets are most pronounced in each case. Otherwise put, the contractual environment should determine the optimal design of conditionality arrangements either in terms of fine-tuning the mix and pacing of reforms, maximizing the probability of successful implementation, tailoring them to local conditions and local knowledge (Marchesi et al. 2011), screening between good and bad debtors (Marchesi and Thomas 1999), or simply enhancing program ownership (Drazen 2002; Drazen and Isard 2004).[3]

Moreover, unlike the official approach, the above line of reasoning suggests that program acceptance does not necessarily presuppose local ownership of the adjustment program, construed

[3]Over the past couple of decades, there has been a lot of thinking on the optimal design of conditionality by scholars and practitioners, namely in terms of fine-tuning the mix and pacing of reforms (Rodrik 2006), maximizing the probability of successful implementation of such programs (Ivanova et al. 2001), tailoring them to local conditions and local knowledge (Marchesi et al. 2009), screening between good and bad debtors (Marchesi and Thomas 1999), and enhancing the degree of program ownership (Drazen 2002; Drazen and Isard 2004). As a result, IOs such as the IMF and the WB have become much more attuned to the effects of domestic factors such as political feasibility constraints, the domestic level of polarization, the strength of domestic anti-reform groups, and the electoral cycle (Alesina et al. 2020; Rickard and Caraway 2014) on the political economy of reforms.

as buy-in, administrative capacity, and/or political will for reform and adjustment, all of which are effectively a function of the target government's latent set of policy preferences and capabilities. Without prior knowledge over which one(s) of the function(s) listed above the design of any conditionality program is supposed to serve or else a deep understanding of the specifics of a case, ownership is not directly observable or measurable either *ex ante* or *ex post*. For example, a text analysis of an IMF letter of intent (LoI) and memorandum of understanding (MoU)—i.e., the documents outlining planned economic reforms to be undertaken in exchange for an IMF loan—cannot systematically capture *ex ante* program ownership as no external observer may unequivocally ascribe authorship of different parts of the contract to either the IMF staff or the borrowing government. Nor would a content analysis of political speeches and communiqués make for an unbiased estimate of revealed government preferences as any rhetorical ploy to attack the IMF (say in order to deflect the blame or neutralize the opposition) may simply amount to a noisy signal that fails to distinguish between cases of high and low *ex post* program ownership. For similar reasons, existing measures of ownership, both direct and indirect ones through the use of proxies such as government ideology, partisanship, cabinet durability, legislative majorities, may also suffer from bias, unreliability, and other methodological limitations.

As a result, the jury is still out on the relationship between IMF program design, ownership, and program implementation as the concept of ownership and the manner in which it mediates between domestic/systemic causes and effects remain conceptually convoluted and inadequately operationalized. To address this gap in the IMF and the broader conditionality literature, we seek to (i) propose a systematic approach toward measuring and operationalizing the concept of ownership and (ii) validate that measure against some known determinants, proxies, and effects of ownership. Building upon an intuitive conceptualization, we stipulate that ownership occurs in a situation in which the policy content of a program is similar to what the country would have organically chosen itself in the absence of explicit conditionality (Drazen 2002; Bird and Willett 2004). Based on this operationalizable definition of ownership, we use tools of causal inference—viz., the

synthetic control method—to estimate unobservable and counterfactual adjustment policies predicated on an underlying set of government preferences. We systematically measure ownership over a sector-specific set of conditions as a function of a treatment effect of hard IMF conditionality on countries' (observable and verifiable) *de jure* reform trajectories, which effectively amount to adjustment measures enacted through either executive or legislative acts. This approach allows us to conceptualize, operationalize, and measure ownership as a latent variable that mediates between program design and implementation.[4] Due to data limitations, we are not able to create a complete dataset of ownership measures over the universe of IMF arrangements to date; instead, we apply our ownership measure on a restricted sample of such arrangements.

Our paper contributes to the vast literature on the political economy of IO-sponsored lending and foreign-aid programs (e.g., Dreher 2009; Nelson 2014; Nooruddin and Simmons 2006). While there is ample theoretical work discussing the role of ownership for development outcomes, empirical measurements of ownership have been lagging behind. Moving beyond *ad hoc* proxies, our approach is the first to offer a systematic, valid, and reliable measure of ownership that is also replicable beyond IMF conditionality programs. In fact, ownership is a prominent theme in the literature on foreign aid and development policy (e.g., Candel-Sánchez 2021; Dornan 2017), where it has been extensively argued through the use of qualitative evidence that local ownership is key for achieving the Sustainable Development Goals (Keijzer et al. 2020). However, it has been also shown that donors may undermine ownership by pursuing other goals such as "aid effectiveness" and "value for money" that may involve centralization of control and the use of an excessive amount of conditions (Dornan 2017; Honig 2019; Swedlund and Lierl 2020).

In what follows, we start by discussing the role of ownership within the broader research agenda

[4]In this regard, ownership can only be endogenously defined in respect to the contractual relationship between a creditor organization (principal) and a borrowing country or government (agent). In the absence of external conditionality constraints, the concept of ownership of a reform package becomes tautological and essentially vacuous.

on the political economy of IO lending. Then, we proceed to conceptualize ownership as a latent mediating variable and provide a systematic identification and measurement of the concept by applying the synthetic control method (Abadie et al. 2010, 2015; Abadie and Gardeazabal 2003). Subsequently, we operationalize our measure of ownership over a restricted sample of IMF programs completed between 1980 and 2014—using hard external- and financial-sector conditions as our treatment—and gauge its reliability by deriving bootstrapped standard errors. Later on, we illustrate its face validity against qualitative anecdotal evidence from primary and secondary sources evidence in the form of an in-depth case study of the IMF's involvement in Indonesia (1997-2003).[5] Finally, we assess its criterion and construct validity through simple bivariate regressions against some known predictors, proxies, and outcomes of ownership (or the lack thereof).

Ownership and the political economy of IMF lending

The extensive literature on the political economy of IMF conditional lending delves into both the determinants and the consequences of IMF program design. IMF conditionality arrangements may comprise *quantitative* adjustment measures (e.g., short-term external debt, net international reserves, public external arrears, social-safety-net expenditures) and/or *structural* reforms (e.g., privatizing a state-owned enterprise, reforming the central bank, reducing public sector employment) criteria, as well as a mix of *hard* prior actions, quantitative performance criteria, and structural performance criteria, and/or *soft* indicative targets and structural benchmarks.[6] Failure to implement

[5] See Online Appendix B for more qualitative evidence in the form of an extended discussion of the 1998 IMF arrangement in Bosnia and Herzegovina.

[6] Accordingly, IMF lending facilities may be divided between *non-concessional* (e.g., SBAs, EFFs) vs. *concessional* (e.g., ESAFs, PRGFs/ECFs) loans, and *regular* (e.g., SBAs, PCLs) vs. *special* (e.g., SRFs, CCLs) arrangements, depending on the severity of the crisis, the level of interest rates, and the grace and maturity periods.

hard conditions can lead to program interruption or termination. The attractiveness of conditionality as a tool to gain leverage over target countries' political and economic reforms (or else as a risk-sharing insurance mechanism that allows the target country to receive the benefits it seeks) is such that most major countries or organizations active in international policies now design such programs (Stone 2002).

Figure 1 below illustrates the broader research design of this literature and distinguishes between the various links discussed in this body of work: Link (a) encompasses a large number of papers that have sought to explain the design and the determinants of IMF conditionality *per se* by studying the effects of both *systemic* factors—such as major donor countries' geopolitical interests (Dreher et al. 2009, 2015; Stone 2008; Thacker 1999), a country's geopolitical proximity to the IMF's major shareholders and especially the U.S. (Dreher and Jensen 2007), preference heterogeneity among donor states (Copelovitch 2010), financial interests (Gould 2003), and bureaucratic rent-seeking (Dreher and Vaubel 2004; Vaubel 1986)—and *domestic* factors—such as the electoral cycle (Rickard and Caraway 2014), citizens' economic interests (Caraway et al. 2012), and the incumbent's ideological orientation (Nelson 2014). While some studies have dealt with the preliminary question of whether financial assistance will be granted in the first place, others have shown that the design of conditionality programs themselves obeys a geopolitical logic (Dreher et al. 2015; Dreher and Lang 2019) as much as an economic or developmental one (Vreeland 2007).

Link (b) in Figure 1 captures the direct effects of program design on aggregate policy outcomes and *de facto* program implementation.[7] One strand of this literature has sought to assess the direct welfare effects of implemented adjustment reforms (in terms of their content) on political, socioe-

[7]The term *de facto* here refers to the real on-the-ground implementation of and the sense of commitment to the spirit—not just to the letter (*de jure*)—of a reform program (Henisz and Mansfield 2019).

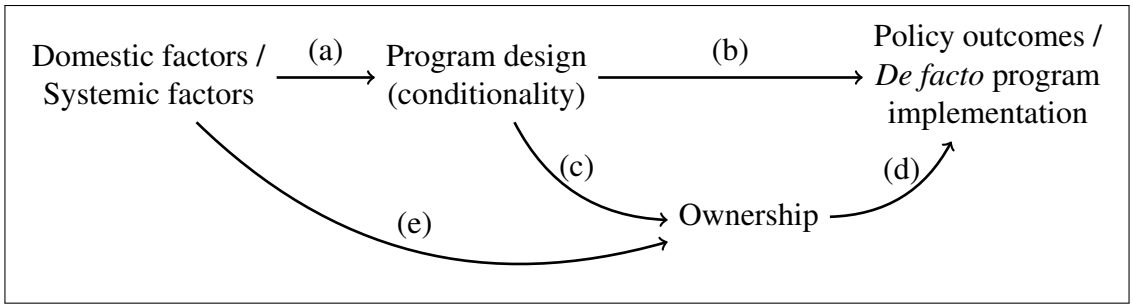

Figure 1: A graphical illustration of the broader research design of the literature on the political economy of IMF lending.

conomic, and developmental outcomes. Some, for example, have argued that the nature and scope of IMF conditionality can undermine democratic institutions and bureaucratic capacity (Reinsberg et al. 2019), set unattainable standards of austerity, hamper economic development and social justice (Przeworski and Vreeland 2000), and lead to poverty especially among those already poor (Stiglitz 2004; Vreeland 2007). Others have found that, controlling for selection effects, successfully completed IMF programs *per se* have been more effective at enhancing growth performance among *long*-term than *short*-term users (Bas and Stone 2014). Furthermore, Jensen (2004) has shown that IMF conditionality may also reduce foreign direct investment in target countries.

Another strand of this literature takes a principal-agent approach to explaining the direct effect of systemic factors on *de facto* program implementation outcomes, i.e., whether the principal's objectives and the design of conditionality lead to either high levels of actual on-the-ground compliance or high rates of shirking, target slippage, and re-negotiation. By and large, the related IMF literature depicts a predominantly pessimistic story according to which conditions are usually not met (Killick 1997; Vreeland 2006). Program failure might occur because of the interference of powerful countries such as the U.S. that may render the threat of conditionality less credible in support of their allies (Stone 2008; Thacker 1999), or due to high monitoring and enforcement costs (Martin 2006).

Meanwhile, links (c) and (d) in Figure 1 illustrate the indirect effects of program design on *de facto* program implementation (and by extensions policy outcomes) via the mediating effect of

domestic variables such as local ownership. The contentious experience of the financial crises and corresponding IMF programs of the 1990s and early 2000s sparked a theoretical discussion on the relationship between IMF conditionality and ownership, primarily drawing upon political economy models of special interests, common agency, and heterogeneity in actor preferences (Mayer and Mourmouras 2004, 2008; Paloni and Zanardi 2006). Most of these papers start with the premise that all conditionality programs are contingent on domestic political factors such as the popularity of reform, the number of institutional veto players, administrative capabilities, and electoral timing effects, and concur that under certain circumstances conditionality may enhance the government's or the IMF's bargaining leverage vis-à-vis recalcitrant special interests opposing specific adjustment measures and structural reforms.[8] In other words, conditionality arrangements are negotiated by reform-minded governments so as to overcome the opposition of domestic special interests and thus mitigate the short-term political costs of macroeconomic and structural adjustment (Vreeland 1999; Mayer and Mourmouras 2008).

There have been three distinct approaches in the study of the relationship between IO conditionality and local ownership:[9] (i) *Preference*-based models emphasize the heterogeneity of interests between the lending IO and the target government, commitment and time-inconsistency problems (Candel-Sánchez 2021; Diwan and Rodrik 1992), moral hazard (Svensson 2000), the role of partisanship and ideology (Beazer and Woo 2016), and potential resistance by domestic "vested interests" (Drazen 2002; Mayer and Mourmouras 2004; Paloni and Zanardi 2006). In line with this approach, the need for hard and enforceable conditionality is highest when there are stark discrepancies between the respective (constrained) objectives of creditors and debtors so that the

[8]Scholars in this strand of the literature often distinguish between country and government ownership, thereby drawing a clear distinction between the perceived aggregate welfare benefits of liberalization reforms and the short-term political costs that affect the government's intrinsic political will for such reforms.

[9]See Dreher (2009) for an excellent review of this literature.

negotiated design of these "loans-for-reforms" contracts basically reflects a series of policy concessions.[10] Moreover, conditionality may help reform-minded governments neutralize the power of domestic veto players by *scapegoating* (or deflecting the blame onto) external creditors and IOs (Bird and Willett 2004; Vreeland 1999, 2003). (ii) *Capacity*-based approaches construe ownership as the technical, bureaucratic, and fiscal capacity to implement certain reforms (Drazen and Isard 2004). The key question then becomes how the technical design of conditionality—especially the balance between hard and soft conditions—can enhance state capacity, program effectiveness, and policy learning.

Finally, (iii) *informational* approaches consider program design as the outcome of either a non-cooperative bargaining game with incomplete information or a deliberative process of consensus-building. In the former case, creditor and debtor beliefs about each other's motives and constraints do not generally coincide in equilibrium, while the principal organization often suffers from lack of context-specific knowledge (Marchesi et al. 2011). Furthermore, making financial assistance conditional on the implementation of certain reforms may end up undermining the target government's intrinsic belief in the effectiveness of the program and the necessity for reforms (Konstantinidis and Karagiannis 2020). In the latter case, the deliberated design of the contract reflects a convergent set of creditor and debtor preferences and beliefs over the economic model that maps adjustment policies into socioeconomic outcomes (Khan and Sharma 2003). At the same time, letters of intent (LoI) and memoranda of understanding (MoU) represent debtors' signals to international capital markets of commitment to carrying out the necessary reforms. This represents a view of conditionality and ownership as *complements* (International Monetary Fund 2001).

While strand (c) of the literature does not effectively focus on why some conditionality programs are more successful than others (Barro and Lee 2005; Killick 1997), existing work on link (d) between ownership and the *de facto* implementation of policy reforms (e.g., Bird and Willett

[10]Conversely, the absence of hard conditions indicates the lack of any conflict of interest—and hence the highest level of ownership—which would contradict the official IMF view.

2004; Wei and Zhang 2010) remains rather scant arguably due to the theoretical murkiness and empirical elusiveness of the concept of ownership. Experimental evidence by Dal Bó et al. (2010) has shown that home-grown policies and institutions tend to be more effective at improving behavior or enhancing performance than reforms transplanted from outside. Therefore, ill-designed and excessive levels of conditionality may undermine ownership and lead to poor long-term *de facto* outcomes even if medium-term targets of *de jure* adjustment have been met (Henisz and Mansfield 2019).

Finally, link (e) in Figure 1 refers to existing attempts at estimating local ownership through the use of indirect proxies other than the design of the program itself. *Quantitative* scholarship has relied on proxies such as the government's left-right orientation and neoliberal bias (e.g., Beazer and Woo 2016; Nelson 2014), the partisan ideology of the country negotiation team (e.g., Chwieroth 2013), public opinion toward IO involvement, cabinet durability, and legislative support for mandated reforms (e.g., Alesina et al. 2020). Some have even considered aspects relating to the history between borrowing countries and IOs and the design of the loan program itself: For example, Wei and Zhang (2010) proxy for countries' "willingness to reform" through their track record in implementing non-trade structural benchmarks and the share of non-prior actions out of total conditions in the first program year. Meanwhile, *qualitative* scholarship has considered textual case-specific evidence to gauge ownership. Through methods such as elite interviews and quantitative text analysis of official (archival) documents, political speeches, and media sources, case-specific research has sought to capture ownership in a more direct manner within a narrower set of observations (Arpac and Bird 2009; Dornan 2017; Keijzer et al. 2020).

While existing approaches as represented by link (e) amount to a useful first cut at estimating ownership, they are likely to be biased and unreliable. For one, indirect quantitative proxies of ownership drawn from a pool of domestic political variables may fail to capture the specifics of particular cases and generate erroneous results. This validity bias can be aptly exemplified by the case of the third Greek bailout in 2015, which comprised an unprecedentedly high cross-partisan

level of legislative support[11] as well as ample scapegoating tactics and rhetorical ploys, coupled with an avowedly very low level of ownership.[12] In a similar vein, proxies based on a country's historical relationship with the IMF are unhelpful because poor implementation of past programs might be due to factors other than limited ownership; besides, such proxies do not effectively capture the contemporaneous level of program ownership of any given incumbent. At the same time, the share of (non-)prior actions in IMF programs only reflects the IMF staff's subjective assessment of the borrower's willingness to reform rather than an objective measurement of government ownership and is, therefore, endogenous to the relationship between conditionality and ownership.

