Do International Bureaucrats Matter? Theory and Evidence from the International Monetary Fund*

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Abstract

Scholars of international cooperation argue that member states delegate decision making authority to international organizations (IOs) as a commitment to non-interference. We argue this logic conflates the *objects* of delegation with the *mechanisms* by which delegation is made credible. Member states delegate decision making authority to international bureaucrats. This delegation is made credible by institutional features of the relevant IO. Where delegation is credible, individual bureaucrats will exercise an independent impact on policy making. We test the credibility of delegation within the International Monetary Fund (IMF). We develop a formal model of bureaucratic appointments, characterize their equilibrium impact on market valuations of sovereign debt, and provide causal estimates of this impact employing event study methods. Our analytical results provide a direct test of the credibility of delegation as well as a transparent theoretical interpretation of the causal estimand. We find strong and consistent support for the credibility of delegation within the IMF.

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1 Introduction

In late 2011 IMF policy towards Eurozone countries took a sharp turn. From the earliest days of its involvement in the crisis the IMF played the role of external financier as well as impartial monitor, uniquely capable to reassuring markets about the sustainability of European countries' debt. Via new exceptional access provisions the IMF lent unprecedented amounts to EU member states. Greece received financial assistance amounting to twenty-two times its quota, the largest IMF package in history relative to the size of its economy.[1] Ireland similarly received sixteen times its quota in loans, suggesting a soft money policy intended to ease financial market anxieties and buy time for policy makers to design long-term solutions (Stevis and Talley, 2013).

Over the following years the IMF reversed course. By the time negotiators met to hash out a bailout for Cyprus in 2013 the IMF took a hard line, agreeing to a loan of just six times quota and forcing unpopular losses on bank depositors. Similarly, the IMF threatened to cut off Greek financing unless it could negotiate a debt write-down with other EU member states, a previously unthinkable position. Rumors circulated that IMF staff had considered debt restructuring, but been overruled by EU member states. In explaining the IMF's course reversal, one journalist noted: "a two syllable reason: Reza" (Jones, 2013).

"Reza" refers to Reza Moghadam, an Iranian-born British national who took over as director of the Fund's European Department in November 2011. With a reputation as a tough negotiator, Moghadam set out to change perceptions that the Fund was showing favoritism to European economies in deference to powerful members of its executive board. Unsurprisingly, Moghadam's replacement in late 2014 by Poul Thomsen, the architect of original Greek bailout, led to widespread speculation about the direction of IMF policy towards Europe as well as the state of the European economy (Talley, 2014).

This type of speculation about bureaucratic appointments, common among journalists and commentators, is not limited to the Eurozone crisis. The Fund encompasses five Area Departments and changes in leadership are regularly reported in the news media, with reports scouring the employment history of new appointees—or in some cases their scholarly work—for clues about their predispositions and likely impact on regional policy

[1] Member quotas are funds payed into the IMF when a country joins and which it may subsequently borrow in times of crisis. While it is increasingly common for the IMF to lent amounts larger than quota, the Greek package was the largest in history relative to the size of its economy.

making. Especially during times of crisis, Area Department heads come to be well-known figures in their own right. Hubert Neiss, who headed the Asia Pacific Department during the Asian Financial Crisis, "became one of the most recognized faces in Jakarta...hordes of reporters trailed his visits to different government buildings and reported every syllable he said" (Webb et al., 2000).

The popular emphasis on individual bureaucrats is at odds with the conception of these organizations advanced by the mainstream literature on cooperation and delegation. The dominant approach in the literature invokes the principal-agent framework: member states, acting as principals, delegate policy making functions to an international organization, the agent (Hawkins et al., 2006).[2] A number of studies explore the role of bureaucratic preferences within organizations, yet these studies conceptualize bureaucratic preferences as monolithic or uniform across individuals and therefore synonymous with the preferences of the IO itself (Chwieroth, 2013; Copelovitch, 2010; Nelson, 2014). To our knowledge the current work represents the first study to conceptualize the role of individual bureaucrats - with diverse preferences - in the policy making process.

This approach highlights a conceptual shortcoming of existing approaches. These approaches conflate the *objects* of delegation (individual bureaucrats) with the *mechanisms* of delegation (the institutional structures that make up an organization). Where institutional structures commit member states to the delegation of authority, this authority is enacted by individuals; individuals with motivations and preferences about which little is known within existing scholarship. A central question then, is the extent to which the structure of international organizations does in fact commit member states to credible delegation.

Where delegation is credible, individual bureaucrats will exert an independent effect on policy making outcomes suggesting a fruitful and novel avenue for future studies of international organizations. Where delegation is not credible, but rather is easily abrogated, individual bureaucrats will exert little independent influence on IO policy making. To resolve this question, the current work develops and tests a theory of credible delegation within the context of the International Monetary Fund (IMF). The IMF is widely regarded as one of the more autonomous international organizations making this a valuable case in which to explore the *de facto* credibility of delegation.

[2]See Dreher and Vaubel (2004) and Barnett and Finnemore (2004) for classic analyses focusing on the IMF as an autonomous actor.

Establishing the credibility of delegation—and therefore the independent impact of individual bureaucrats—presents a considerable empirical challenge. This is the case because bureaucrats are selected carefully by the same principals they serve. When a newly appointed bureaucrat changes course it may be due to his or her own independent influence on the policy making process or simply a reflection of the changing preferences of the organization's principals. Principals may select new bureaucrats not only to implement policies close to their own preferences, but also to provide information to markets and members alike about their intentions moving forward.

Estimating shifts in market perceptions of sovereign risk following the announcement of new bureaucratic appointments may provide insights into both investor expectations regarding the bureaucrat's own policy preferences or leanings as well as the extent to which these preferences will matter for lending or other outcomes, in other words the extent of delegation from principals to bureaucrats. Yet interpreting these observed market reactions is non-trivial if they simultaneously convey new information to investors regarding principals' evolving preferences, or even changing expectation of member state behavior in the wake of these new appointments.

Our core contribution is to propose a framework through which it is possible to interpret these market reactions while taking into account each of the mechanisms just described. We develop a formal model incorporating strategic interaction by principals, member states, and investors. Within the model a principal must trade off encouraging long-term economic reform in member states with signalling the availability of IMF financing, thereby reducing investor perceptions of sovereign risk in the short term. The model highlights the moral hazard problem inherent in IMF lending: the availability of financing may encourage poor economic policies in member states (Dreher and Vaubel, 2004). Yet in the absence of credible delegation, principals cannot commit themselves to restricting available finance *ex post* if a crisis arises.[3] At the same time we take into account the equilibrium effects of delegation as a signal to investors regarding the principal's preferred policy. In equilibrium these competing incentives offset one another leading to partial separation of types and therefore informative signalling to market actors.

Our analytical results allow us to characterize shifts in investor beliefs following the announcement of new bureaucratic appointments. In turn this highlights the contribution of each mechanism which contributes to the inferred change in investor beliefs. Most im-

[3]Following Rogoff (1985) we emphasize delegation to bureaucrats whose lending preferences are (weakly) more stringent than those of the principal.

portantly this characterization of the change in investor beliefs reveals a counter-intuitive dependence on the presence of credible delegation. We show analytically that when there is no credible delegation to bureaucrats, shifts in investor beliefs must approach zero. The logic underlying this result is simple: in the absence of delegation, selection of bureaucrats is a costless signal of the principals' intent. Incentives to misrepresent the principals' commitment to austerity will render signals - or appointments - meaningless to both member states and skeptical investors. This counter-intuitive result forms the basis of a simple hypothesis test. Under the null hypothesis of no delegation, it must be the case that we observe no systematic shifts in investor beliefs following the announcement of new staff appointments. Rejecting this null hypothesis necessarily implies a positive level of delegation.