Qualitative approaches to measuring ownership also suffer from certain methodological flaws. Interview-based or archival accounts of policy choices and political decisions may be subject to hindsight bias and a predilection towards revisionist history, thus rendering it effectively impossible to determine the true counterfactual state of affairs. Furthermore, rhetorical scapegoating tactics aiming at deflecting the blame for painful reforms to external actors effectively amount to cheap talk, which makes it very hard to determine *ex ante* the extent to which they reflect the true underlying preferences of incumbents across a wide array of observations. Finally, in light of the non-transparent nature of negotiations between IOs and debtor governments, ownership is not directly observable by analyzing the content of negotiated documents (e.g., the IMF's LoI or MoU) as there is no systematic way of ascribing authorship to specific aspects of the agreement.[13] In

[11]The third Greek bailout program was ratified by parliament with the widest level of legislative support out of the three (garnering 222 votes out of 300 from government backbenchers and moderate pro-EU opposition MPs).

[12]In an interview conducted on 14 July 2015, i.e., a couple of days after the signing of the bailout agreement, incumbent Greek Prime Minister Alexis Tsipras was quoted as follows: "I am fully assuming my responsibilities, for mistakes and for oversights, and for the responsibility of signing a text that I do not believe in, but that I am obliged to implement" (The Guardian 2015).

[13]Selection effects are key here. What is selected into the final agreement can be equally infor-

sum, most existing measures of ownership arguably suffer from methodological flaws that render them imperfect, inconsistent, unreliable, and/or unsystematic.

To sum up, strand (b) of the literature on the political economy of IMF lending, which focuses on the *direct* effects of program design on policy/implementation outcomes, under-identifies the relationship and hence suffers from omitted variable bias—even after controlling for selection effects. On the other hand, in the absence of a consistent, reliable, and systematic exogenous measure of the latent variable of program ownership (link (e)), strands (c) and (d) on the *indirect* effects mostly amount to theoretical work (with interesting normative implications); there have only been but a few attempts at rigorously identifying, operationalizing, and testing this indirect channel of causality (see, e.g., Wei and Zhang 2010). The goal of this paper is to make a strong contribution toward addressing this gap in the literature. In what follows, we seek to conceptualize, identify, operationalize, and validate our latent measure of ownership using the synthetic control method (Abadie et al. 2010; Abadie and Gardeazabal 2003; Xu 2017).

Conceptualizing and identifying ownership

In what follows, we seek to conceptualize and identify ownership of external (IO-sponsored) conditionality programs attached to particular lending facilities and viewed as "second-order" extrinsic incentive schemes (Dixit 2000) and incomplete contracts (Hart and Holmström 1987) whose design accounts for potential preference heterogeneities and informational asymmetries between creditors and debtors. Accordingly, we adhere to a counterfactual and continuous conceptualization of ownership as "a situation in which the policy content of the program is similar to what the country

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mative to what is selected out of it. In that vein, Erbas (2003) finds that greater parsimony in terms of the number of conditions can enhance local ownership, while Drazen and Isard (2004) argue to the contrary that streamlining can be counterproductive as those conditions that will probably be eliminated first will be the ones for which ownership is highest.

would have chosen in the absence of IMF [IO] involvement" (Khan and Sharma 2003, 235). This definition allows us to identify an ideal upper bound on the degree of program ownership by assuming perfect alignment between the *actual* level of *de jure* reforms and the *counterfactual* reform trajectory, where the latter is predicted by a given set of underlying domestic and systemic factors.

By way of formalization, we assume that country i's sector-specific level of adjustment $r_{it}^{j} \in \mathbb{R}$ in period t through the enactment of *de jure* reforms in policy area j by the incumbent may be modeled along a unidimensional scale, where higher values correspond to more pro-globalization adjustment measures. Let $R_{it} = \left[r_{it}^{j}\right]_{j=1}^{J}$ denote the economy-wide reform package implemented in period t across the full range of policy areas J. An initial IO-sponsored loan program $P_{iT}\left(L_{iT};C_{iT}\right)$ of time-length T for country i—as outlined in an official letter of intent (LoI) and expounded in a memorandum of understanding (MoU)—comes into force, i.e., $e_{i0} = 1$ at time $t = 0$ subject to the agreement of both parties (e.g., the IMF and the target government). It normally comprises a pre-specified schedule of financial loan tranches and future repayments (possibly including a "grace period") $L_{iT} = (l_{i0}, l_{i1}, \ldots, l_{iT})$ and a conditional structural adjustment program C_{iT}, where $C_{iT} = \left[c_i^j\right]$ is a $1 \times J$ conditionality vector that captures whether at least one *hard*[14] *de jure* policy condition $c_i^j \in \{0,1\}$ applies in each policy area $j \in J$ throughout the duration of the arrangement T.[15]

In line with the literature on the political economy of ownership and international lending (Drazen 2002; Mayer and Mourmouras 2004), we model the target government's latent preferences over (constrained or unconstrained) structural reform packages R_{it} through a quasi-concave,

[14] *Hard* conditions are those whose implementation is a prerequisite for the successful completion of a program review and the disbursement of funds. In the case of the IMF, those include prior actions that a country agrees to enact before the IO approves financing, quantitative performance criteria, and structural performance criteria.

[15] Formally, $\bar{J}_{iT} = \{j \in J | c_{it}^{j} = 1 \text{ for some } t = 0, 1, \ldots, T\}$ denotes the initial full scope and sectoral coverage of conditionality as outlined in an official LoI and expounded in an MoU.

continuous, and twice differentiable function $s_{it}\left(R_{it}|P_{it};D_{it},S_t\right)$, which amounts to a time-varying reduced-form political support function of a weighted average between general welfare and financial contributions from special interest groups (Grossman and Helpman 1994). Political support for reforms at time t is conditional on the accepted terms of the program $\left(P_{it}\right)$ and a host of time-varying (institutional and political) *domestic* $\left(D_{it}\right)$—e.g., ideological profile of the government, electoral mandate,[16] public opinion, administrative capacity, electoral institutions, veto players, etc.—and *systemic* $\left(S_t\right)$—e.g., foreign competition, financial contagion, diffusion, learning, etc.—factors. These factors, which may be idiosyncratic and privately known thereby introducing potential ideological bias and informational asymmetries, will influence the design of both the conditional adjustment program C_{it} itself and the government's *de jure* reform output r_{it} (see links *a* and *e* in Figure 1).

Local ownership then becomes a function of the target government's latent preferences. In other words, we adopt a "revealed-preferences" approach to extrapolate the utility cost of abiding by the conditionality constraint from observed and counterfactual *de jure* reforms. Accordingly, our measure of ownership reflects the "shadow price" (Lagrangian multiplier) of the conditionality constraint on the incumbent's latent utility (political support) from structural reforms. As a result, our ownership estimates reflect a political cost-benefit calculus—as mediated by institutions, veto players, and societal preferences—and vary depending on the weight placed on votes (general interests) as opposed to campaign contributions (narrow sectoral interests).

We consider the design of $P_{iT}\left(L_{iT};C_{iT}\right)$ as a second-best outcome within an environment of imperfect contractibility. The choice of lending facility L_{iT} and repayment terms will depend on a host of factors such as the country's short- and long-term financing needs, the severity of its balance-of-payments imbalance, the need to smoothen out the implementation of reforms, and the

[16]Note that more often than not a crisis-induced reform agenda is not part of a government's electoral mandate; adjustment is usually forced upon it by the exigencies of an economic/financial crisis.

country's income level among others. External financial loans are presumably meant to help countries spread out the costs of adjustment (but the evidence shows otherwise, programs tend to be much more front-loaded than desired by the government). The original design of P_{iT}, even if subject to mutual agreement, does not pre-determine government i's level of ownership.[17] Uncertainty over the government's latent set of preferences and beliefs does not help infer whether the imposed conditionality is desirable say because it allows governments to deflect the blame for painful reforms ("scapegoating" argument) or it enhances the country's creditworthiness in capital markets (International Monetary Fund 2001), or whether it is the necessary sacrifice of a loan-for-reforms trade-off hoisted upon a crisis-ridden country.

We assume that only *de jure* reforms enacted through either executive or legislative acts are observable and hence directly contractible, as opposed to *de facto* on-the-ground reforms $y_{it}^{j}\left(R_{it}^{j};D_{it},S_{t}\right)\in\mathbb{R}$ (Henisz and Mansfield 2019). In that vein, a government is said to be in compliance with the terms of the loan contract P_{it} at the end of review cycle t, i.e., $m_{i}^{j}=1,\ \forall j\in\bar{J}_{it}$, *if and only if* $r_{it}^{j}\geq\bar{r}_{it}^{j},\ \forall j\in\bar{J}_{it}$, where the $\bar{r}_{it}^{j}$'s denote the period- and sector-specific hard structural reform targets and policy benchmarks. Since the IO sponsor would not agree to a program that sets targets and conditions that have already been achieved, we posit that $r_{i,-1}^{j}\leq\bar{r}_{i0}^{j}\leq\ldots\leq\bar{r}_{iT}^{j}$ *if and only if* $c_{i}^{j}=1$, i.e., the mandated level of structural reforms should exceed the country's pre-program level and weakly increase throughout the duration of the IMF program. After all, successfully implemented international lending programs tend to generate higher levels of market-oriented structural reforms than those predicted by other domestic or systemic political economy factors. Finally, note that for any given type of lending facility L_{iT}, the adjustment program C_{iT} and the initial scope of conditionality $\bar{J}_{iT}$ can be renegotiated and amended *ex post* through the use of waivers $\left(w_{it}^{j}=1\right)$ that redesignate some hard structural conditions as unnecessary prerequisites for the disbursement

[17] We are measuring ownership over the initial design of the conditionality program (as stated in the letter of intent). The IO principal cannot perfectly predict whether these conditions will be implemented in the future.

of the funds, i.e., at the end of review cycle t, $C_{it} = \left[c_i^j\right] \odot \left[1 - w_{it}^j\right]$,[18] where $j = 1, 2, \ldots, J$ and $0 \leq t \leq T$.

In line with our counterfactual conceptualization and our operationalization strategy below, we identify government ownership (or the deviation therefrom) over sector-specific IO conditionality (as agreed upon and expounded in the MoU) as a function of a treatment effect on a directly observable set of *de jure* reforms. In other words, to measure program ownership, we compare the actual post-crisis reform trajectory of a country subject to IO conditionality with the latent reform trajectory of the same country without such conditionality and determine whether the actual rate of reforms would have been indeed incentive-compatible with respect to the true underlying preferences of the incumbent government without the extrinsic (positive or negative) incentivization of financial assistance and explicitly-enforced conditionality provided by the contractual terms of the arrangement.

Formally, assuming that country i has selected into a binary treatment of sector-specific conditionality at time $t = 0$, we identify government i's ownership α_{it}^j over the conditional adjustment program in sector j and t periods after program initiation as a function of a time-varying "treatment effect on the treated" (TET), i.e., conditional on $e_i = 1$,

$$\alpha_{it}^j = -\left| \frac{r_{it}^{j1*}\left(L_i, \left(C_i^{-j}, 1\right); D_{it}, S_t\right) - r_{it}^{j0*}\left(L_i, \left(C_i^{-j}, 0\right); D_{it}, S_t\right)}{r_{it}^{j0*}\left(L_i, \left(C_i^{-j}, 0\right); D_{it}, S_t\right)} \right| \qquad \text{(TET)}$$

Here, $r_{it}^{j1*} \in \underset{r_{it}}{\operatorname{argmax}}\, s_{it}\left(R_{it}; D_{it}, S_t\right)$ subject to $P_{it}^{j1} = \left(L_i, \left(C_i^{-j}, 1\right)\right)$ reflects the (observed) actual support-maximizing policy output of *de jure* reforms in sector j for the treated unit, and $r_{it}^{j0*} \in \underset{r_{it}}{\operatorname{argmax}}\, s_{it}\left(R_{it}; D_{it}, S_t\right)$ subject to $P_{it}^{j0} = \left(L_i, \left(C_i^{-j}, 0\right)\right)$ captures the (unobserved) counterfactual support-maximizing policy output of *de jure* reforms in sector j for the same unit in the absence of the sector-specific conditionality treatment.[19] Ownership α_{it}^j over *de jure* reforms in

[18] This symbol denotes the element-wise Hadamard product of vectors.

[19] Note that in line with our operationalization approach, we collapse the temporal dimension of

policy area j at time t reaches a maximum value of zero that denotes a situation of perfect alignment between the actual implemented level of *de jure* reforms and the counterfactual level that would materialize were the incumbent's reform program not subject to explicit conditionality. The lower α_{it}^{j} is found to be, the lower the estimated level of ownership since one can then infer that post-intervention adjustment and reform is primarily driven by the extrinsic (positive and negative) incentives of the IMF program itself (both in terms of the size of the loan and the overall design of conditionality). In the absence of ownership, a country would presumably adjust much less without an explicit conditionality program in place. This type of causal inference relies on the so-called "stable unit treatment value assumption" (SUTVA), according to which the realized outcome for each particular unit depends only on the value of the treatment of that unit and not on the treatment or outcome values of other units (Athey and Imbens 2017; Abadie and Cattaneo 2018).

Note that so far we are assuming perfect compliance with the treatment $(m_i^j = 1)$ for all governments willingly selecting into the program $(e_i = 1)$, i.e., $Pr(m_i^j = e_i = 1) = 1$. Nonetheless, as countries do not necessarily comply in full with IO arrangements (Vreeland 2006; Reinsberg et al. 2019), a more accurate measure of ownership would account for two-sided imperfect compliance, i.e., $0 \leq Pr(m_i^j = 1|e_i = 0) \leq 1$ and $0 \leq Pr(m_i^j = 1|e_i = 1) \leq 1$, and identify ownership as a function of a treatment effect on treated compliers (TETC), i.e., a TET conditional on $m_i^j = e_i = 1$ (Angrist et al. 1996; Marbach and Hangartner 2020). Implementation failures and lack of compliance with IO conditionality could in fact go hand-in-hand with a popular backlash against liberalization, backpedaling of reforms, and thus a negative TET. For this reason, the sample of IMF programs over which we operationalize our measure of ownership is restricted to include only those that were not partially or fully interrupted. [20]

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the conditionality program C_i and assume that $c_{it}^{j} = 1$ for all t *if and only if* $c_{i0}^{j} = 1$, i.e., a country is subject to sectoral conditionality as long as its adjustment program comprises at least one hard condition in that sector during the first review cycle.

[20] While we have verified that the assumption of full compliance holds for the purposes of our

By way of estimation, we apply the synthetic control method (SCM) for causal inference in comparative case studies (Abadie and Gardeazabal 2003; Abadie et al. 2010, 2015) and estimate the effect of the conditionality treatment applied at time $t = 0$ by comparing the evolution of an aggregate policy outcome for a unit affected by the intervention with the evolution of the same outcome variable for a synthetic control group. The synthetic control group is constructed through an optimization algorithm that seeks to minimize a loss function between the weighted combination of control units and the unit affected by the intervention in terms of factors that are predictive of the outcome. The post-intervention evolution of the outcome for the endogenously-derived synthetic control group is used to identify the counterfactual of what would be observed for the affected unit in the absence of the intervention, i.e., $r_{it}^{j0*}|e_i^j = m_i^j = 1$. In other words, the synthetic control method allows us to determine whether the actual rate of reforms would indeed be incentive-compatible with respect to the true underlying preferences of the incumbent government without the extrinsic (positive or negative) incentivization of financial assistance and explicitly-enforced conditionality provided by the contractual terms of the arrangement.

Our approach allows us to measure ownership with respect to an IO program as a whole or a specific set of IO conditions. We identify two measures of ownership (or rather deviation from a state of perfect ownership): Our first measure is defined with respect to specific points in time during the post-treatment period $(1 \leq t \leq T)$ until the program is successfully completed; year-level ownership is then inversely proxied by the absolute gap between observed and counterfactual *de jure* policy outcomes in one, two, or more years after program initiation at time $t = 0$ until program completion at time T.[21] The higher the absolute gap, the less ownership the incumbent has

illustrative case studies, the lack of condition-specific compliance and implementation data does not allow us to exclude cases where the actual trajectory of *de jure* liberalization reforms falls below the counterfactual level due to imperfect compliance with a sector-specific set of conditions.

[21] We use the absolute value in order to account for some anomalous cases where the effects are found to be negative due to backlash and non-compliance with the specific policy condition.

over specific conditions or the program as a whole in any given year. This measure is informative because year-level ownership can vary throughout the duration of a program due to changes in domestic conditions that make a given program more or less viable from the perspective of the incumbent government.