We use these result to ground our subsequent empirical analysis. Specifically we test the hypothesis that bureaucratic appointments do lead to shifts in investor beliefs, implying a strictly positive level of credible delegation. We derive the theoretical quantity of interest—the change in investor beliefs following announcement of new bureaucratic appointments—and show that it is causally identified within a standard event study framework. We subsequently estimate the change in investor beliefs following the appointment of new IMF area department heads. We find that, on average, new appointments lead to statistically significant shifts in investor beliefs, supporting the credibility of delegation to high-level IMF staff.

We also explore the micro foundations of our argument by analyzing the individual impacts of area department directors on the biannual economic forecasts which form the basis of the IMF's World Economic Outlook. While lending typically receives greater media and scholarly attention we argue there are particular theoretical as well as empirical reasons to focus on the Fund's surveillance activities instead, themselves an important component of the organization's mandate. We explore the systematic impact of individuals' prior employment experience, finding significant differences in the expected forecast error for individual directors. This supports our intuition that bureaucrats differ in systematically meaningful ways and that in the presence of delegation these differences have important implications for policy outcomes.

Finally, we conclude with a discussion of how the study of bureaucratic impacts may be advanced in future work, highlighting a growing number of data sources and creative approaches with promise to facilitate further accumulation of knowledge on the role of individuals within international organizations.

2 Model

There is a principal, a member state, and a (non-strategic) representative investor. The principal's objective is to minimize the expected losses associated with a balance of payments crisis within the member state. This expected loss depends both on the probability of crisis occurring and on the loan disbursement issued given that crisis materializes. We begin by describing the role of the IMF in case a crisis materializes and subsequently describe the actions of each player which may mitigate the potential for and costs of crisis.

In the event that a crisis occurs, the IMF offers a loan to cover the balance of payments gap. The *optimal* loan is a random variable, ω, normalized to lie within the unit interval. ω is distributed according to F with associated density, f, which is continuous with full support. Substantively ω may represent either the expected economic needs of the member state or alternatively may incorporate a measure of the importance of that state to powerful members of the executive board. Thus ω may interchangeably represent the ideal loan of either the IMF's managing director or of the executive board. We assume that ω is realized at the beginning of the game and observable to both principal and member state (though not the representative investor).

The realized loan provided by the IMF will also depend on the level of delegation embedded within the institution and the type of bureaucrat appointed by the principal to oversee policy making. Let $r \in \mathbb{R}_0^+$ represent the exogenous degree of (credible) delegation and $\theta \in [0, 1]$ the bureaucrat's type, observable to all. The expected loan is given by $l(\theta, \omega, r)$, continuously differentiable in each of its arguments. Letting subscripts denote derivatives we make the following assumptions: $l_1 \geq 0$, $l_2 > 0$, $l_3 \leq 0$. In addition, $l_{1,3} > 0$ so that when r is high, changes in the bureaucrat's type have a significant impact on the realized disbursement. Lastly we assume $\lim_{r\to 0} l_1 = 0$ and $lim_{r\to 0} l(\theta, \omega, r) = \omega$.[4] The former implies that when there is no credible delegation, the bureaucrat has no effect on policy. The latter implies that when the bureaucrat has no effect on policy, the realized disbursement will coincide with the principal's ideal disbursement, $l(\theta, \omega, 0) = \omega$.

While we maintain the more general form throughout the analysis, it is easy to identify an intuitive functional form for $l(\theta, \omega, r)$. Let $\theta\omega$ be the bureaucrat's ideal policy and r be

[4]Implicitly we draw on the logic of Rogoff (1985) in assuming that principals select bureaucrats more conservative than themselves in order to resolve the moral hazard induced by the availability of IMF financing. Formally $\max_r l(1, \omega, r) = \omega$ for any $r \geq 0$.

the bargaining power of the bureaucrat vis á vis the principal. In this case, the expected loan can be represented by a simple contest function,

$$l(\theta, \omega, r) = r\theta\omega + (1 - r)\omega$$

Note that in the general case $l(\theta, \omega, r)$ may be probabilistic - representing the expected disbursement - or fully deterministic. Note that selecting $\theta < 1$ reduces the ability (or expected ability) of the principal to achieve her ideal disbursement, ω.

Next we introduce two channels through which the principal's selection of θ may moderate the likelihood of crisis, thus factoring into the principal's optimal choice of bureaucrat. First, the probability of crisis is decreasing in the extent of economic reform chosen by the member state, $x \in \mathbb{R}^+$. Second, the probability of crisis is decreasing in the valuation of a representative investor, updated continuously throughout the game. This valuation depends on the anticipated (or realized) level of reform adopted by the member state as well as the expected loan in case of crisis. In contrast to the principal and member state, the investor is unable to observe the true state of the world, $\omega \in \mathbb{R}$. Instead denote the investor's beliefs about the state of the world by, $\hat{\omega}$.

The investor's valuation is, $V(x, \theta, \hat{\omega})$ where $V_1 > 0$, $V_2 \geq 0$ and $V_3 > 0$. In addition, $V_{11} < 0$, and $\lim_{r \to 0} V_2 = 0$.[5] Note that the principal's equilibrium choice of θ may depend on the realization of ω and thus reveal information about the state of the world to investors. Where this is the case, $\hat{\omega}$ will reflect beliefs updated via Bayes' Rule. The probability of crisis at any given time is $\gamma(x, V(x, \theta, \hat{\omega}))$, where γ is a mapping, $\gamma : \mathbb{R}^+ \times \mathbb{R}^+ \to [0, 1]$. We make the following assumptions on the probability function: $\gamma_1 < 0$, $\gamma_2 < 0$, $\gamma_{11} > 0$, and $\gamma_{22} > 0$.

Turning to the member state's incentives, economic reforms induce political costs to the member state government according to continuously differentiable mapping, $\phi : \mathbb{R}^+ \to \mathbb{R}^+$, where $\phi' > 0$ and $\phi'' > 0$. The member state balances the costs of reform against the expected losses of crisis, given by $u_m(l(\theta, \omega, r), P)$. We assume that crisis produces a strictly negative payoff for any realized loan so that $0 > u_m$ for any parameters. The member state's expected loss from crisis depends on the anticipated loan in case a crisis is realized and a second parameter, $P > 0$, which represents the costs of long-term loss of

[5] We allow for the possibility that the internal workings of the IMF are not fully observable to investors thus the dependence of V on θ, $\hat{\omega}$, and r may take a different form than $l(\theta, \omega, r)$ as is the case for the principal and member state.

market confidence. Let $u_{m,1} > 0$ and $u_{m,2} < 0$. In sum, member state expected utility is

$$U^m = \gamma(x, V(x, \theta, \hat{\omega}))u_m(l(\theta, \omega, r), P) - \phi(x)$$

Finally, consider the preferences of the principal. As in the case of the member state, the costs incurred by a crisis are strictly negative and depend on the realized loan size, the optimal loan size, and an exogenous parameter $D > 0$ representing the non-monetary costs of crisis to the principal. The principal's expected utility is,

$$U^P = \gamma(x, V(x, \theta, \hat{\omega}))u_P(l(\theta, \omega, r), \omega, D)$$

where we assume $u_{P,1} > 0$, $u_{P,2} < 0$, $u_{p,3} < 0$, and $u_{P,1,2} > 0$.

The sequence of events is as follows. Following the realization of ω, the principal appoints a bureaucrat of type θ. After observing θ, the member state selects an optimal level of reform, x. Following this, crisis is realized or not, and the IMF disburses a loan according to $l(\theta, \omega, r)$. Investors update beliefs according to Bayes' Rule continuously through the game. For our analysis we focus on characterizing investor beliefs following the announcement of θ in keeping with our empirical design below. We refer to these beliefs as investor interim beliefs. At times we also refer to investor *ex ante* or *ex post* beliefs defined respectively as those prior to the principal's choice of bureaucrat and those following the member state's choice of reform respectively.

The equilibrium concept is Bayesian Perfect Equilibrium, requiring that investor beliefs about ω are derived according to Bayes' rule on the equilibrium path and that the actions of other players are sequentially rational given these beliefs. Note that investor uncertainty transforms a game of moral hazard into a signalling game with lying costs similar to Kartik (2009). The latter establishes the impossibility of a fully separating equilibrium in a general model of signalling with costs of dissimulation. Accordingly we focus in the present work on characterizing semi-separating and pooling equilibria.[6] A strategy for the principal is given by $\mu : \Omega \to \Theta$. A strategy for the member state is, $x : \Omega \to \mathbb{R}^+$. Denote a game by $\Gamma^c = (r, F, \gamma, \phi, l)$.

[6] Kartik (2009) also shows that every semi-separating equilibrium of a general signalling game with lying costs converges to a pooling equilibrium as costs increase. Our analysis below yields a similar result.

3 Results

We begin by describing equilibrium behavior when information transmission is possible.[7]

Proposition 1 (Equilibrium Characterization)**.** *Let $r > 0$. Then Γ^c admits a semi-separating equilibrium. For some $\bar{\omega} \in (0,1)$:*

1. *The Member State's choice of x^* is strictly decreasing in θ.*
2. *The Principal's optimal strategy is defined piece wise.*
   - *For $\bar{\omega} > \omega$, μ is one-to-one (perfectly informative), monotone increasing, and continuously differentiable.*
   - *For $\omega \geq \bar{\omega}$, the principal's optimal choice is $\mu(\omega) = 1$ (no information transmission).*
3. *Investor interim beliefs are,*

$$\hat{\omega} = \begin{cases} \omega & \text{if } \bar{\omega} > \omega \\ \frac{1}{1-F(\bar{\omega})} \int_{\bar{\omega}}^{1} t dF(t) & \text{otherwise} \end{cases} \tag{1}$$

Note that when delegation is at least somewhat credible - i.e. the bureaucrat is invested with influence over policy making - equilibrium selection of the bureaucrat's type described in Proposition 1 must balance several direct and indirect effects. θ exercises a *direct* effect on the member state's choice of policy reform and on investor (interim) beliefs about the likely level of IMF financing in case crisis occurs. θ also *indirectly* affects both investor (interim) valuations and the member state's choice of policy reform since both are influenced respectively by the direct effects just described: the member's state's optimal reform choice reflects investor valuations while investor valuations reflect the choice of reform induced by θ.

To provide a concise intuition, we momentarily set aside these indirect effects and consider only the immediate impact of θ on policy reform and investor valuation. Choosing a higher bureaucratic type signals to investors that the IMF will provide generous levels of finance in case of crisis. This reassures investors, increasing their interim valuation and reducing

[7] Appendix A provides a formal characterization of equilibrium strategies as well as proofs of all Propositions stated below.

the probability of crisis. On the other hand, a higher value of θ signals the same to the member state - that generous IMF financing will be made available in case of crisis - reducing that member's incentive to reform. This has the opposite effect of increasing the probability of crisis.

In contrast to a simple game of cheap talk in which the principal would have no credible way of signalling its intentions to markets, the offsetting effect of reducing member state incentives to reform renders higher signals more costly to the principal. Again, setting aside indirect effects, in equilibrium the principal selects the bureaucrat's type so that the marginal gain from inflating investor valuations is exactly offset by the marginal cost of reducing incentives to reform. Proposition 1 establishes that this same intuition holds when taking into account the indirect effects of markets and member state decision making.

Next we consider equilibrium behavior as the level of delegation approaches zero. Proposition 2 establishes that as $r \to 0$ no information transmission is possible.

Proposition 2 (Convergence to Pooling Equilibrium)**.** *As $r \to 0$ every semi-separating equilibrium converges to a pooling equilibrium in which $\theta^* = 1$. Investor interim beliefs are,*

$$\hat{\omega} = \int_0^1 t dF(t)$$

When delegation is no longer guaranteed by the institutional setting, the choice of bureaucrat exerts no influence on expected IMF financing. In turn is also exerts no influence on member states' incentives to reform. This reduces the cost of selecting higher bureaucratic types while also reducing the informativeness of this selection. In the limit, no information transmission is possible. At the same time, the member state's reform efforts are no longer influenced by the bureaucratic type. Thus when delegation is not credible investor valuations will reflect neither information transmission nor changes in the expected level of member state reform. Proposition 3 establishes this formally: in the absence of credible delegation, the selection of bureaucrats exerts no equilibrium impact on investor valuations.

Proposition 3 (Irresponsiveness of Equilibrium Beliefs)**.** *As $r \to 0$, the investor's interim beliefs converge to her ex ante beliefs.*

This result provides a basis for our empirical test below. Shifts in market valuations following the announcement of bureaucratic appointments necessarily implies credible delegation.

At the same time, leveraging the equilibrium characterization in Proposition 1 we can derive the change in investor beliefs following the announcement of bureaucratic appointments. As described above doing so is of central importance to our empirical analysis as it highlights what can and cannot be inferred from the empirical results below. Suppose $r > 0$. The change in investor valuations following the appointment of a bureaucrat of type θ is,

$$\Delta^{\theta} = V^{ExAnte}(\mathbb{E}_F[x^*|\mu(\omega), \omega], \mathbb{E}_F[\mu(\omega)], \mathbb{E}_F[\omega]) - V^{Interim}(\mathbb{E}_{F|\theta^*}[x^*|\theta^*, \omega], \theta^*, \mathbb{E}_{F|\theta^*}\omega) \tag{2}$$

Note the selection of θ impacts this quantity firstly by shifting investor beliefs from F to $F|\theta^*$ and secondarily by eliminating uncertainty as the realization of θ^* itself. In the next section we show that it is possible to estimate $Delta_0$ using standard event study methods.

4 Framework for Estimation

In this section we first describe the event study methodology which we employ as well as our data and (statistical) modelling strategy. Following this we discuss the connections between our event study framework and the analytical results described above. In particular, we establish that the estimand recovered from the event study corresponds directly to Δ_0, the shift in investor beliefs attributable to newly-announced bureaucratic appointments.

We employ an event study framework to estimate the impact of staff appointments. Event studies have been widely employed in the literature on corporate finance and, increasingly, the study of international institutions. In the context of corporate finance, event studies explore changes in firm stock prices in reaction to the disclosure of new information. The core intuition behind these event studies is that the magnitude of unanticipated returns to stock prices provides a useful measure of the impact of events on shareholder wealth (Kothari and Warner, 2007).

More recently, within the study of international institutions, event studies have been employed to assess the credibility of negotiated outcomes. Wilf (2016) studies the impact of Basel III negotiations on regulated banks, finding evidence that international negotiations are viewed as credible and thus impact perceptions of banks' value. In a similar vein Kucik and Pelc (2016) demonstrate that dispute settlement rulings within the World Trade Organization impact the value of firms even in countries not party to specific disputes, evidence that investors anticipate systemic shifts in regulatory policy following novel judicial rulings.