Ownership may be counterfactually defined with respect to the scope and content of required reforms, the optimal mix of actions to achieve target outcomes, as well as the timing and sequencing of implementation. Accordingly, we identify two measures of ownership. Let I denote the set of treated units and K the "donor pool" of control units (with $|K|$ as its measure). Then, our *first* year-level measure is defined as follows:

$$\widehat{\alpha}_{it}^{j} = -\frac{\left| r_{it}^{j} - \sum_{k \in K} w_k^* r_{kt}^{j} \right|}{\sum_{k \in K} w_k^* r_{kt}^{j}}. \tag{1}$$

Here, $t = 1, \ldots, T$ refers to the successive years of the post-treatment period until the completion of the program at time T and r_{it}^{j} denotes the observed level of *de jure* structural reforms implemented by country $i \in I$ at time t in sector $j \in J$. Let X_i be an $x \times 1$ vector containing the values of the pre-intervention characteristics of the treated unit $i \in I$ and X_0 an $x \times |K|$ matrix containing the values of the same variables for the control units in the donor pool K. Then, the optimal $|K| \times 1$ synthetic control vector of weights W^* is such that $\|X_i - X_0 W\|$ is minimized subject to $0 \leq w_k \leq 1$ for all $k \in K$ and $\sum_{k \in K} w_k = 1$.

Our *second* measure is estimated with respect to the post-treatment goodness of fit between actual and counterfactual *de jure* policy outcomes throughout the duration of an (uninterrupted) IO arrangement. As such, it captures government ownership over the timing and sequencing of conditional structural reforms of an entire IO program. Even if cumulative policy outcomes do not differ as much between treated and synthetic units by the end of the program, borrowers may

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Alas, we are not able to systematically account for such cases due to the lack of condition-specific compliance data.

find their conditionality programs relatively too front-loaded in terms of adjustment measures. In formal terms, a target government i's ownership $\widehat{\rho}_i$ over the timing and sequencing of conditional reforms as stipulated in the LoI and MoU of an IO program at time $t = 0$ is directly proportional to minus the root mean square prediction error (RMSPE), i.e.,

$$\widehat{\rho}_i^j = -\frac{1}{T}\left(\sum_{t=1}^{T}\left(\widehat{\alpha}_{it}^j\right)^2\right)^{1/2} = -\frac{1}{T}\left(\sum_{t=1}^{T}\left(\frac{r_{it}^j - \sum_{k\in K} w_k^* r_{kt}^j}{\sum_{k\in K} w_k^* r_{kt}^j}\right)^2\right)^{1/2}. \quad (2)$$

As before, the maximum level of ownership is attained at value 0 when there is perfect overlap between the actual and the synthetic post-treatment *de jure* reform trajectories.

While our counterfactual-based identification strategy can apply to *program*-level measures of ownership over an IO program as a whole, our operationalization strategy estimates ownership over *sector*-specific policy conditions. In what follows, we operationalize our concept by using external-sector conditions as our treatment across the sample of IMF arrangements (1980-2014) and proceed to assess the reliability of our measure by deriving bootstrapped standard errors. Moreover, we probe its *face* validity against the case of the 1997-2003 IMF programs in Indonesia—using anecdotal evidence from primary and secondary sources—and its *construct* validity through simple bivariate regressions with known predictors and outcomes of ownership.[22]

Operationalizing ownership of IMF conditionality (1980-2014)

In line with our definition, ownership may vary across policy sectors. Due to data limitations we focus on just two sectors for which IMF conditions can be clearly mapped onto specific *de jure* policy outcomes, namely the *external* sector, which includes measures of trade and capital-account liberalization, and the *financial* sector, which comprises measures of financial institutional

[22] Also see our case-study analysis of the 1998 IMF program in Bosnia and Herzegovina discussed in Online Appendix B.

reform and market liberalization.[23] We then apply the SCM method on all uninterrupted IMF arrangements that comprised at least one external- or financial-sector condition, which allows us to compare actual *de jure* policy reforms to the level of *de jure* (economic or financial) structural reforms that would have materialized had the country not been subject to conditionality in either of those two sectors.

For our policy outcome variable r_{it}^{j}, we use the KOF index of *de jure* economic globalization (Dreher 2006; Gygli et al. 2019) (instead of its sub-component of trade liberalization) in order to match the relatively broad category of external-sector conditionality, which goes beyond trade liberalization and also includes capital-account liberalization. Its main advantage is that it is conceptually close to the types of sectoral reforms mandated by IMF programs that we examine here. An additional empirical advantage is that this measure is continuous and has extensive coverage.[24]

[23] In Online Appendix B, we carry out the same analysis with respect to financial-sector conditionality for robustness purposes. The financial sector comprises measures of financial institutional reform and market liberalization. Financial-sector conditions include, e.g., legal reforms, regulatory changes, supervisory policies on financial institutions, privatization of state-owned banks and insurance companies, as well as central-bank transparency and independence (Kentikelenis et al. 2016). In terms of financial institutions, these conditions typically require governments to strengthen central bank independence and to adopt stricter regulatory oversight over domestic banks (Kern et al. 2019).

[24] We use a common outcome measure across both sectors for two reasons. First, the scope of conditionality in each sector is broader than what would be covered by the respective subcomponents of the KOF index. For example, the KOF index of *de jure* trade openness would only partially capture the policy outcomes of external-sector conditions, which include both current- and capital-account liberalization outcomes. Second, using the broader economic globalization measure for both the external and financial sectors preserves our ability to pool results, which would not be possible under two separate outcome variables. Our results are similar when disaggregating

As for our treatment variable, we use a dummy variable indicating the presence of structural conditions in the external or financial sectors during the first year of the program. In line with the IMF classification, external-sector conditions comprise trade-related issues (e.g, lifting of tariffs and non-tariff barriers, quotas changes, etc.), exchange-rate regimes, capital-account liberalization, and foreign direct investment policies, among others (Kentikelenis et al. 2016). Financial-sector conditions include measures such as financial market liberalization, legal reforms, privatization of state-owned banks and insurance companies, regulatory oversight over domestic banks, as well as central-bank transparency and independence (Kentikelenis et al. 2016). We focus on *structural* conditions because these specify the exact legislative instruments that governments are expected to adopt, typically in order to achieve broader macroeconomic aims. By contrast, *quantitative* conditions specify policy goals that cannot be effectively legislated, which renders them less useful for measuring ownership as governments can simply choose policy instruments as they see fit.

To obtain well-identified treatment effects on treated compliers, we need to ensure that countries have not been under an IMF program during the *pre*-intervention period and that the program remains uninterrupted during the *post*-intervention period.[25] Therefore, we impose a gap of at least five years from the last active program in order to avoid picking up reform activity from a previous program, which would bias our results. The same gap is required for control units. Moreover, for lack of a systematic way to code program spells, we apply a uniform program duration T of four years. Furthermore, we restrict our sample to only include programs that were subject to temporary or permanent interruptions after initiation in order to eliminate negative deviations from

economic globalization into trade liberalization and financial liberalization.

[25] While we have verified that the assumption of full compliance holds for the purposes of our illustrative case studies, the lack of condition-specific compliance and implementation data does not allow us to exclude cases where the actual trajectory of *de jure* liberalization reforms falls below the counterfactual level due to imperfect compliance with a sector-specific set of conditions.

the counterfactual trend due to imperfect compliance.[26] As many countries have had consecutive spells of IMF programs, these exclusion rules, treatments, and sampling criteria reduce the number of treated cases available for analysis and allow us to estimate ownership measures for 32 IMF arrangements in total.[27]

By way of calibrating our SCM model, we include covariates reflecting fundamental macroeconomic characteristics, features of the political system, the international environment, and pre-treatment outcomes in order to make synthetic control units as similar as possible to treated units along this range of observable predictors. We control for the following covariates because they likely predict both IMF conditionality and *de jure* structural reforms. In terms of macroeconomic variables, we include (logged) GDP per capita reflecting the level of economic development of a country (The World Bank 2019), (logged) population as a proxy for country size, the level of economic growth, the stock of foreign-exchange reserves in months of imports, the current-account balance as a percentage of GDP, and debt service as a percentage of GNI (The World Bank 2019). These variables collectively help predict whether countries will require IMF financial assistance in the first place (Moser and Sturm 2011; Vreeland 2003). Our domestic political predictors in-

[26] In fact, we find that almost all negative deviations from the counterfactual in the full sample of 59 treated cases are due to program interruptions occurring in the four years after the initial treatment. Since we are interested in estimating the treatment effect on treated compliers (TETC), we drop these cases altogether. This leaves us with a small number of cases with negative deviations we cannot readily explain mainly due to the lack of data on program interruptions and condition-specific compliance. Exclusion of these anomalous cases does not substantively alter the diagnostic plots and, therefore, we keep them in the diagnostic sample.

[27] For the sake of robustness, we extend the time distance to prior treatment to either three or seven years. While this alters sample size, we obtain qualitatively similar results for the intersecting set of treated cases. Furthermore, we re-run our models using a time distance of three years to prior treatment. The results are again similar.

clude the Veto Player Index (Henisz 2002), which denotes the relative strength of actors that can undermine policy reforms, as well as the State Capacity Index (Hanson and Sigman 2021), which captures limits in the technical and/or bureaucratic capacity of governments to implement reforms. Finally, we control for international linkages, including a measure of fuel exports, military expenditures as a percentage of GDP (The World Bank 2019), the political globalization index (Dreher 2006; Gygli et al. 2019), and the fraction of time in which a country has been involved in any type of war during the previous five years (Gleditsch et al. 2002; Sarkees and Wayman 2010). As shown in Online Appendix A, our results are robust to the inclusion of additional control variables such as political instability and business cycle effects.[28]

We choose these variables because they likely predict both IMF conditionality and *de jure* liberalization reforms. In addition, we control for the total number of conditions as well as the scope of conditionality at the start of the program (Kentikelenis et al. 2016) in order to ensure that similar IMF programs will be matched, thereby isolating the effect of sector-specific conditionality on post-treatment outcomes. To control for unobserved factors and to enhance the fit, we also match on pre-treatment outcomes, both one year before treatment and five years before treatment. Finally, we match treated and synthetic control units with respect to the number of countries under IMF programs in the year before treatment. As this systemic variable varies only over time, its inclusion allows us to effectively control for unobserved time-period effects.[29] The time window for optimizing covariate balance comprises the five years before the onset of the IMF conditionality treatment.[30]

[28]Controlling for government turnover could help reduce estimation uncertainty in the post-treatment period given that deviations from ownership may be a result of political instability.

[29]Since we do not require synthetic control units to be observed throughout the same time period as treated units, this variable helps eliminate bias due to omitted global trends (Stubbs et al. 2018).

[30]For treated units before 1985 we use the years available from 1980 onwards, with the earliest possible treatment year being 1981.

An important aspect of the synthetic control method is how best to define the "donor pool", i.e., the universe of potential cases that the matching algorithm considers when constructing the synthetic control unit (Abadie and Gardeazabal 2003; Abadie et al. 2010, 2015). Our preferred donor pool includes observations of all onset years of IMF programs without structural conditionality in the treated sector (external or financial). This choice of donor pool balances the need to maximize available observations with the need to include similar-enough cases for which the hypothesized mechanism can plausibly hold. Our donor pool—consisting of 195 programs—is relatively large as we do not restrict the search to control units under IMF program in the same year. While this permissive donor pool sampling strategy deviates from the standard SCM approach, it helps us achieve the necessary enlargement of the donor pool so that it includes control units that are similar enough to treated ones in terms of requesting IMF assistance, thereby allowing for better matching in line with the so-called "possibility principle" (Mahoney and Goertz 2004).

By way of robustness, we consider several alternatives to our proposed donor pool specification. *One* is to include all IMF programs without sectoral conditionality starting in the same year as the treated unit as potential control units. This specification drastically reduces our donor pool size and thus leads to poor matching. A *second* alternative is to also include untreated observations not under an IMF program, while matching on their propensity score of being under an IMF program (Rosenbaum and Rubin 1983, 1985). To obtain the propensity score, we use a probit model of IMF program selection with standard variables from the literature (Moser and Sturm 2011; Vreeland 2007).[31] A *third* donor pool specification matches on the propensity score of receiving the treatment, using a selection model for IMF programs and adding the scope of conditionality and the total number of conditions as additional program-specific predictors. A *fourth* alternative ex-

[31]Predictors of IMF program participation include the number of countries under programs, temporary UN Security Council membership, UN General Assembly vote alignment with G7, civil liberties and political rights, executive elections, GDP per capita, GDP growth, reserves, debt service, British legal origin, region dummies, and year dummies.

cludes countries from the same region because their inclusion might introduce bias due to regional spillover effects. We find that our key results remain remarkably robust to different donor-pool specifications (see Online Appendix A).

A remaining task is to estimate confidence intervals around point estimates of ownership levels obtained from the SCM approach. We use a bootstrapping method, which entails the following three steps. First, in each bootstrap iteration, we perform the SCM approach using a (smaller) subsample of potential control cases obtained through resampling with replacement from the entire donor pool.[32] Second, we store all ownership estimates in a vector of length B, where B denotes the number of bootstrap iterations. In the interest of computational efficiency, we perform seven iterations throughout as additional iterations yield similar results but take more time to compute. Third, we aggregate the SCM point estimates from the various donor pools and compute the confidence interval based on the standard error of the empirical distribution of point estimates.Specifically, the 90th percentile upper confidence band is

$$\bar{r}^{j}_{kt} + 1.645\sqrt{\frac{1}{B}\sum_{b=1}^{B}\left(\hat{y}^{j}_{bt} - \bar{r}^{j}_{kt}\right)^2}, \tag{3}$$

where $\bar{y}_i$ is the mean policy outcome estimate.

We first present the results of our SCM-based estimation of ownership in the context of IMF programs with sector-specific conditionality. Table 1 below presents the goodness-of-fit ownership estimate (ρ_i), the pre-treatment RMSPE, and the ratio between post- and pre-treatments RMSPEs for all treated cases in our sample. Lower negative values of ownership indicate larger deviations

[32] In line with recent suggestions for inference under the SCM method (Li 2017), our bootstrapped standard errors rely on "resampling with replacement" from the entire donor pool such that for each iteration we keep the unique donor pool units. This induces more variability than the traditional leave-one-out cross-validation procedure. Hence, our standard errors are more conservative.

from a state of perfect ownership (0). The last column lists the Mahalanobis distance, a commonly used measure of covariate imbalance (Hainmueller 2012), where lower values indicate more covariate balance between treated and synthetic control units. To get a better sense of which factors have higher loadings in the matching process, we also track covariate weights for all treated units. We find that pre-treatment outcomes are particularly influential, while other domestic and systemic factors such as population, past conflict, the number of countries under an IMF program, and military expenditures are on average less important (see Table A.2 in Online Appendix A).

Across world regions, we find that high-income economies have the highest levels of ownership (-2.33), followed by East Asian/Pacific (-3.12), Latin American (-4.32), Sub-Saharan African (-5.70), and South Asian countries (-8.47). The region of Eastern Europe and Central Asia (-14.07) has the lowest average level of ownership in our sample.[33] Along the temporal dimension, we do not find evidence of enhanced ownership following the so-called "Streamlining Initiative", a rhetorical commitment by the IMF leadership in 2000-01 to cut back on structural conditions. To the contrary, it seems that ownership levels have declined on average from -4.6 in 1980-2000 to -7.3 in 2001-2014. At the same time, their variance has increased from 3.6 to 10.1 over these two periods, suggesting more heterogeneous country experiences with IMF programs. Comparing RMSPE ratios across the sample, Latin America has the lowest ownership levels ($\mu = 12.7$, with range from 2.8 to 32.6), whereas high-income economies have the highest ($\mu = 2.1$, with range from 1.1 to 3.5), with Sub-Saharan Africa, East Asia, and the Pacific region being in the middle. The programs with the lowest level of ownership seem to fall within the post-Soviet transition era and the Asian Financial Crisis. Moreover, our ownership estimates seem to increase on average following the rhetorical commitment by the IMF leadership to cut back on structural conditionality ("Streamlining Initiative") from 8.0 in 1980-2000 to 6.3 in 2001-2014.

[33]This does not seem to be driven by outliers given that two out of the five programs have low levels of ownership.