In contrast to these approaches we employ an event study to assess whether the identity of new staff appointments conveys credible information about the likely directions of future policy and thus the riskiness of sovereign lending. We define an event as any change in the heads of area departments, and consider the date of the event the first announcement identifying the appointment of a new Department Director. The IMF has a total of five area departments, in charge of offering macroeconomic and financial sector advice to the countries under their jurisdiction: African Department; Asia and Pacific Department; European Department; Middle East and Central Asia Department; Western Hemisphere Department.[8] While non-regional departments also operate in the IMF, they cover functional or support activities only, so we do not consider them in our analysis.

4.1 Event Study Estimation

To identify the events in our sample we gather IMF press releases detailing new staff appointments. The press releases constitute the first public announcement of such changes in senior official positions. We then code all announcement dates as the dates of the events. Table 1 lists the set of staff appointments which we identify and employ in the analysis below. A condition for identification of market reactions to new appointments is for these new appointments to be unexpected. If new appointments were entirely predictable, they should not impact investor sentiment in a specific window. We employ three methods to verify the events in our sample were unexpected. First, we conduct systematic reviews of investor news reports such as the Wall Street Journal (WSJ) for the year preceding an appointment announcement. We pay particular attention to those cases where the head of an area department announces her decision to quit far in advance, or similarly, in cases where exogenous shocks led to a change in the head of an area department. Second,

[8] https://www.imf.org/external/about/staff.htm#area

we conducted a series of interviews about the appointment process at the IMF. Both methods led us to conclude that the process is very confidential, and that, in practice, both internally within the IMF and externally the information on the appointment is known almost simultaneously, when the communications department issues an official announcement about the appointment. We also tackle this issue empirically, by dropping days -4 to -1 from our estimation window. Doing so ensures that any anticipation effects will not affect the results.

To construct our dependent variable, we collect data on daily sovereign bond interest rates for all available countries for each event using the Global Financial Database (GFD). Data availability is limited in particular for developing countries prior to 2010. For this reason we are forced to drop several events from our sample, as noted in Table 1. To calculate our dependent variable we subtract the U.S. daily interest rate on benchmark ten year bonds from that of each country in the sample. The resulting measure, *Spread*, describes the risk premia associated with a particular country's borrowing.

We follow the standard literature in finance to implement the event study. For each event we define treated countries as those within the region corresponding to the new staff appointment, irrespective of the country's IMF loan status. That is, we include all countries in the relevant region regardless of their prior interactions with the IMF. In doing so we rely on the anticipated systemic consequences of IMF lending decisions for the region as a whole.

Index events by $k = 1, ..., K$ and treated countries by $i = 1, ..., N_k$. For all events we define an estimation window, $[-L, -l]$, consisting of a continuous period of $L - l + 1$ days prior to the event date. We also define an event window, $[t, T]$, consisting of a $T - t + 1$ length interval of days following each event. Index days by $t = -L, ..., 0, ..., T$, where $t = 0$ corresponds to the announcement date. For each treated country and each event we first estimate the following "market model" of normal returns using data from days within the estimation window only, $t \in [-L, -l]$,

$$Spread_{i,t} = \alpha_i + \beta_i Index_{k,t} + \epsilon_{i,t}$$

$Index_{k,t}$ is calculated as the mean of $Spread_{j,t}$ for all countries outside of the treated region for event k. We use the resulting parameter estimates to predict $Spread_{i,t}$ for each day in the event window, $t \in [t, T]$.

Country i's abnormal return following event k is equal to the difference between the

Table 1: IMF Senior Staff Appointments, 1990-2016

Department	Director	Announcement Date	In Sample?
European II	John Odling-Smee	9-Jan-92	No
Middle Eastern	Paul Chabrier	20-Nov-92	No
African	Evangelos Calamitsis	5-Oct-94	No
Western Hemisphere	Claudio Loser	5-Oct-94	No
Asia Pacific	Hubert Neiss	6-Dec-96	Yes
European I	Michael Deppler	6-Feb-97	Yes
African	G.E. Gondwe	8-Dec-98	No
Asia Pacific	Yusuke Horiguchi	28-Jan-00	Yes
African	Abdoulaye Bio Tchane	10-Jan-02	No
Middle Eastern	George Abed	10-Apr-02	No
Western Hemisphere	Anoop Singh	10-Jun-02	No
Asia Pacific	David Burton	30-Sep-02	Yes
Middle East and Central Asia	Mohsin Khan	30-Jul-03	Yes
Asia Pacific	Anoop Singh	1-May-08	Yes
Middle East and Central Asia	Masood Ahmed	1-May-08	No
African	Antoinette Sayeh	27-May-08	No
European	Marek Belka	15-Jul-08	Yes
Western Hemisphere	Nicolas Eyzaguirre	27-Aug-08	Yes
European	Antonio Borges	26-Oct-10	Yes
European	Reza Moghadam	16-Nov-11	Yes
Western Hemisphere	Alejandro Werner	6-Nov-12	Yes
Asia Pacific	Changyong Rhee	26-Nov-13	Yes
European	Poul Thomsen	3-Nov-14	Yes
African	Abebe Aemro Selassie	15-Sep-16	Yes
Middle East and Central Asia	Jihad Azour	1-Dec-16	Yes

Announcement of new senior staff appointments 1990 - 2018. *Announcement Date* refers to the date of IMF press release detailing the appointment of a new director. Gray indicates those events dropped from the analysis due to data limitations.

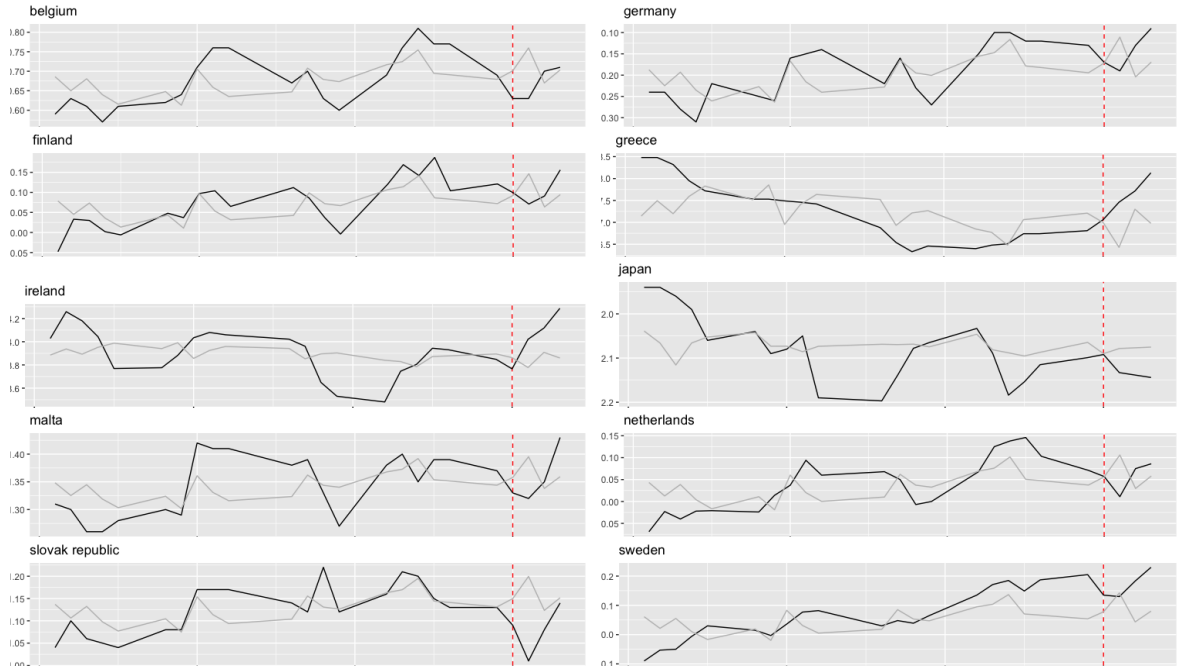

Figure 1: **Country Trends.** Raw risk premia trends following the announcement that Antonio Borges would be appointed director of the European Department. Plots depict estimation window $(-30, -5)$ and event window $(0, 5)$.

observed risk premium $Spread_{i,t}$ and the predicted risk premium $\widehat{Spread}_{i,t}$. This abnormal return corresponds to unanticipated shifts in risk premia resulting from the information conveyed by announcement k. We define cumulative abnormal returns for country i following event k as,[9]

$$\widehat{CAR}_{i,k} = \sum_{t \in [t,T]} Spread_{i,t} - \widehat{Spread}_{i,t}$$

and cumulative average abnormal returns following event k as,

$$\widehat{CAAR}_k = \frac{1}{N} \sum_{i=1}^{N} \widehat{CAR}_{i,k}$$

Our estimate of the sampling variance for event k is calculated as the mean variance of abnormal returns observed during the estimation window (Kothari and Warner, 2007). Figure 1 provides intuition for the methodology by plotting the observed (black) versus predicted (grey) risk premia for several countries before and after an event announcement (indicated by the red dotted line).

[9]For discussion of these quantities of interest and their associated variance see Dasgupta et al. (1998).

4.2 Relation to Analytical Results

We next establish that the standard market model precisely recovers the quantity of interest, δ_0, derived from the formal analysis above. Index countries by $i = 1, 2, ..., N$ and time periods by $t = ..., -2, -1, 0, 1, 2, ...$ We normalize time so that $t = 0$ corresponds to the announcement of a new bureaucratic appointment relevant to country i. Let $\alpha_{i,0}$ denote time invariant factors prior to announcement of the type θ, that is for all periods, $t < 0$. The parameter $\alpha_{i,0}$ thus incorporates investors' *ex ante* beliefs and expectations. Let $\boldsymbol{X}_{i,t}$ be a vector of time-varying factors which also influence investor valuations. We assume that *ex ante* valuations of country i's perceived risk take the following form,

$$V_{i,0} = \alpha_{i,0} + \boldsymbol{X}_{i,t}\boldsymbol{\beta}_i + \epsilon_{i,t}$$

where $\boldsymbol{\beta_i}$ is a coefficient vector and $\epsilon_{i,t}$ is an exogenous shock distributed normally with mean zero and variance σ^2.

Now, consider investor *interim* beliefs. Again, these can be decomposed into the *ex ante* time-invariant component $\alpha_{i,0}$, a shift parameter, $\alpha_{i,1}$ representing the change in investor beliefs and expectations as well as and time-varying factors $\boldsymbol{X}_{i,t}$. *Interim* valuations then take the form,

$$V_{i,1} = \alpha_{i,0} + \alpha_{i,1} + \boldsymbol{X}_{i,t}\boldsymbol{\beta}_i + \epsilon_{i,t}$$

where each element of β_i is assumed constant across time. With these expressions in hand, we can characterize the change in investor valuations attributable to the bureaucratic appointment,

$$\Delta_0 = \alpha_{i,1} = V_{i,1} - \alpha_{i,0} - \boldsymbol{X}_{i,t}\boldsymbol{\beta}_i - \epsilon_{i,t}$$

The above equation directly corresponds to the standard "market model" frequently employed in the context of event studies. To see this, note that the expression can also be written in terms of the predicted value of sovereign risk employing estimates of $\alpha_{i,0}$ and $\boldsymbol{\beta_i}$ from the pre-announcement period,

$$\Delta_0 = \alpha_{i,1} = V_{i,1} - \hat{V}_{i,1} - \epsilon_{i,1}$$

where $\hat{V}_{i,1} = \hat{\alpha}_{i,0} + \boldsymbol{X}_{i,t}\hat{\boldsymbol{\beta}}_i$

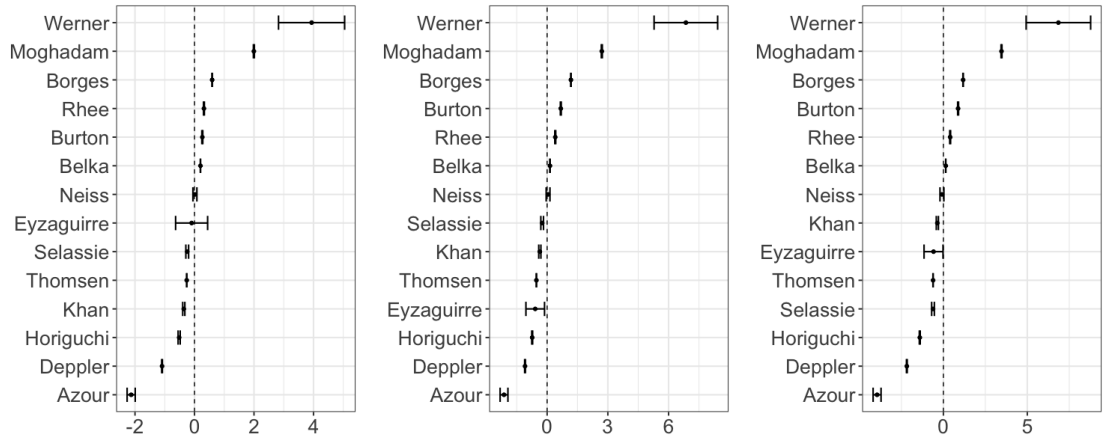

Figure 2: **Cumulative Abnormal Returns Effects by Event.** Cumulative abnormal returns effects by event for estimation window $(-180, -5)$ for different event windows (from left to right): $(0, 1)$, $(0, 3)$, and $(0, 5)$. Standard Errors clustered by region.

4.3 Results

Figure 2 presents the main results of our event study analysis, testing the primary hypothesis that financial markets will react to new appointments. We present results for an estimation window of $(-180, -5)$ days and three different event windows, $(0, 1)$, $(0, 3)$, and $(0, 5)$. The figure illustrates substantial heterogeneity in both direction and magnitude of the Cumulative Abnormal Returns Effects. Nevertheless, the analysis establishes that the announcement of new area department directors sends credible signals to the market about the future direction of IMF policy for the region where the change occurred. Effects are consistent across estimations.

To assess the substantive significance of the estimated shifts in risk premia we compare the results above with those corresponding to several high-profile events which we also expect to have an impact on expectations of country risk. First, we consider the announcement of reforms to the IMF's formal governance structure. These reforms reflect a growing consensus that the representation of countries such as Brazil, China, and Mexico have not kept pace with their growing contribution to the global economy. To examine these changes, we consider two milestones in the reform process: first, the entry into force of the Voice and Participation Amendment and second, passage of legislation by the United States Congress approving reforms to IMF quota allocations.[10],[11]

[10]While we identified several additional milestones in the course of the reform process we are forced to limit our attention to the two described above due to data constraints.