Country	Year	Sector	Duration	Size	Facility	Ownership (ρ_i)	Pre-RMSPE	RMSPE ratio	Covariate imbalance
BIH	1998	EXT	12	50	SBA	-0.052	0.006	8.878	2.789
CZE	1993	EXT	12	30	SBA	-0.056	0.018	3.087	0.992*
GHA	2009	EXT	36	81	ECF	-0.055	0.008	7.144	2.387*
GRC	2010	EXT	36	1088	SBA	-0.015	0.004	3.383	0.697*
HTI	2006	EXT	36	90	PRGF	-0.113	0.009	12.355	1.276
IDN	1997	EXT	36	490	SBA	-0.055	0.011	5.253	1.757
KHM	1999	EXT	36	67	ESAF	-0.121	0.046	2.607	1.489
LBR	2008	EXT	36	185	PRGF	-0.004	0.005	0.853	2.741
LTU	2000	EXT	15	43	SBA	-0.019	0.013	1.382	1.394
MDG	1996	EXT	36	90	ESAF	-0.025	0.004	5.546	0.375
MEX	1995	EXT	18	688	SBA	-0.023	0.009	2.659	1.821
MMR	1981	EXT	12	25	SBA	-0.009	0.010	0.959	2.523*
AFG	2006	FIN	36	50	PRGF	-0.101	0.011	8.965	1.736
ALB	1998	FIN	36	100	ESAF	-0.037	0.012	3.056	1.405
AZE	2001	FIN	36	50	PRGF	-0.063	0.001	49.547	3.120
BIH	1998	FIN	12	50	SBA	-0.039	0.004	9.398	3.106
BOL	1994	FIN	36	80	ESAF	-0.026	0.006	4.685	1.277
CRI	2009	FIN	15	300	SBA	-0.029	0.013	2.163	1.138*
CZE	1993	FIN	12	30	SBA	-0.055	0.018	3.005	0.957
DOM	2003	FIN	24	200	SBA	-0.106	0.059	1.806	1.523
GHA	2009	FIN	36	81	ECF	-0.062	0.014	4.538	2.442
GRC	2010	FIN	36	1088	SBA	-0.017	0.004	4.293	0.693
HND	1999	FIN	36	121	ESAF	-0.091	0.013	7.095	1.392
HTI	2011	FIN	48	25	ECF	-0.039	0.029	1.344	2.623
IDN	1997	FIN	36	490	SBA	-0.061	0.013	4.549	1.714
KEN	2011	FIN	36	180	ECF	-0.012	0.009	1.390	1.654
KHM	1999	FIN	36	67	ESAF	-0.147	0.038	3.822	3.004
KOS	2010	FIN	30	112	SBA	-0.042	0.049	0.856	0.959
LKA	2009	FIN	36	286	SBA	-0.037	0.017	2.232	0.780*
LTU	2000	FIN	15	43	SBA	-0.017	0.017	1.040	1.050
MDG	1996	FIN	36	90	ESAF	-0.024	0.004	5.358	0.579
MEX	1995	FIN	18	688	SBA	-0.023	0.015	1.501	1.779

Table 1: Estimates of ownership over external- and financial-sector IMF conditionality.

Program duration is measured in months. Size denotes the loan amount as percentage of the quota of the country. Facilities include Stand-By Arrangement (SBA), Enhanced Structural Adjustment Facility (ESAF), Extended Credit Facility (ECF), and Poverty Reduction and Growth Facility (PRGF). The root mean square prediction error (RMSPE) ratio is the post-RMSPE divided by the pre-RMSPE. Covariate imbalance is measured by the Mahalanobis distance. Cases marked with an asterisk are those that comprise negative deviations from their counterfactual trend in the post-treatment period and for which it is hard to ascertain whether they involved some type of implementation failure.

Validating measures of ownership

In Subsection , we proceed to discuss the well-studied case of IMF-induced external-sector structural reforms in Indonesia (1997-2003), which allows us to probe the face validity of our estimation results against the qualitative findings from the literature. We also provide some additional diagnostics for the SCM-based ownership estimates.[34]

Qualitative validation of ownership measures: Indonesia (1997-2003)

Indonesia turned to the Fund amid the Asian Financial Crisis of 1997-8. The country entered the crisis with relatively strong macroeconomic fundamentals: Since the early 1970s, the country had sustained annual real GDP growth rates averaging about 7 percent while maintaining inflation rates below 10 percent. Poverty rates had also declined from 60 to 11 percent (Cerra and Saxena 2000). This positive performance had been driven by prudent macroeconomic policies, rising investment and saving rates, as well as substantial economic liberalization efforts. These strong fundamentals, however, masked the underlying roots of the crisis. Trade and investment reforms aimed at opening the real economy were accompanied by measures in the financial sector to foster competition and growth in capital markets. Following a decade of reforms to remove controls on interest rates, entry, and credit allocation as well as to lower reserve requirements, Indonesia had come to possess one of the most liberal banking systems (Pincus and Ramli 2001). This encouraged ex-

[34] In this case we present our analysis of ownership with respect to reforms in the external sector. Online Appendix B presents the Bosnia and Herzegovina 1998 case with respect to reform outcomes in the financial sector and replicates our main findings. The plots look exactly the same for Indonesia using financial-sector conditions and for Bosnia and Herzegovina using external-sector conditions as the two treatments respectively. Therefore, our sector-specific measure of ownership does not appear to be sensitive to the choice of sector, capturing instead a more general feature of the contractual relationship between the IO and the target government.

cessive risk-taking among financial institutions in an environment of lax supervision. The absence of restraint on borrowing and lending practices led to the overexposure and overleveraging of Indonesian businesses, taking advantage of the spread between international and domestic interest rates.

After the Thai government was forced to float the baht in early July 1997, concerns of contagion started to spread, undermining confidence in the Indonesian economy as well. To counter what were then considered temporary speculative pressures, the Bank of Indonesia widened its intervention band and later floated the rupiah in August. As the currency depreciated by 30 percent over the following three months, the government sought assistance from the IMF primarily to boost confidence in its economy (International Monetary Fund 2000c). In a meeting with IMF Deputy Managing Director Stanley Fisher, the former governor of the Central Bank of Indonesia Soedradjad Djiwandono proposed a Precautionary Arrangement with the Fund instead of a full-fledged Stand-by Arrangement (SBA). That proposal was later pushed aside in follow-up discussions between the team under the IMF Director for the Asia–Pacific, Hubert Neiss, and the Indonesian delegation during the IMF Annual Meetings in 1997 (Djiwandono 2000, 53). Outlining the reasons for his proposal, Djiwandono argues: "[O]ur problem then was to restore market confidence, and this objective could be well served by the presence of the IMF under a precautionary arrangement. But a more important reason was that I was afraid I would not be able to persuade the President to agree to the stringent conditionality of a stand-by arrangement. A precautionary arrangement would bear much less stringent conditions even though it did not automatically include funds. Funding was not our major problem [. . .]" (Djiwandono 2000, 54).

Following self-imposed austerity measures and continuing depreciation of the rupiah, the Indonesian government once again approached the IMF in October 1997, requesting a three-year Stand-by Arrangement of SDR 7.3 billion, of which SDR 2.2 billion were to become immediately available upon Executive Board approval in November (Boughton 2012). Another SDR 2.9 billion would be disbursed during 1998 subject to full program implementation, while the remainder of

the funds were scheduled for 1999-2000 (Boughton 2012). The program focused on restructuring the financial sector and reducing barriers to trade. Trade-related structural measures targeted the elimination of import restrictions, monopolies, and large-scale projects with ties to associates of President Suharto. Two upfront measures included the dismantling of the government-owned Bulog trading monopoly in wheat, soybeans, and garlic, as well as the inclusion of chemicals, steel, and fishery products in the tariff reduction schedule (Schadler et al. 2009). Additional reforms entailed the reduction in export taxes and restrictions, and the liberalization of FDI (Schadler et al. 2009).

Despite a short-lived recovery of the rupiah, the IMF program did not put a halt to massive capital flight and thus triggered wider sociopolitical backlash. Djiwandono notes: "After some 'flip-flop' implementation of the IMF-supported program, the confidence problem shifted from being just an economic problem to being one of national leadership" (Djiwandono 2000, 55). Bank closures, which were unavoidable as the floating of the rupiah together with tightened monetary policy exposed the high-risk positions of banks and undermined the viability of the banking system (Basri 2018), led to a loss of confidence at a time when "monopoly practices and other inconsistencies reappeared in the implementation of the program for restructuring the real sector" (Djiwandono 2000, 55). President Suharto had perfected a system of "crony capitalism," whereby power and wealth was concentrated in monopolies controlled by a coterie of individuals well-connected to him, thus generating controversy and mistrust in the government. As a result, not only was the devaluation of the rupiah difficult to rein in, but "the economic crisis was [also] spiraling into a 'total crisis'" (Djiwandono 2000, 56).

The first program review was delayed due to non-implementation of performance criteria on base money, net reserves, short-term debt, fiscal deficits, and price increases, which were eventually waived by the Fund. To bring program implementation back on track and restore investor confidence, then-IMF Managing Director Michel Camdessus decided to personally intervene in January 1998 and persuade President Suharto to carry out the reform package (Boughton 2012). The

infamous picture of Suharto hunching over to sign a document dictated by the IMF and Camdessus standing next to him with folded arms belied the initial expectation that Suharto would assume full responsibility himself. As he almost immediately informed his cabinet that a full implementation would not be necessary, the agreement never reached the IMF Executive Board for approval (Boughton 2012).

The revised SBA included 20 prior actions and 114 structural conditions to be implemented over the lifetime of the program (Boughton 2012). Regarding the external sector, Indonesia would have to reduce tariffs and lift restrictions on foreign investment in wholesale trade. The government would also have to issue instructions to local governors to eliminate all local export taxes (Kentikelenis et al. 2016). To control the budget deficit, the Fund devised a sharp increase in fuel and electricity prices as prior action. The announcement of this unpopular measure by Suharto himself intensified riots that had already started in response to skyrocketing food prices, resulting in his resignation and replacement by Bacharuddin Jusuf Habibie (Boughton 2012).

The program went completely off track in August 1998, following the expansion of access to IMF credit by one billion SDR (to a total of SDR 8.3 billion) during the delayed second review (Boughton 2012). The Indonesian government requested the SBA be canceled and replaced by an Enhanced Funding Facility (EFF), covering the undrawn amount in the SBA (SDR 4.7 billion) (International Monetary Fund 2009a). The second program was tied to a strengthened reform agenda, primarily targeting inflation in order to restore food security. Corporate restructuring, an effective bankruptcy system, deregulation, privatization, and improved governance accompanied banking-sector reforms as another pillar of the program (International Monetary Fund 2000c). Trade conditionality was largely carried over from the previous SBA. As a further structural performance criterion (SPC), the IMF requested the reduction of export taxes on logs and sawn timber to 20 percent alongside the elimination of Bulog's last monopoly in the rice market (Schadler et al. 2009). Trade-related measures were extended to privatization, the enactment of new legislation in the areas of competition and investment, as well as new mechanisms to administer food and

fuel prices. The program was eventually replaced by another EFF in January 2000. The third program included additional trade commitments, establishing a three-tier tariff structure, removing all exemptions from import tariffs, non-tariff barriers, and export restrictions, as well as allowing transitional import tariffs on rice and sugar (Schadler et al. 2009).

The fact that, according to historical records, key actors in the Indonesian government disagreed with many of the policy measures imposed by the Fund would imply low levels of government ownership. First and foremost, the Indonesian government initially questioned the need for a full-fledged SBA, arguing that countries under similar circumstances had obtained precautionary credit lines in the past. Furthermore, Indonesian authorities were uncomfortable with agreeing to the state provision of full guarantees for bank deposits and bank liabilities (Djiwandono 2000, 63), while at the same time the Fund addressed neither the problem of unsustainable corporate debts denominated in foreign currency nor the question of social safety nets. These two issues were tackled by the IMF only upon the insistence of the Indonesian team and following criticism by international pundits all over the world (Djiwandono 2000, 64).

When Suharto took charge of the negotiations with Camdessus, disagreements with the IMF continued. In late January 1998, Suharto proposed a currency board as a way to stabilize the rupiah. IMF officials declined to take the currency board proposal seriously; upon realizing that it was a real option for Suharto, they concluded it would quickly lead to disaster (Boughton 2012, 531). After Suharto signed the revised SBA agreement, he immediately proclaimed his lack of commitment to and ownership of the program. The infamous picture of Suharto hunching over to sign a document dictated by the IMF and Camdessus standing next to him with folded arms belied the initial expectation that Suharto would assume full responsibility himself. As he almost immediately informed his cabinet that a full implementation would not be necessary, the agreement never reached the IMF Executive Board for approval (Boughton 2012). At the IMF Executive Board meeting of May 1998, several developing country directors argued that the Fund was injecting itself much too deeply into micromanaging Indonesia's structural policies, while directors from

key creditor countries were highly skeptical of the government's willingness or ability to reform and carry out the program (Boughton 2012, 537). All in all, it is safe to conclude that the case of IMF-induced structural reforms in Indonesia was characterized by low levels of ownership.

Our own analysis of the Indonesian case using the SCM approach confirms these findings. Figure 2 below shows that Indonesia—prompted by a bundle of structural conditions—was effective at reforming its external sector, as reflected in its increasing *de jure* economic liberalization score. In terms of ownership, however, the graph suggests that it was rather low throughout the duration of the program since there is a large discrepancy between the outcomes of the treated and the synthetic control units. Counterfactually speaking, Indonesia would not have undertaken these structural reforms on a similar scale or at the same pace had it not been for the hard conditionality of the IMF programs. It is only toward the end of the IMF's involvement in the country that the actual and counterfactual reform trajectories start to converge but again at a relatively lower level. This would also suggest that there is little evidence of long-term policy learning and capacity-building taking place.

While the IMF offered generous programs to Indonesia, it also imposed a number of conditions that were met with criticism by Indonesian authorities. Former central bank governor Djiwandono rejected the program because he thought that conditionality would not be necessary to restore market confidence, while Suharto reneged on his commitment to implement IMF-backed reforms, especially seeing how Camdessus forced him to sign the agreement in a humiliating posture. In sum, our ownership measure in this case is characterized by high levels of face validity, reflecting the fact that the Indonesian government substantially deviated from fully owning its IMF-sponsored structural adjustment program. The lack of ownership helps explain the high number of implementation waivers and the renegotiation of successive programs during a short period of time.

We also check which covariates weigh most heavily in the synthetic counterfactual. In line with our aggregate statistics presented in the main text, Table 2 shows pre-treatment outcomes to be the most influential matching variables, followed by conflict history, scope of conditionality,

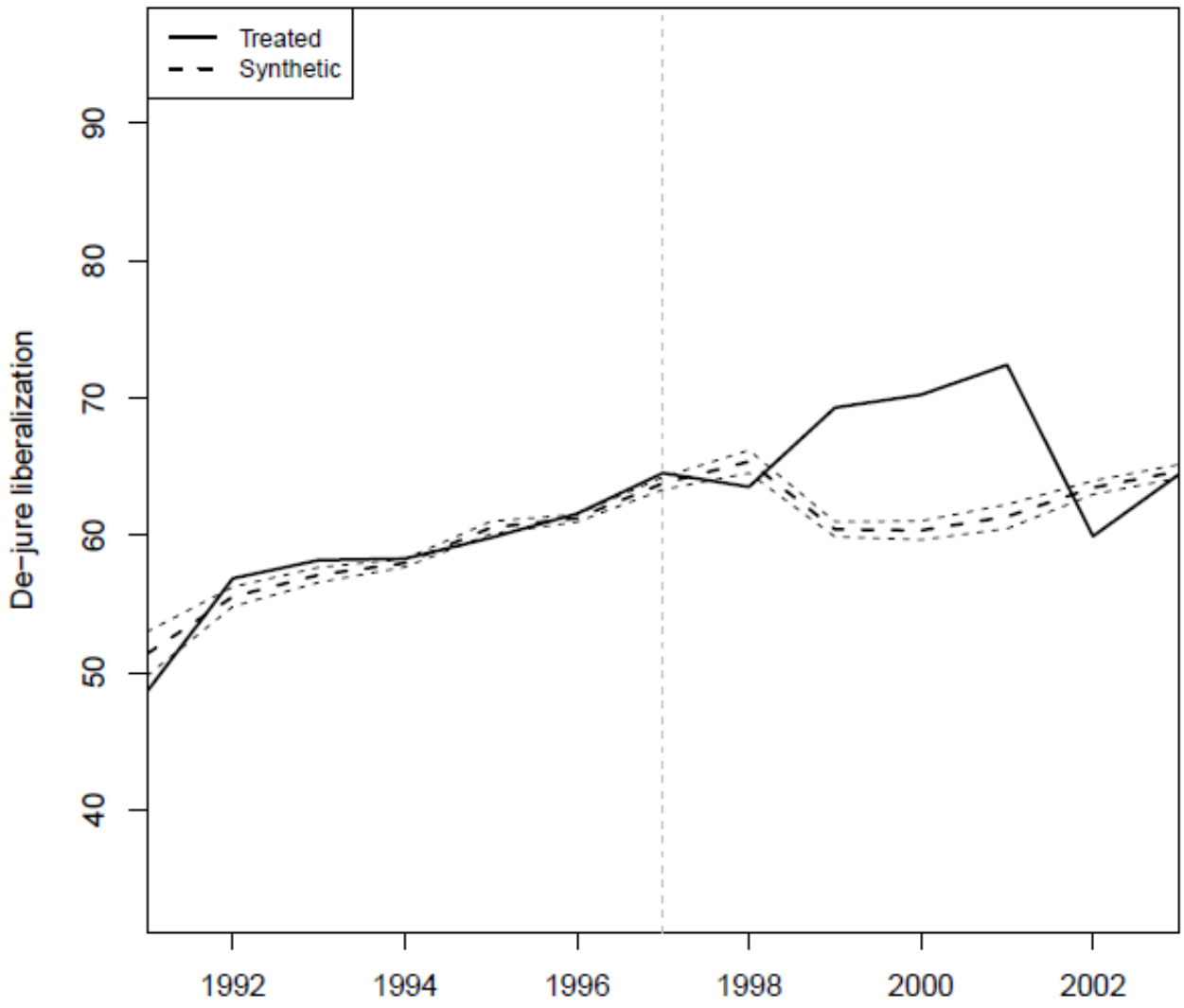

Figure 2: *De jure* economic globalization (as measured by the KOF index) in actual and synthetic Indonesia before and after the initial IMF intervention (1997).