[11]See Appendix A for additional background on quota reform and the specific events employed in the

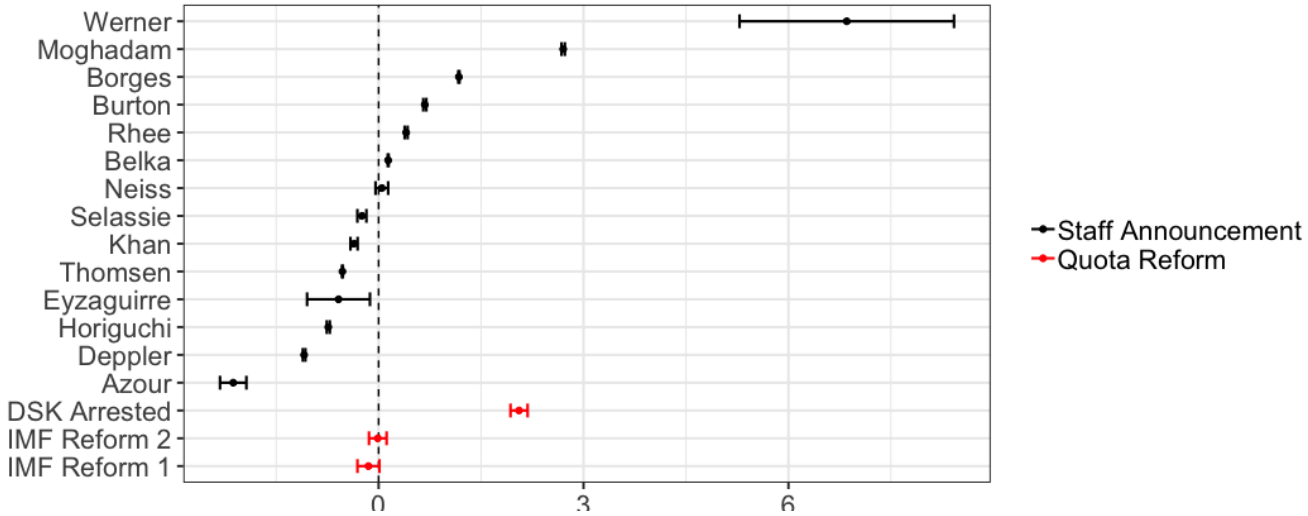

Figure 3: **Substantive Significance: Magnitude Relative to Other Events.** Cumulative abnormal returns with confidence intervals by event for estimation window $(-180, -5)$ and event window $(0, 3)$. Black indicates staff announcement. Red indicate either quota reform announcements or the date former IMF Managing Director Dominique Strauss-Kahn was arrested. Standard Errors clustered by region.

As a second benchmark we consider financial market reactions to the arrest of then-IMF Managing Director, Dominique Strauss-Kahn for sexual assault of a hotel employee on May 14, 2011. Strauss-Kahn resigned his position at the IMF in the immediate aftermath of his arrest, though given the seriousness of the charges against him we employ the date of arrest as the first significant signal that a change in top-level leadership was immanent.

Our analysis of these three events is similar to that in the main estimation above with one change. Rather than define treatment at the region level (since all regions arguably receive the same treatment for each of these events) we define treated countries only as those most directly impacted by IMF policies, that is countries under IMF lending arrangements during each event. We anticipate that quota reforms, which result in a more equal distribution of political influence, will lead to higher risk assessments, reflecting enhanced credibility of IMF conditionality.[12] We anticipate that the arrest of Strauss-Kahn will also lead to higher risk assessments reflecting market uncertainty about his replacement and implications for future policy. The results are plotted in Figure 3.

Finally we consider variation in country-level abnormal returns as well as well as the sta-

analysis.

[12]This follows Copelovitch (2010) which argues that heterogeneity of influential state interests can act as a check on overall politicization of lending decisions.

bility of these country level estimates across event and estimation windows. As Figure 5 makes clear the appointments of Poul Thomsen and Reza Moghadam produced dramatically different assessments by financial actors of the direction of future IMF policy and its implications for regional stability. While estimates for Thomsen are consistently negative and statistically significant across countries the reverse is true for Moghadam, suggesting that the former appointment was viewed as reassuring by financial actors while the latter was interpreted pessimistically.

4.4 Robustness to Alternative Estimation Strategies

The validity of our results above rely on the accuracy of our predicted bond spreads for treated countries in the absence of a new staff appointment. Where the mean bond spread of untreated countries follows a different trend from our treated observations our predictions will be noisy and potentially biased. We can see from the example in figure 1 that our estimation does not achieve a perfect fit for the pre-treatment period. We use 2 different robustness checks to address the possibility of biased bond spread predictions.

Permutation Test

First, we conduct a permutation—or randomization—test (Fisher, 1935) to assess the sharp null hypothesis that our treatment—changes in area department heads—has no effect on the quantity of interest, change in investor beliefs. Under the permutation test, we compare our observed average treatment effect to its reference distribution under the sharp null hypothesis. To do so, we first shuffle the day of appointment of the area department head over a window of 60 days prior to the true appointment announcement date, as we expect appointments during this window to be plausibly random. We do so 100 times for each area department head announcement. We run the same estimation as above with these alternative permutations of treatment assignment, in practice calculating the test statistic that would have been observed under this alternative treatment schedule.

Table 2 shows results of the permutation test for an estimation window of $(-180, -5)$ days and event windows of $(0, 1)$ and $(0, 5)$ days. The p-value under the null hypothesis represents the proportion of permutations with a test statistic at least as large as the observed one. Lower p-values indicate a low number of shuffled CAAR estimations exceeded the true CAAR calculated under observed data. We can see that for most appointments the

proportion of permutations with a CAAR as large in magnitude as the observed CAAR is lower than 0.05. Figure 4 shows histograms of the CAAR under the permutation test by new area department head appointment, for an estimation window of $(-180, -5)$ days and event windows of $(0, 1)$ days. Here, we plot the true CAAR (vertical red line) against the histogram of shuffled CAAR. Since our hypothesis is not directional, no shuffled CAAR preceding the true CAAR—for true CAAR less than 0—and no shuffled CAAR exceeding the true CAAR—for true CAAR larger than 0—indicate we can reject the null hypothesis of no effects. We therefore reject the null hypothesis for most appointments.

Table 2: **CAAR Permutation Test**

Staff Name	p event (0, 1)	p event (0, 5)
Azour	0.010	0.010
Belka	0.110	0.150
Borges	0.010	0.090
Burton	0.020	0.120
Deppler	0.010	0.040
Eyzaguirre	0.040	0.120
Horiguchi	0.010	0.070
Moghadam	0.010	0.010
Neiss	0.130	0.150
Rhee	0.060	0.140
Sayeh	0.020	0.120
Selassie	0.040	0.120
Singh	0.010	0.010
Tchane	0.010	0.060
Thomsen	0.040	0.120
Werner	0.010	0.010

Note: Proportion of permutations with a CAAR as large as the true CAAR computed from the observed data. P refers to the p-value under the null hypothesis and represents the proportion of permutations with a CAAR as large in magnitude as the observed CAAR. Bolded area department head appointments indicate a p-value lower than 0.05.

Weighted Difference-in-Differences

As an additional robustness exercise, we employ a weighted difference-in-differences design to estimate the average treatment effect on the treated (ATT) of a change in area department head on $Yield_{i,t}$. We match treated observations with control observations similar on observable characteristics, using the matching methodology proposed by Imai

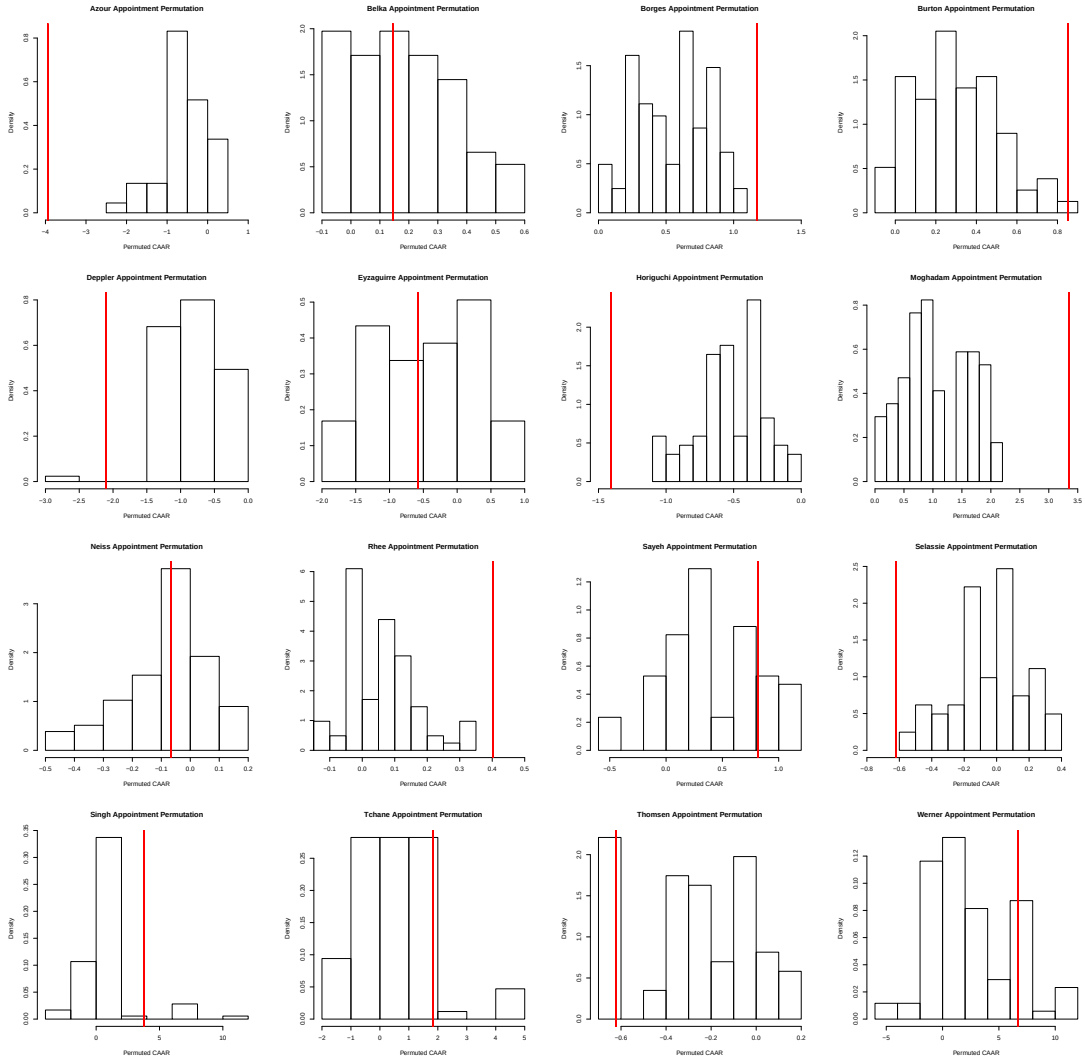

Figure 4: **Histogram of CAAR Under Permutation Test.** Red vertical lines indicate the true CAAR value under observed data, plotted against histograms of shuffled CAAR for each area director appointment event.

et al. (2019). They propose a nonparametric generalization of the difference-in-differences estimator that does not require the researcher to rely on the linearity assumption, as is the case with the more commonly used regression with two-way fixed effects. This is appropriate for our country-day data format, in which units receive treatment at different times and potentially multiple times.

To apply the method, we first select a set of control observations with identical treatment history in the period preceding treatment, for each treated observation. Following the main estimation, we consider each area department head appointment as separate treatments, and the countries under the area department where the change occurred as treated units. We therefore have a total of 25 treatments. Due to data limitations, the total number of treatments that enter the analysis is 16. We further refine this matched set to increase similarity of the covariate histories and outcomes of the matched control and treated observations. To do so, we use the lagged Yield for 5 days prior to treatment, as well as IMF Program—an indicator variable that takes the value 1 if a country was under an IMF program at the time of treatment—as matching covariates. We use two refining methods: minimizing the Mahalanobis distance between control and treated observations, and Covariate Balance Propensity Score (CBPS) weighting. Finally, we adjust for possible unobserved time trends via a difference-in-differences estimator.

We estimate the average treatment effect on treated units (ATT) at time t on the outcome Yield from time $t + 0$ to $t + 3$.[13] Results for the CBPS weighting method are shown in figure 8, while results for Mahalanobis matching are shown in figure 9 in the appendix. In most cases, the direction of the ATT effect is similar to the direction of CAAR effect identified in the main estimation. Not all effects in the estimation obtained with panel match achieve statistical significance. This could be because of the difference in sample sizes: the matches control set is much smaller than the full control set in the CAAR estimation. Also note that in the CAAR estimation, we average the cumulative abnormal return over a period of up to 5 days after a unit receives treatment, whereas the ATT in the weighted difference-in-differences specification is estimated separately for each day up to 3 days following treatment. In most cases, the average ATT over these 3 days following treatment would lead to more statistically significant results.

[13] All analysis has been implemented in `R` with the package `PanelMatch` (Imai et al., 2019), available at https://github.com/insongkim/PanelMatch

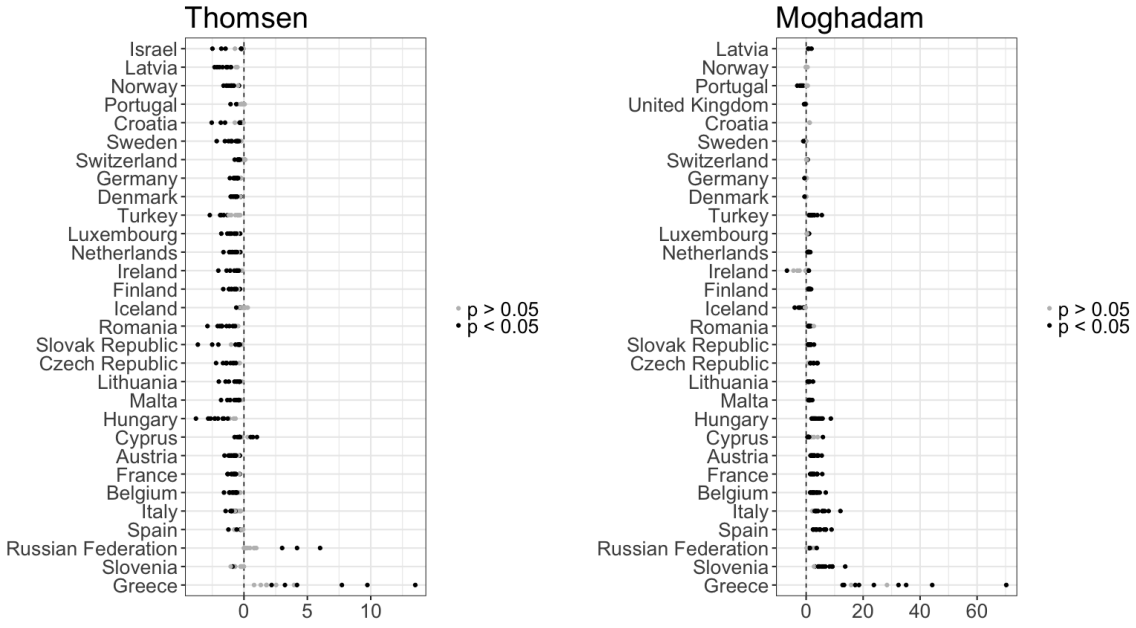

Figure 5: **Homogeneous Cumulative Abnormal Returns.** Cumulative abnormal returns for all event and estimation windows by country for indicated event. Black dots indicate significance at $p < 0.05$.