Covariates	Weight
GDP per capita	0.009
Population	0.021
State capacity	0.006
Political globalization	0.000
GDP growth	0.000
Reserves	0.005
Current account	0.010
Debt service	0.047
Fuel exports	0.016
Veto player index	0.011
Past conflict	0.071
Military expenditure	0.001
Total conditions	0.000
Scope of conditionality	0.058
Pre-treatment outcome (t-1)	0.400
Pre-treatment outcome (t-5)	0.328
Trade openness (t-1)	0.013
Countries under programs (t-1)	0.002

Table 2: Synthetic control variables and associated covariate weights for Indonesia 1997.

and debt service.

Table 3 allows us to assess the covariate balance for all variables included in the matching model for the treated and the synthetic control units. It is reassuring that covariate balance is particularly good for those variables that are weighted most heavily in the synthetic control unit. The case also illustrates how lack of ownership leads to more frequent use of implementation waivers and renegotiation of programs over a short time period.

Finally, we use the Indonesian case to establish that lack of ownership lessens reform effort. To that end, we add the *de facto* economic liberalization index to the gap plot in which lack of ownership is indicated by a divergence in the *de jure* liberalization paths between Indonesia and its counterfactual (Figure 3). In the first year of the IMF program, when ownership was high, *de facto* reform increased sharply, but began to fall steeply as the government lost ownership of the program. With the disengagement of the Fund, the country ended up at the same level of *de facto*

Covariate	Treated	Synthetic	Sample
GDP per capita	6.926	8.062	6.980
Population	19.064	17.817	15.804
State capacity	0.065	0.333	-0.303
Political globalization	73.045	74.608	48.898
GDP growth	7.830	3.091	2.639
Reserves	3.271	3.021	3.313
Current account	-2.464	-2.052	-6.862
Debt service	9.369	7.751	5.090
Fuel exports	29.630	27.996	15.378
Veto player index	0.000	0.241	0.180
Past conflict	3.333	3.017	1.214
Military expenditure	1.277	4.047	5.193
Total conditions	9.000	18.660	19.267
Scope of conditionality	5.000	4.840	4.923
Pre-treatment outcome (t-1)	61.566	61.238	39.921
Pre-treatment outcome (t-5)	56.852	55.519	39.199
Trade openness (t-1)	52.265	50.914	69.868
Countries under programs (t-1)	75.000	58.079	56.631

Table 3: Covariate values for actual Indonesia 1997, synthetic Indonesia 1997, and sample means.

reform as in the pre-treatment period—a typical case of backsliding.

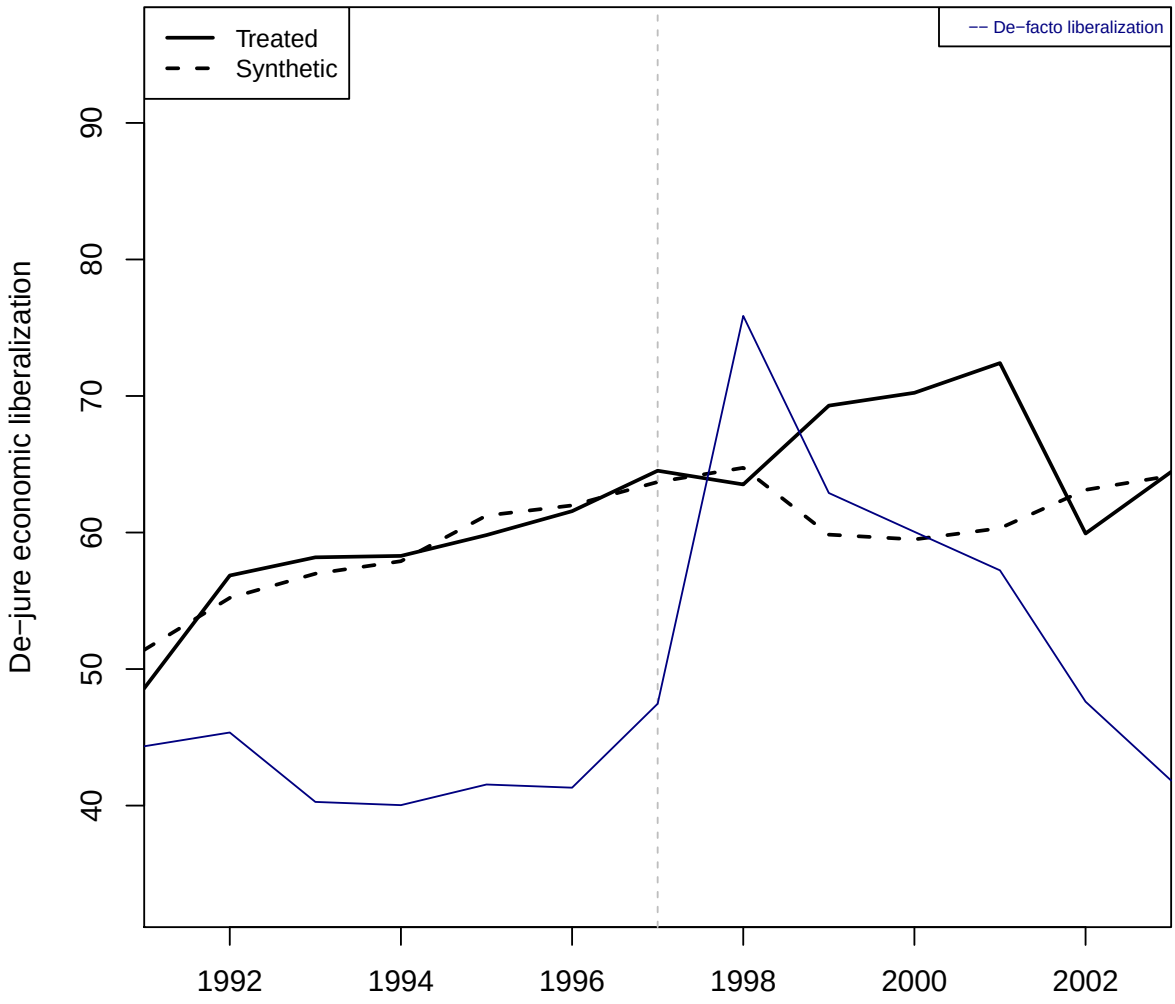

Figure 3: Year-level deviation from ownership and *de facto* reform in Indonesia (1997-2003).

Validation against known determinants, proxies, and effects of ownership

Using our sample of treated units, we proceed to conduct systematic tests of aggregate-level relationships between our ownership measure and known determinants, proxies, and effects of ownership to demonstrate the criterion and construct validity of our measure in the context of IMF conditionality programs. To that end, we first examine the determinants of ownership, focusing specifically on the relationship between the design of IMF conditionality and program ownership. We then turn to the impact of ownership on program compliance and *de facto* reform outcomes. Our graphical evidence below pools together treated units from both sectors, while Online Appendix A presents disaggregated results by sector as well as additional validation plots.

Figure 4 below shows the relationship between the total number of structural conditions across the external and financial sectors (as outlined in IMF program LoIs and MoUs) and our goodness-of-fit ownership measure ρ_i (Equation 2). To the extent that the extent of conditionality is an indicator of how much the IMF staff trusts the government to undertake reforms without external inducement, one would expect a larger deviation from a situation of perfect ownership in cases where the IMF staff prescribe more conditions (Erbas 2003; Konstantinidis and Karagiannis 2020). This expected pattern is borne out by the data. While the goodness-of-fit measure ρ_i in Figure 4 captures the level of government ownership over the timing and sequencing of reforms throughout the duration of an IMF program, Online Appendix A illustrates the same relationship using the year-level ownership measure $\widehat{\alpha}_{it}$ (Equation TET).

Demonstrating the criterion validity of our measure, we further examine the correlation of our goodness-of-fit ownership measure with other plausibly related proxies. First, we consider the total number of prior actions in an IMF program as an indirect proxy for the lack of ownership in the sense that when the IMF staff make a negative subjective assessment of the borrower's willingness to reform, they are wont to ask governments to implement a higher number of reforms before lending can begin (Wei and Zhang 2010). Figure 5 below validates this expectation as indicated by the negative slope line of the fitted regression line between the total number of prior actions and

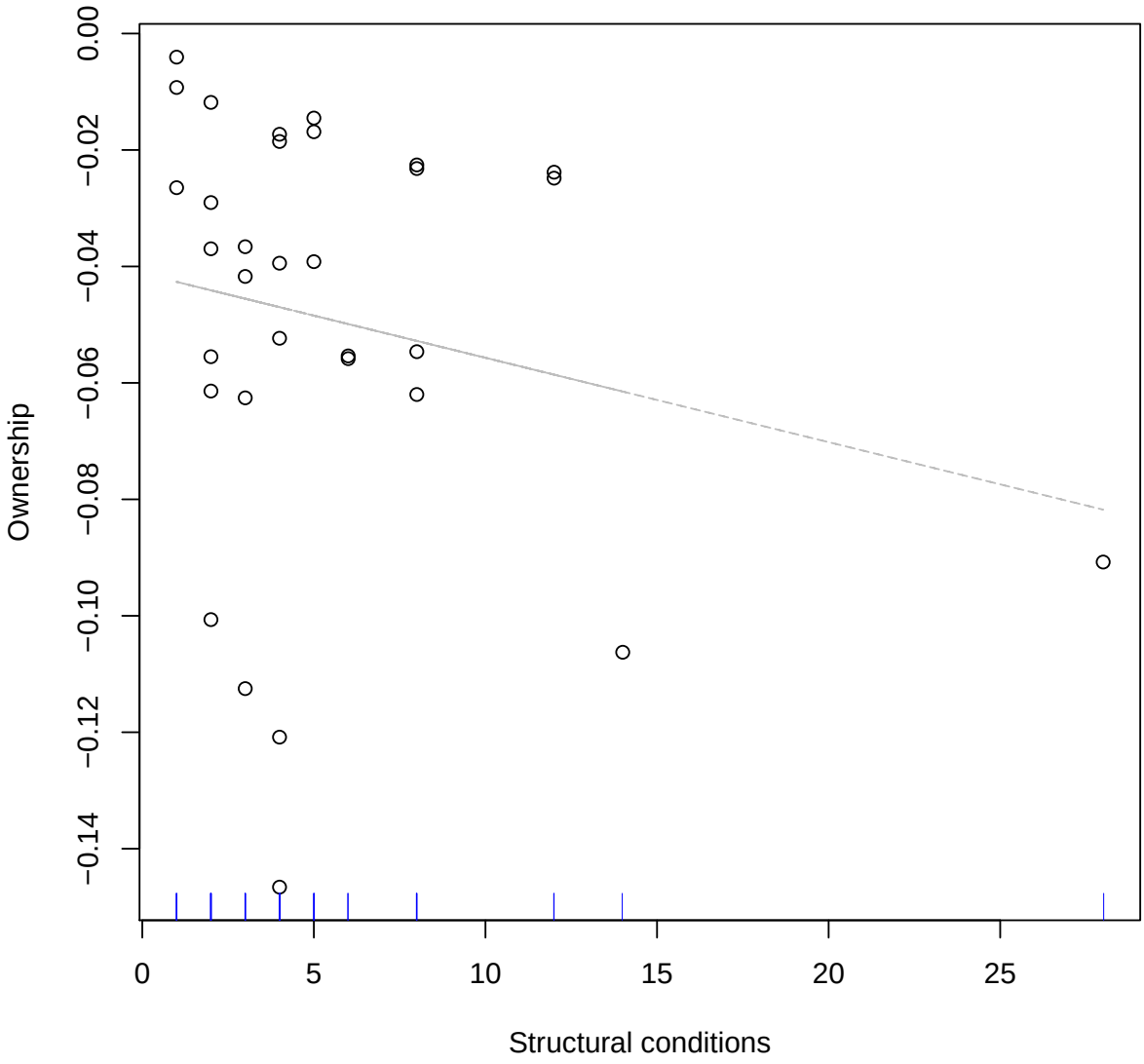

Figure 4: Total number of external- and financial-sector structural conditions and government ownership as estimated by the goodness-of-fit measure ρ_i.

the goodness-of-fit ownership measure ρ_i.

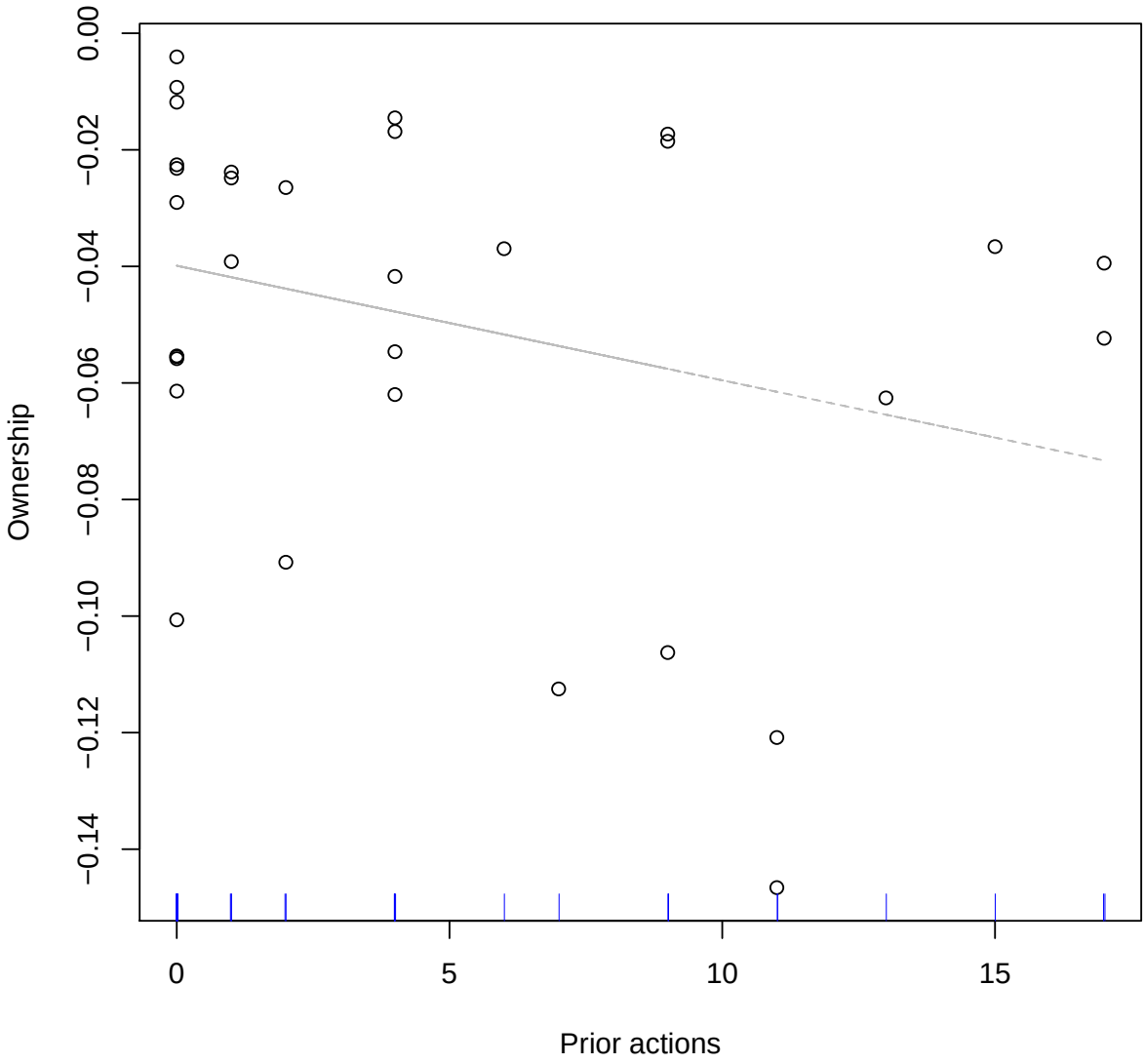

Figure 5: Total number of prior actions per IMF arrangement and government ownership as estimated by the goodness-of-fit measure ρ_i.

A second set of plausible predictors of ownership are external to the program. In particular, we confirm that ownership levels are higher for more market-oriented governments in line with the expectation that such governments need less external inducement to commit to market-liberalizing

reforms (Beazer and Woo 2016). In addition, we find that ownership levels are higher when many foreign banks are present in the borrowing country. Our interpretation of this result is that the more exposed Western banks are to the borrower's economy, the more lenient Western governments will be in respect to the design of IMF conditionality (Copelovitch 2010). Further analysis suggests that exposure of U.S. banks to the local economy has an even stronger association with ownership. All related figures are presented in Online Appendix A.

In terms of the effects of ownership, Figure 6 below illustrates the relationship between ownership over the timing and sequencing of reforms and the post-program participation rate defined as the proportion of years in which a country has been under an IMF program during the five-year period after the completion of its original program. While borrowers generally show a tendency for such "IMF recidivism" (Vreeland 2003), we find it to be particularly high among governments with low levels of ownership. This is likely so because such governments are not as effective in terms of implementing *de facto* reforms and addressing the structural problems of their economy, which makes them more prone to reapply for IMF assistance. Our bivariate correlation implies a strong effect given that the estimated likelihood of returning to the Fund during the five-year period after the end of the initial program is as low as 18% under perfect ownership and as high as 40% for the lowest level of ownership in our sample.[35]

As a final test of construct validity, we examine the relationship between ownership and *de facto* reform outcomes. Our hypothesis here is that if countries truly "own" a program, they will not only adopt reforms on paper but also really implement them on the ground. The KOF indices allow us to distinguish between *de jure* and *de facto* trade and financial globalization reforms. Figure 7 below shows that the lower the level of ownership, the more will *de facto* reforms lag behind *de jure* reforms (i.e., the double difference between *de facto* and *de jure* globalization indices becomes negative), thereby confirming our expectations. We interpret this as evidence of

[35]In Online Appendix A, we discuss some further results with respect to yet another implementation outcome, namely the share of waived conditions.

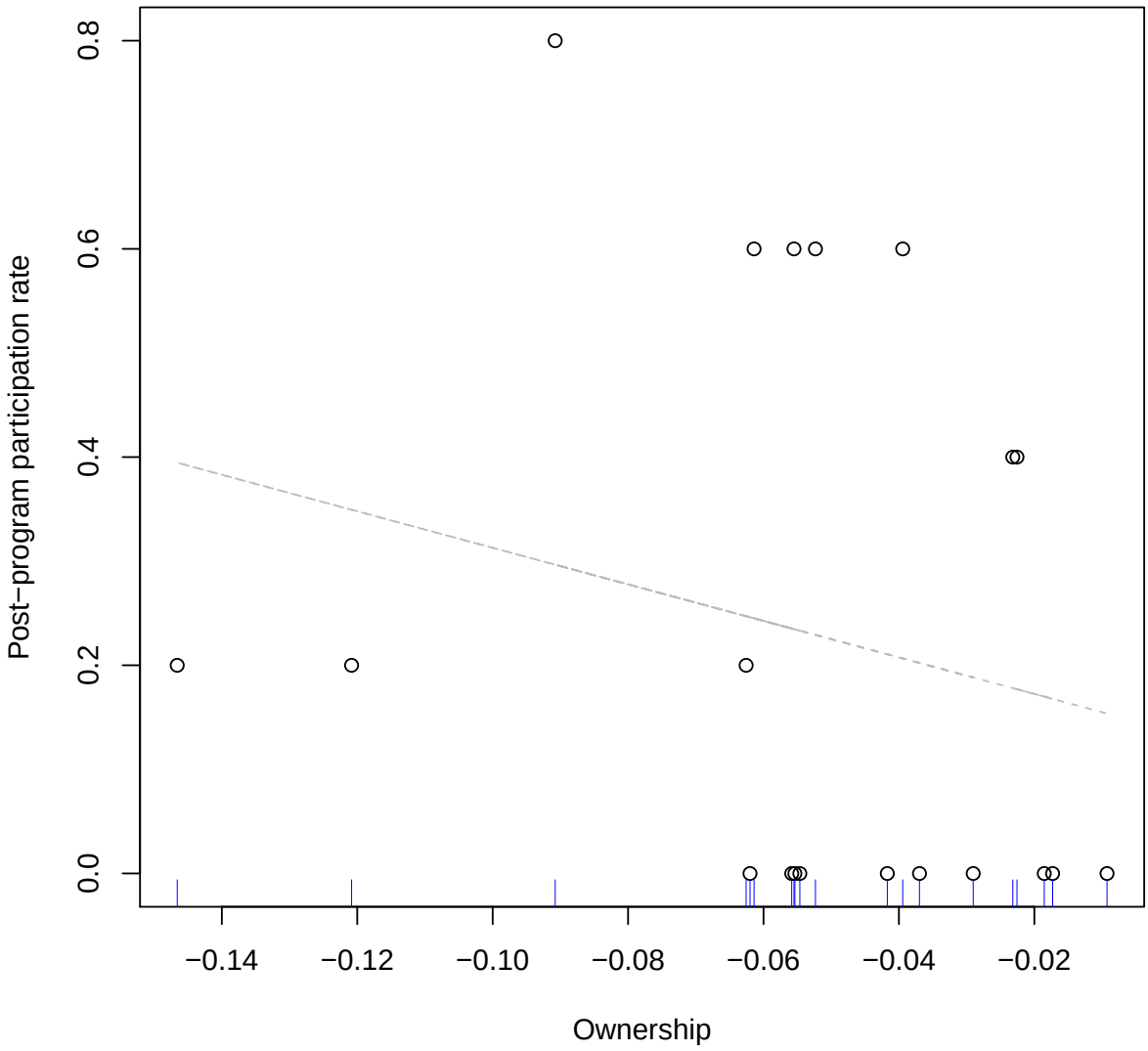

Figure 6: Government ownership as estimated by the goodness-of-fit measure ρ_i and IMF recidivism as measured by the post-program participation rate.

backsliding on reforms, whereby governments with low levels of program ownership commit to the letter but not the spirit of a reform package.

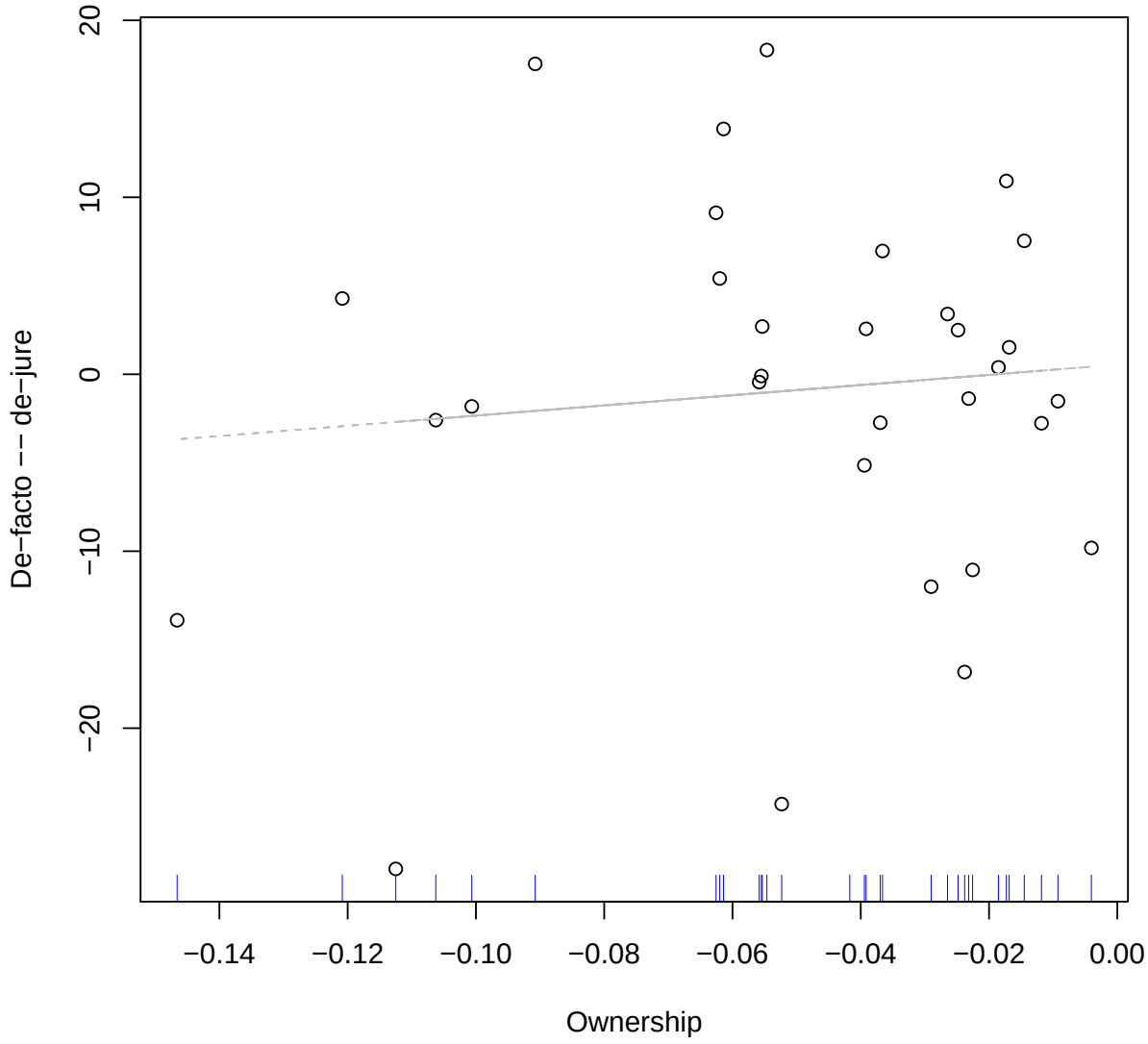

Figure 7: Government ownership as estimated by the goodness-of-fit measure ρ_i and the double difference between *de facto* and *de jure* trade and financial globalization respectively.

The variable on the x-axis is the goodness-of-fit measure of government ownership (ρ_i) and the one on the y-axis is the double difference between *de facto* and *de jure* economic globalization r (as measured by the sector-specific KOF indices), i.e., $(r^{df}_{t=3} - r^{df}_{t=0}) - (r^{dj}_{t=3} - r^{dj}_{t=0})$.

Conclusions

Although IMF conditionality policies have been investigated to a considerable extent and with unambiguous success, we are still limited in our capacity to explain different responses in otherwise similar target countries. On the demand side of conditionality, a standard argument in the literature is that contracting governments often resort to external assistance to deflect the political costs of painful liberalization reforms and to restore international credibility. On the supply side, the design of conditionality programs is dictated by either the foreign policy interests of the major donor countries (especially the U.S.) or the policy agenda of international bureaucrats or even the domestic politics of the borrowing country. We argue that in order to provide a more complete picture of the political economy of IMF lending, one needs to fully identify and operationalize the concept of program ownership as a mediating link between program design and policy implementation.

In this paper, we propose what is to our knowledge the first reliable, replicable, valid, versatile, robust, and systematic measures of government ownership across a wide range of cases (Adcock and Collier 2001). We systematically operationalize our measures through the SCM method across a restricted sample of uninterrupted IMF arrangements (1980-2014) and assess its reliability by deriving bootstrapped standard errors. While our measurement approach is purely data-driven and non-axiomatic, it is nonetheless consistent with a wide range of assumptions and theories of ownership. To address concerns about potential endogeneity with respect to crisis conditions, we match treated units with countries that are also under IMF programs but without the sector-specific conditionality, whilst controlling for a host of pre-treatment variables capturing borrowers' underlying macroeconomic fundamentals, the severity of the crisis, the features of their political systems, and their bargaining power. We probe the face validity of our ownership measure against the case of IMF involvement in Indonesia (1997-2003) and demonstrate how it offers a sensible interpretation of the country's actual reform trajectory. Our measures pass several (criterion and construct) validity tests against known predictors, indicators, and effects of ownership. Finally, they are versatile

with respect to both program- and year-specific levels of ownership over conditional structural reforms, qualitatively robust to any meaningful alterations in the setup of our operationalization strategy, and replicable across a wide range of sectors and IO programs.

This is a timely research project given the continued debate on structural conditions and country ownership in the context of IMF programs. As a recent IEO review of IMF conditionality highlights, "the IMF has significantly revamped its policies on structural conditionality over the past decade ... to ease strains on the authorities' implementation capacity, reduce stigma, and enhance program ownership," noting at the same time that "interviews with executive directors indicated that authorities' implementation capacity and/or country ownership has remained strained" (Independent Evaluation Office of the International Monetary Fund 2018, 20). In fact, the number of structural conditions has risen in 2011–6, prompting IMF executive directors to "underscore the need to adhere to the macro-criticality criterion" (Independent Evaluation Office of the International Monetary Fund 2018, 21). They further emphasize that "prior actions [are] not a substitute for country ownership and should ... be applied with great care" (Independent Evaluation Office of the International Monetary Fund 2018, 8). In line with these concerns, we have found a negative relationship between conditionality and country ownership with potential adverse effects on compliance (Reinsberg et al. 2021). Our findings, therefore, validate the IEO's recommendations by demonstrating the need to "examine the factors affecting compliance and ownership, analyzing trends in the depth and criticality of structural measures, and assessing the broader impact of structural conditionality on policies and performance" (Independent Evaluation Office of the International Monetary Fund 2018, 21).

Beyond the IMF context, our work is also relevant to the normative concerns of other IOs. Along these lines, for example, the Paris Declaration on Aid Effectiveness (Organisation for Economic Co-operation and Development 2005) established that aid policy should be reformed with the goal (among others) of increasing country ownership of reforms. Moreover, according to a 2005 WB report, "the experience [of the 1990s] showed that government discretion cannot be

bypassed" (The World Bank 2005, 14). Finally, our research also contributes to ongoing policy debates on debt suspension (e.g., the G20's 2020 Debt Service Suspension Initiative), fiscal expansion, public investments, and structural reforms (e.g., the EU's Recovery and Resilience Facility) in response to the COVID-19 crisis. We seek to advance these debates by proposing ways of operationalizing the direct and indirect links between the various components of the political economy of IO-sponsored financial assistance programs. Despite the weakening of the liberal economic order, IOs will continue playing an important role in the global financial system as most governments seem unable to single-handedly grapple with the economic uncertainty and turbulence of our times. And yet, the design of the contractual relationship between official creditors and national borrowers remains as contentious as ever.

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Online Appendix A

We start by summarizing the descriptive statistics of our variables and then proceed to present additional diagnostic plots referred to in the main text as well as robustness tests on our ownership estimates using the synthetic control method (SCM).

Variables

Variable name	Observations	Mean	Sd	Min	Max
External-sector conditions	7,141	0.16	0.73	0	11
Financial-sector conditions	7,141	0.46	1.65	0	28
De jure economic liberalization	5,977	48.64	20.34	9.12	97.39
De facto economic liberalization	6,292	53.06	17.83	4.39	98.63
GDP per capita	6,228	8.06	1.63	4.24	11.97
Population	6,033	15.34	2.19	8.99	21.05
State Capacity Index	5,137	-0.08	1.14	-3.51	2.86
Political globalization	6,709	49.35	26.61	1	98.42
GDP growth	6,283	3.51	6.58	-64.05	149.97
Reserves	4,778	3.99	4.03	0.002	79.24
Current-account balance	4,792	-3.18	13.92	-240.52	291.32
Debt service	3,588	4.88	5.79	0	135.38
Fuel exports	4,298	16.10	27.25	0	100
Veto Player Index	6,114	0.25	0.22	0	0.73
Prior war	6,755	0.98	1.80	0	5
Military expenditure	3,836	2.71	6.16	0	158
IMF propensity score	2,420	0.51	0.23	0.004	1
External-sector propensity score	2,312	0.08	0.07	0	0.48
Financial-sector propensity score	2,312	0.17	0.10	0	0.50
Countries under programs	7,560	56.97	10.01	41	75
UN Security Council membership	5,973	0.06	0.23	0	1
Civil liberties	6,061	3.62	1.93	1	7
Political rights	6,061	3.61	2.22	1	7
Executive election	5,351	0.10	0.30	0	1
British legal origin	7,560	0.24	0.42	0	1

Table A.1: Variable names and descriptive statistics.

Covariate	Mean	Std. Dev.	Min	Max
GDP per capita	0.027	0.054	0.000	0.267
Population	0.033	0.057	0.000	0.272
State capacity	0.028	0.061	0.000	0.314
Political globalization	0.032	0.039	0.000	0.152
GDP growth	0.018	0.030	0.000	0.144
Reserves	0.028	0.032	0.000	0.133
Current account	0.022	0.033	0.000	0.118
Debt service	0.020	0.027	0.000	0.100
Fuel exports	0.022	0.035	0.000	0.163
Veto player index	0.023	0.041	0.000	0.182
Past conflict	0.035	0.055	0.000	0.281
Military expenditure	0.044	0.062	0.000	0.261
Total conditions	0.018	0.036	0.000	0.171
Scope of conditionality	0.025	0.029	0.000	0.111
Pre-treatment outcome (t-1)	0.324	0.186	0.000	0.534
Pre-treatment outcome (t-5)	0.235	0.164	0.003	0.445
De facto outcome (t-1)	0.032	0.054	0.000	0.231
Countries under IMF programs (t-1)	0.034	0.052	0.000	0.247

Table A.2: Descriptive statistics of covariate weights for all matched units.

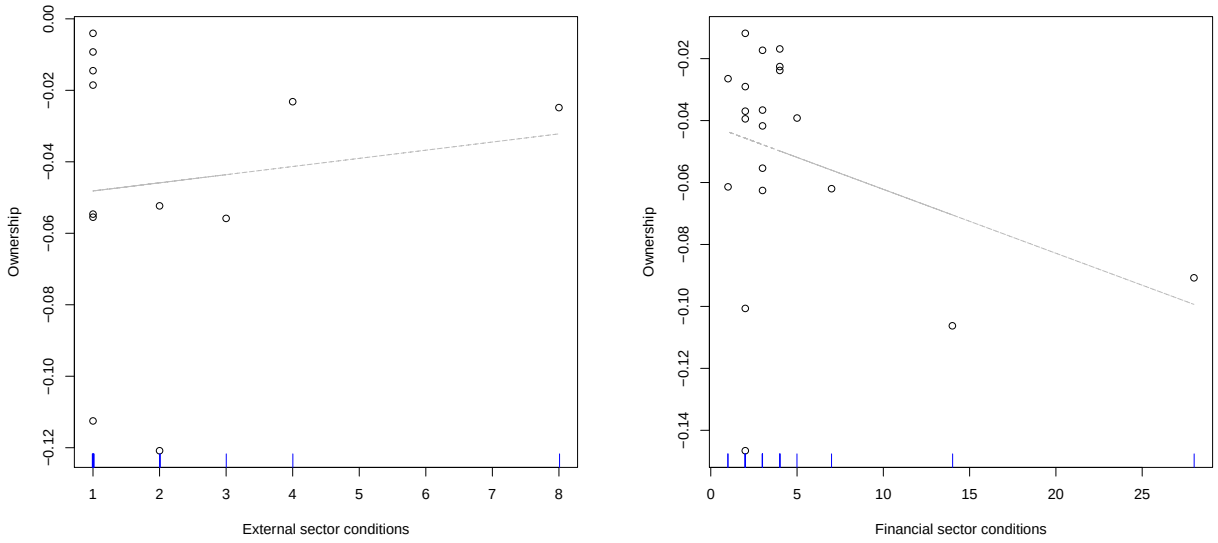
The *de facto* outcome in the table refers to trade openness for external-sector conditions and FDI inflows for financial-sector treatments.

Diagnostic plots

Unless stated otherwise, all diagnostic plots in the appendix are based on the sample of SCM-based ownership estimates pooled across both sectors (external and financial). We exclude treated cases of programs that were interrupted at any time in the first four years, given that all such interruptions cause a negative deviation of the actual reform trajectory from the counterfactual one.

First, we use the goodness-of-fit ownership measure ρ_i and disaggregate the number of structural conditions by sector. Figure A.1 shows that the association between the number of structural conditions and program ownership is weakly positive for external-sector conditions (Subfigure A.1a) and negative for financial-sector conditions (Subfigure A.1b). Next, we use the year-level ownership measure $\widehat{\alpha}_{it}$ (taken from Equation 1 in the main text). The points in the plot correspond to different annual review cycles for each program in our sample. The negative slope of the best-fitting line in Figure A.2 confirms that the number of structural conditions is negatively correlated

with the annual ownership measure.



(a) External-sector conditions.

(b) Financial-sector conditions.

Figure A.1: Total number of structural conditions and government ownership (as estimated by the goodness-of-fit measure ρ_i) by sector (external and financial).

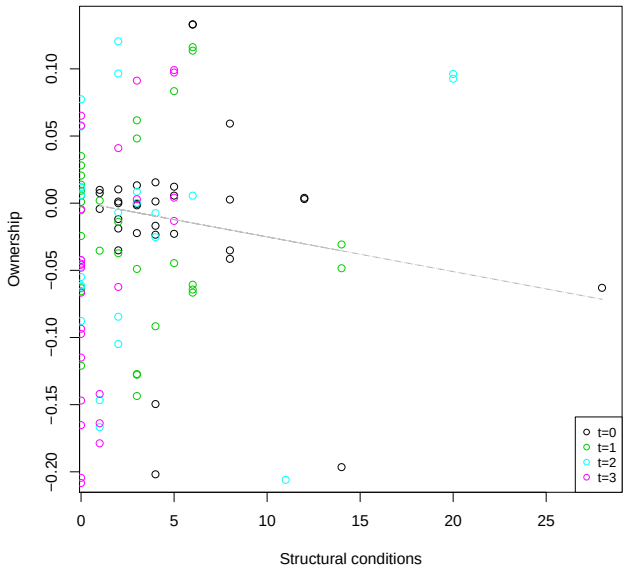


Figure A.2: Total number of structural conditions by sector and government ownership as estimated by the year-level measure α_{it}.

In the figures below, we test for the relationships between our ownership estimates and known proxies and predictors of ownership that are external to program design, as discussed in the main text. Figure A.3 replicates the same result reported in the main in text in terms of the negative association between the number of prior actions and government ownership using the year-level measure α_{it}. Figure A.4 depicts a weak positive association between the Economic Freedom Index (Gwartney et al. 2020) and government ownership as estimated by the goodness-of-fit measure ρ_i. Figure A.5 depicts a positive association between the number of foreign banks in the country (Claessens and Van Horen 2014) and the level of government ownership (as estimated by the goodness-of-fit measure ρ_i).All these relationships are consistent with our theoretical expectations.

We now turn our attention to the effects of ownership or the lack thereof using alternative outcome measures. The main text presents evidence of heightened IMF recidivism in circumstances of low ownership using the goodness-of-fit measure ρ_i. Figure A.6 shows that the same relationship holds when using the year-level measure of government ownership α_{it}.[36]

We also present results with respect to yet another implementation outcome, namely the share of waived conditions. A waiver amounts to a decision by the Executive Board to remove the requirement to implement a certain condition typically in order to ensure the successful completion of the program review cycle and the disbursement of the next loan tranche. Figure A.7 indicates a positive association between government ownership (as estimated by the goodness-of-fit measure ρ_i) and the total incidence of condition waivers by program. This result could be intuited by the fact that the IMF staff will only grant waivers to those countries that have otherwise shown to be trustworthy and effective in the reform strategy, but we cannot provide a definite rationalization as due to lack of condition-level implementation data we do not know whether these waivers are final or simply move some conditions to future review cycles.

[36]Dropping cases with negative deviations does not qualitatively change the plot.

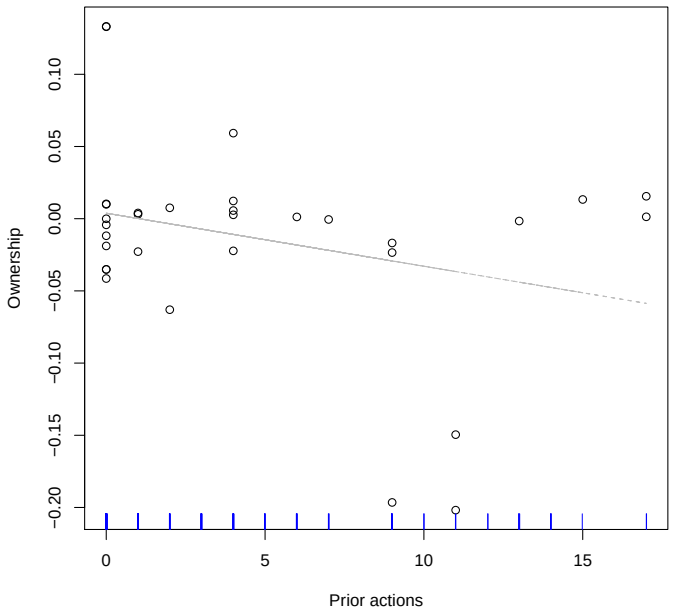


Figure A.3: Total number of prior actions and government ownership as estimated by the year-level measure α_{it}.

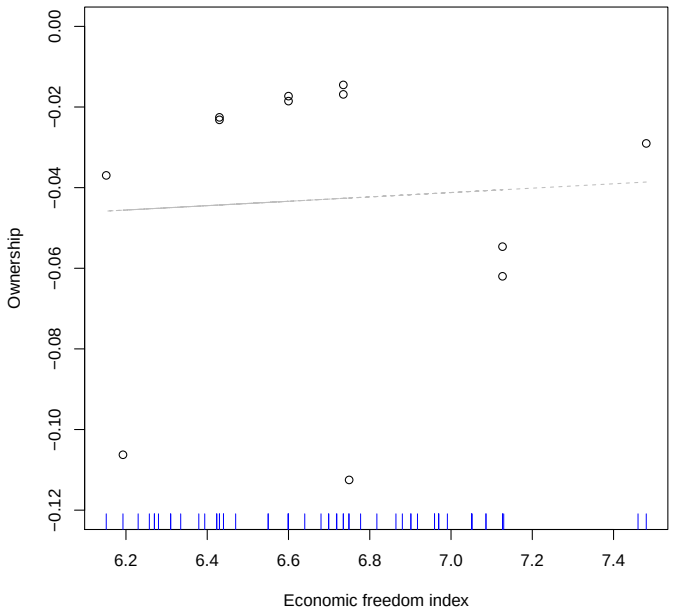


Figure A.4: Economic freedom (Fraser Economic Freedom index) and government ownership as estimated by the goodness-of-fit measure ρ_i.

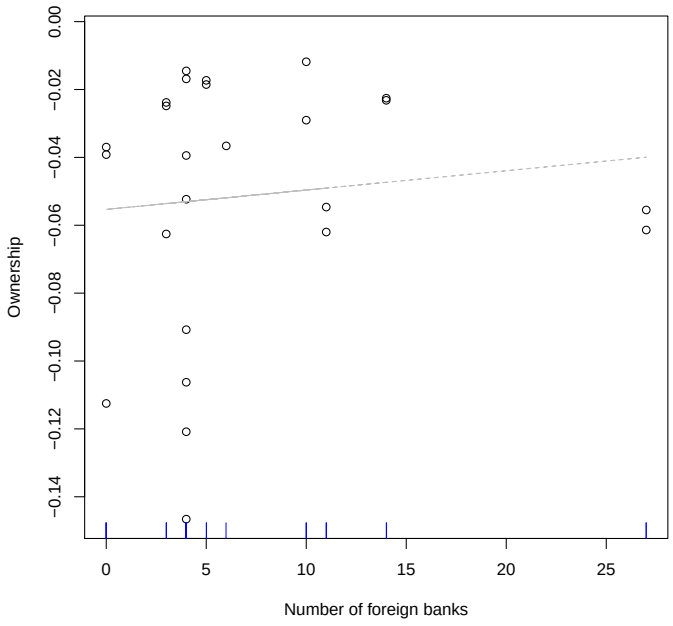


Figure A.5: Number of foreign banks in the borrower's economy and government ownership as estimated by the goodness-of-fit measure ρ_i.

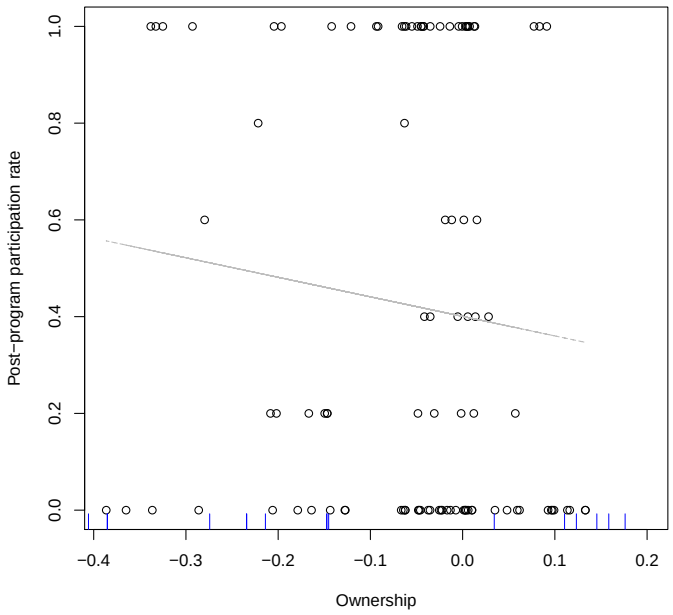


Figure A.6: Government ownership as estimated by the year-level measure α_{it} and post-program participation rates.

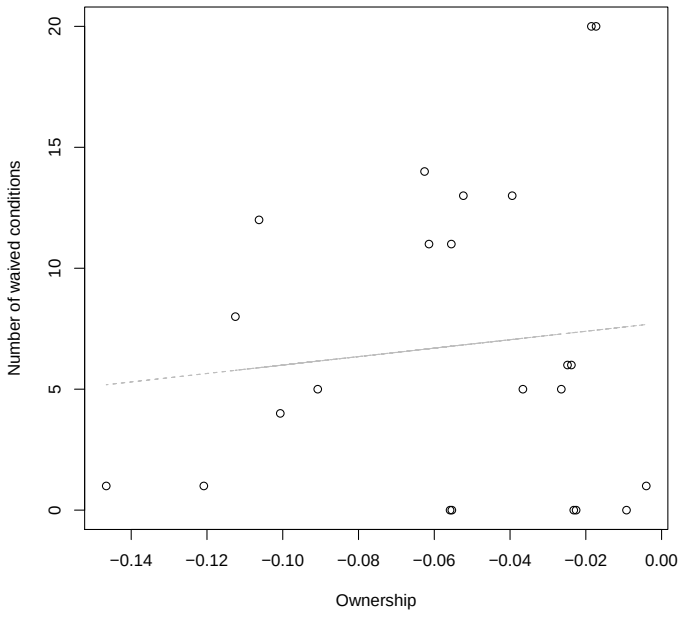


Figure A.7: Government ownership as estimated by the goodness-of-fit measure ρ_i and total number of waived conditions by IMF arrangement.

Robustness of the SCM approach

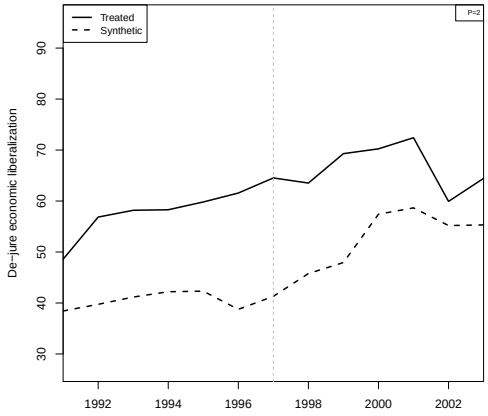
Donor pool configurations: As reported in the main paper, we probe the robustness of our findings to different configurations of the donor pool. While results for all treated units are available upon request, we present robustness results for the focal case of IMF involvement in Indonesia (1997-2003) and its external-sector conditionality treatment (see Figure A.8). In Subfigure A.8a, the donor pool for the SCM algorithm comprises all observations of IMF programs without the respective sector-specific conditionality, starting in the same year as the treated unit. In Subfigure A.8b, the donor pool includes all untreated observations—including countries under IMF programs and countries not under IMF programs—and the SCM algorithm additionally matches units along the propensity score of being under an IMF program. In Subfigure A.8c, the donor pool is the one in the main analysis—i.e., any country starting an IMF program in any year—and the SCM algorithm additionally matches units on the propensity score of receiving the respective sector-specific conditionality. This helps mitigate concerns about the non-random assignment of the treatment. In Subfigure A.8d, the donor pool excludes all potential control units from the same region as the treated unit. One might be concerned that our results are driven by spatial diffusion and learning effects (Simmons and Elkins 2004; Elkins et al. 2006). This would be more likely if the algorithm

picked countries from the donor pool that are contiguous to the treated country or otherwise culturally similar. We, therefore, exclude countries from the same region in this robustness test, with no substantial effects on our results. Finally, in Subfigure A.8e, inclusion in the donor pool requires the absence of a prior IMF program for an extended period of seven years.

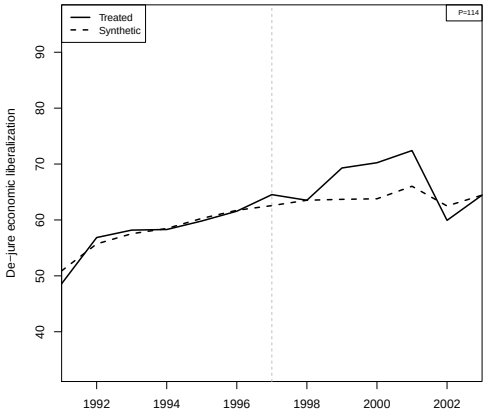
Regional spillover effects: Another way to see that spillover effects are unlikely to be a problem is to inspect the weighted composition of synthetic units when we do not *a priori* eliminate countries from the same region. Synthetic units with non-zero weight for Indonesia include Latvia (37.1%), Uzbekistan (34.2%), Hungary (18.8%), and Poland (9.9%). These control units are all outside the Asia-Pacific region. Synthetic units with non-zero weight for Bosnia and Herzegovina include Morocco (28.8%), Poland (23.0%), Gambia (20.9%), Uzbekistan (15.9%), Hungary (13.0%), St. Kitts and Nevis (12.0%), and North Macedonia (0.1%). In light of these findings, we consider it unlikely that our results are driven by spatial diffusion and learning effects.

Placebo treatments: Following best practice in the implementation of the SCM approach, we verify that our results only hold for actual treatments. To that end, we "pretend" that countries received the respective structural conditionality ten years later when in fact they did not receive it. We then repeat the analysis, expecting no discernible treatment effects for the placebo treatments. We generally find no meaningful treatment effects. In the case of Indonesia 1997, the confidence bands of the outcome variable for the synthetic unit envelop the one of the treated unit throughout the program's duration, which demonstrates that there are no significant treatment effects (Figure A.8f). It take the country up to five years after the treatment for the policy outcome to differ significantly from that of the synthetic control unit. This lends further support to the validity of our approach.

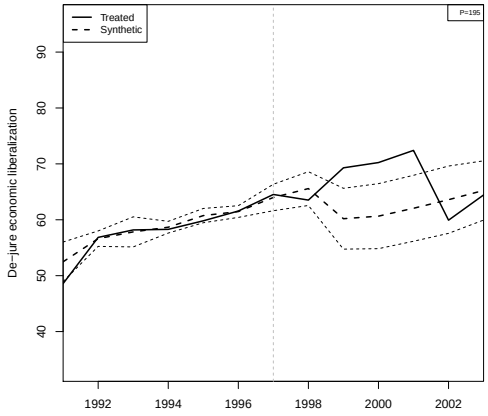
Different sets of control variables: We also find that our results are robust to the inclusion of additional control variables in the matching algorithm (see Figure A.9). In addition to the battery of controls already included, we include two measures of political (in)stability drawn from the *Archigos* dataset (Goemans et al. 2009). First, we construct a measure for the average time share of leader change in the five-year run-up period to the onset of the treatment. This measure is continuous, with higher values indicating more politically unstable systems. Second, we condition on the number of years that a government has already been in office in the year prior to obtaining the treatment. This second variable allows us to further differentiate among political systems without a change in leadership in the immediate past. Subfigure A.9a shows that these changes have no impact on the results for both treated units under scrutiny. The results also do not change



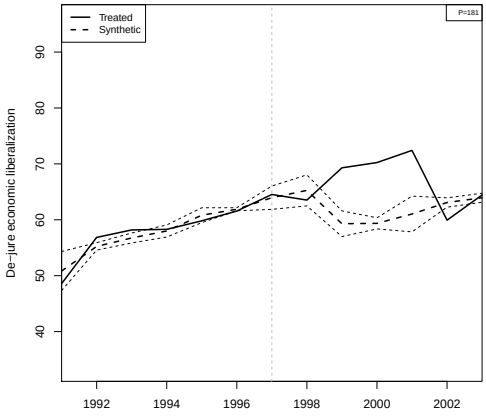
(a) Including only IMF programs starting in the same year.



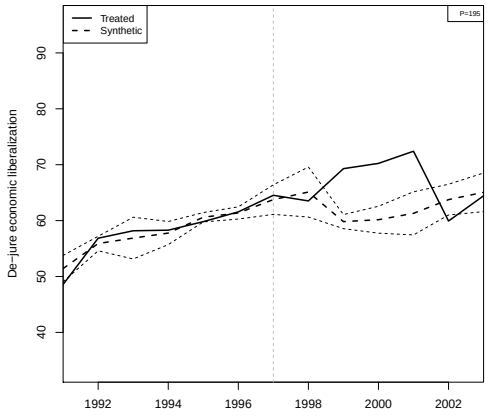
(b) Matching on the propensity score for IMF program onset.



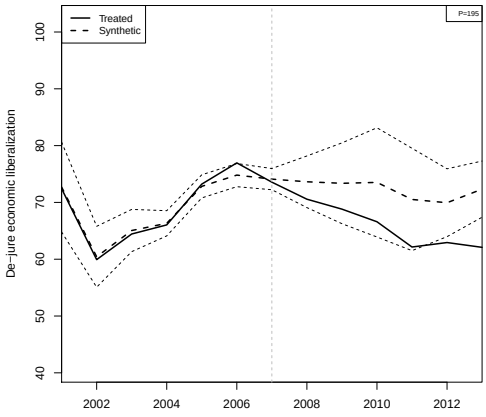
(c) Matching on the propensity score for receiving the treatment.



(d) Excluding all control units from the same region as the treated unit.



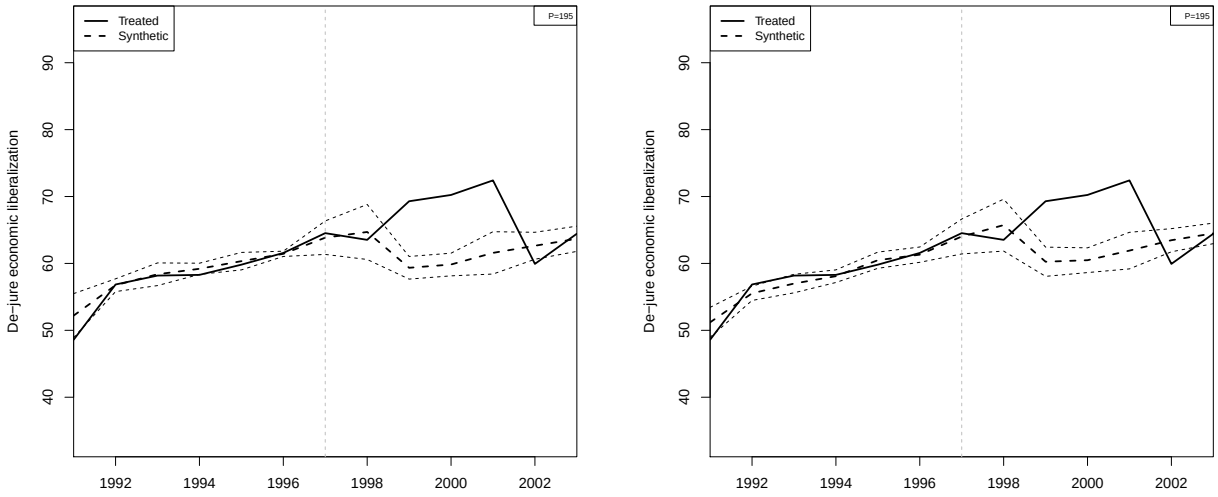
(e) Using a seven-year cutoff rule for previous treatments.



(f) Placebo test "pretending" that the treatment kicked in ten years later.

Figure A.8: Paths of *de jure* economic globalization in actual and synthetic Indonesia (1997) for different donor-pool specifications.

when the measure of political instability covers the post-treatment period. Specifically, we measure the relative frequency of leader change in the period $(t-10,...,t-1,t,...,t+5)$. One might also be concerned that we fail to adequately capture business-cycle effects in our baseline specification. A government's willingness to reform might be higher when the economy is in a boom compared to when it is in a recession (Alesina et al. 2020). Hence, we further include a de-trended economic growth variable, measured as the deviation from the within-country mean of economic growth for each country. This variable effectively distinguishes boom from bust years. Subfigure A.9b confirms that our results are not driven by business-cycle effects as they are qualitative indistinguishable from the previous plots.



(a) Political (in)stability controls.

(b) Business-cycle controls.

Figure A.9: Paths of *de jure* economic globalization in actual and synthetic Indonesia (1997) for additional controls.

Online Appendix B

In this section, we discuss the case of Bosnia and Herzegovina (1998-2003) as another face validity test of our SCM-based ownership measure against qualitative evidence from the 1998 IMF-sponsored financial-sector reform program in that country.

We subsequently present aggregate-level relationships to underscore the criterion and construct validity of our financial-sector ownership measure. In light of our sampling criteria, we identify

ownership measures for 35 IMF arrangements using the *financial* sector conditionality treatment. In sum, we show that our SCM-based estimation approach is not limited to a single sector but has cross-sector applicability.

Ownership over IMF-induced financial-sector reforms in Bosnia and Herzegovina (1998-2003)

Bosnia and Herzegovina approached the IMF in the aftermath of the 1992-1995 war, which had caused damage to its infrastructure and productive capacity, leaving the country in a difficult economic and social situation (International Monetary Fund 1998b). The war that had erupted following Bosnian independence from Yugoslavia ended with the signing of the Dayton Accords, creating a new constitution and a decentralized government structure. The central government required consensus among the three national groups for most decision-making. Its economic competences were limited to monetary and exchange-rate policy, external borrowing and debt service, foreign trade, and customs and tariffs. The country's two regional entities, the Federation and the Republika Srpska, in turn retained competence over functions not explicitly assigned to the central government.

In the post-war reconstruction effort, the Bosnian economy recovered quickly, supported by external financial assistance. However, despite attaining real GDP growth rates of 50 percent in 1996 and 30 percent in 1997, GDP remained at half of the pre-war level. In this context, the government sought external advice to bolster its economic development. Mirsad Kurtovic, then-governor of the Bank of Bosnia and Herzegovina, said: "The goal is to transfer the biggest part of state-owned properties into private ownership. Further development of the financial and banking sector and, above all, to recover trust in domestic commercial banks, as well as the greater presence of the foreign-based bank branches in the country, are necessary preconditions. Another important structural change that is being implemented is the introduction of market-oriented criteria in economic judgment and decision-making. We seek to develop economic environment primarily based on the market as main regulator of economic activities with very limited state interventionism. Finally, we are also facing the very important task of building a sustainable system of social services" (International Monetary Fund 2000c, 68).

In May 1998, the IMF approved a 12-month Stand-by Arrangement (SBA) for Bosnia and Herzegovina over SDR 60.6 million, of which SDR 24.2 million would become immediately available. The supporting economic program was based on four pillars: a fixed exchange rate under a currency-board arrangement, budgets balancing reconstruction and social needs while avoiding

any domestic borrowing, external financial assistance to boost economic recovery, and structural reforms to help the transition to a market economy (International Monetary Fund 1998a). The main areas of structural reform of the program were in banking, exchange liberalization, economic statistics, privatization, and custom tariffs. With regard to the financial sector, the program included several conditions—most of them as prior actions—such as, for instance, the agreement on a timetable for issuing new currency, the appointment of a liquidator for the National Bank of Bosnia and Herzegovina (NBBH), and the separation of the payments bureau from the Serb State Bank. Conditions also required the maintenance of foreign exchange cover at 100 percent for the domestic liabilities of the Central Bank (Kentikelenis et al. 2016).

Delays in the implementation of structural reforms resulted mainly from the complexity of the decision-making process between the central and regional governments (International Monetary Fund 1999). During the course of the program, the IMF approved two augmentations, increasing the total amount of IMF access from SDR 60.6 million to SDR 94.42 million. The initial end date was extended three times from April 2000 to May 2001 (International Monetary Fund 1999, 2000a,b).

Qualitative evidence available from official quotes suggests that the government had high levels of ownership in the program. First and foremost, the program came about as a result of an invitation by the Bosnian government to the international financial institutions in an effort to rebuild the economy. As governor Kurtovic stated, "[t]he role of the World Bank and the International Monetary Fund, headed by President Wolfensohn and Director Camdessus, in the reconstruction and in the stabilization of the economic situation were of decisive importance in reestablishment of macroeconomic stability in Bosnia" (International Monetary Fund 2000a, 68). He continued to argue that "[o]ur commitment to conduct all necessary economic reforms is very resolute and indisputable" (Kurtovic 1998). To a considerable extent, the central government invited the IMF to strengthen its hand against internal opposition from regional authorities against its ambitious economic reform program. As governor Kurtovic explained, "[d]espite the problems, we remain determined to take all necessary measures in line with the spirit of the Dayton Agreement and to insist on its speedy and integral implementation. In the economic domain we shall focus on the following tasks: speedy implementation of economic reforms, with the assistance of the World Bank and IMF, securing additional funding [...] as the implementation of the Stand-by Arrangement has proceeded with great success" (Kurtovic 1998).

Our own analysis confirms that Bosnia and Herzegovina in 1998 is a case of high ownership of reform in line with the qualitative evidence. Figure B.1 suggests that Bosnia and Herzegovina would have undertaken financial reforms even without IMF conditionality throughout the actual

duration of the SBA program. In the immediate post-treatment period, the counterfactual policies closely match the actual policies. After the program, the observed level of economic liberalization seems to be higher than in the counterfactual scenario. One interpretation for this finding is that the IMF program strengthened the pro-reform coalition in the country, flanked by institution-building measures like central-bank strengthening that locked in the reform progress.

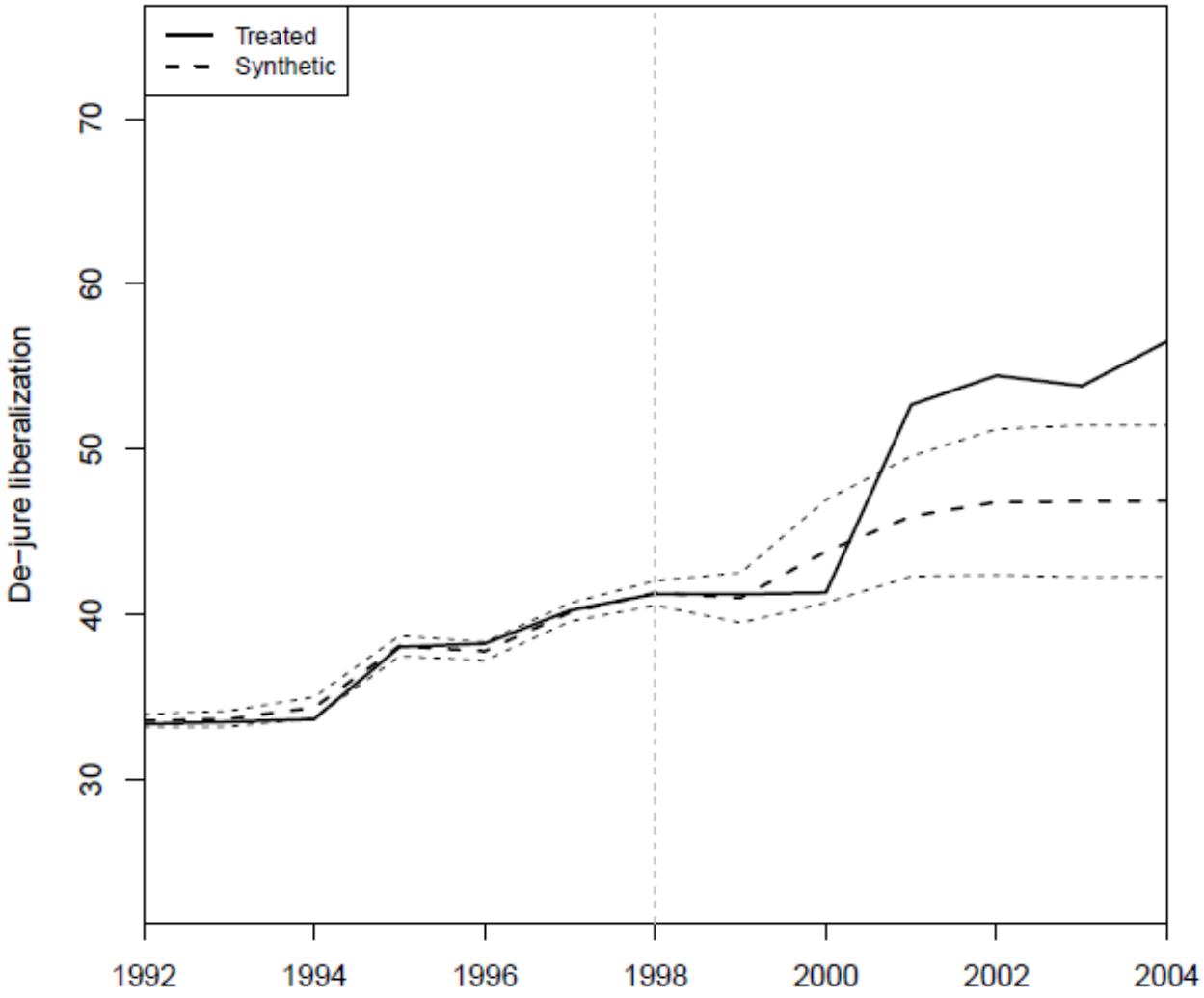


Figure B.1: *De jure* economic globalization (as measured by the KOF index) in actual and synthetic Bosnia and Herzegovina before and after the IMF intervention (1998).