5 Individual Effects on Policy Outcomes

Next we consider the impact of individual senior staff members on policy outcomes, leveraging discontinuities surrounding their appointment to office. The IMF has two main policy functions: lending and surveillance as outlined in the institution's Articles of Agreement. We choose to focus on surveillance, in particular the creation of bi-annual economic forecasts for each country which are compiled and released as the World Economic Outlook.

These forecasts encompass major macroeconomic variables and are a core component of the IMF's surveillance activities. While lending is significantly more high-profile (and has been the subject of the bulk of existing scholarship), surveillance activities in fact account for a larger share of the IMF's internal budget. Forecasts draw primarily on information gathered by staff via consultation in-country with key government representatives. This makes them a valuable source of information which is disseminated not only to IMF member states but also the public at large. While forecasts are generated via econometric models, these methods nonetheless leave significant leeway for members of staff to influence their outcome. As noted by the IMF's Independent Evaluation Office:

"...the IMF [continues] to rely mostly on macroeconomic models where the financial risks and vulnerabilities are added on an ad-hoc basis" (Jeanne, 2018).

We choose forecasts as our outcome of interest for both theoretical and practical reasons. As noted above, a key drawback to delegation highlighted in the theoretical literature is the potential for information loss either via biased information transmission or via lack of competence, information loss which would manifest most directly in economic forecasting. Analyzing area department forecasts is also empirically convenient as WEO estimates are released at regular intervals and is thus independent of the appointment or retirement of individual area heads. In contrast, the timing of IMF lending may be plausibly endogenous to leadership transitions themselves. The biannual nature of WEO forecasts ensures a large number of observations for each director over time, again in contrast to IMF lending arrangements which are significantly more rare.

As discussed above existing literature on domestic bureaucratic performance emphasizes variation in individual policy biases as well as competence. Both of these characteristics will have implications for a staff member's expected forecast error. Thus systematic differences in directors' expected forecast error would suggest that delegation has a meaningful impact on policy outcomes, reflecting individual variation in these core traits.

> *H2: Area department directors will differ systematically in their forecast errors.*

Our outcome of interest is an individual area director's forecast error in period t, calculated as the predicted GDP for year $t+2$ less the observed GDP reported in year $t+4$.[14] We estimate the following model:

$$ForecastError_{i,t} = \alpha + \sum_{k \in \mathcal{K}} \left(\beta_k Director^k_{i,t} + controls + \gamma_i + \delta_t \right) + \epsilon_{i,t},$$

where γ_i represents country fixed effects and δ_t year fixed effects.

Dreher et al. (2008) distinguish among three possible sets of explanations for (optimistically) biased forecasts at the IMF: political strategic; defensive forecasting; and stability or mandate orientation. First, members countries might pressure IMF officials directly, or indirectly determine them to produce favorable forecasts, as the IMF depends on the support of its member states. Second, given the IMF can be seen as responsible for the economic performance of those countries under IMF programs, officials might "opt to roll

[14]The IMF regularly revises its forecasts and historical reporting enabling us to assess the forecast error for each country in each year t.

over the share of debt owed to the Fund" to preserve its reputation. Third, IMF officials might downplay risk to avoid spreading financial crises, since negative forecasts can lead to or intensify crises.

We account for the possibility of politically motivated bias by controlling for a country's propensity to vote with the U.S. at the United Nations General Assembly (`UN Affinity`, Bailey et al. (2017)). We account for the possibility of defensive forecasting by controlling for whether a country was under an IMF program in a particular year (`IMF Program`). In addition to this, predictions rely on the availability and accuracy of past outcomes, as past indicators are necessary to compute forecasts. We can expect officials will have a higher rate of inaccuracy where this data is not available. We account for this potential confounder by controlling for the `HRV Index`, which captures "government dissemination of aggregate economic data" (Hollyer et al.).

To test the two potential mechanisms behind variation in forecast accuracy—bias or ability—we compare the effect previous experience has on forecasts. We focus on two types of previous experience, associated with distinct types of human capital: having previously held office in the public sector/government (`Public`); and having worked primarily at the IMF prior to one's appointment to the level of Director of Area Department (`Internal`).

Internal hires will have extensive macroeconomic expertise, as well as previously developed policy networks, both within the IMF and with policy-makers in member countries. The former is especially important, as IMF decision-making is consensus based, with all departments giving some input into policy before it can be implemented, regardless of the department it originated in. The latter is equally relevant, since consultations with policy-makers in member countries are central to gathering the information used in forecasting. Given their institutional ties to the IMF, we can expect directors with extensive previous experience at the Fund to be more concerned about reputational costs or the perceived effects of IMF policy. These directors should therefore be susceptible to defensive forecasting, stability or mandate oriented forecasts, or political-strategic pressures. However, if, as we have argued in this paper, individual bureaucrats exert an independent effect on policy making outcomes only where delegation is credible, internal hires that exercise an *independent* effect on policy should have the opposite effect, leading to under-prediction.

Former public sector leaders, who might have previously served as Finance Ministers or other high level government positions, will also benefit from extensive macroeconomic expertise, as well as previously developed policy networks with policy-makers in other

countries (perhaps even more so within their own region). In addition to this, they might have more knowledge of the procedures on the other side of the IMF lending process, provided they held office in a country that was under IMF programs. On the other hand, because they lack institutional ties to the IMF, they will be less susceptible to internal or external pressures for optimistic forecasts.

If forecast error reflects ability, then these types of human capital derived from prior experience should increase the probability senior officials will accurately predict:

> *H2a: Absolute forecast error will be lower for Directors of Area Departments with previous experience at the IMF.*

> *H2b: Absolute forecast error will be lower for Directors of Area Departments with extensive previous public policy experience.*

To test our ability hypotheses, we estimate the following model:

$$AbsoluteForecastError_{i,t} = \alpha + \beta_1 \cdot Internal_{i,t} + \beta_2 \cdot Public_{i,t} + controls + \gamma_i + \delta_t + \epsilon_{i,t},$$

where γ_i represents country fixed effects and δ_t year fixed effects. We use the Absolute Forecast Error as our outcome because we are interested in how accurate the forecasts are. Ability should not have an effect on the director of the error, where error occurs.

If forecast error reflects bias, in an environment with credible delegation, departments under Directors with previous experience at the IMF should be socialized to provide more conservative forecasts via the acculturation process which has been documented within the IMF (Barnett and Finnemore, 2004). We make a similar prediction in the case of Directors with previous public sector experience who would be well-versed in the drawbacks of excessively optimistic predictions.

> *H2c: Area department directors with previous experience at the IMF under-predict.*

> *H2d: Area department directors with previous public sector experience under-predict.*

To test our bias hypotheses, we estimate the following model:

$$ForecastError_{i,t} = \alpha + \beta_1 \cdot Internal_{i,t} + \beta_2 \cdot Public_{i,t} + controls + \gamma_i + \delta_t + \epsilon_{i,t},$$

where γ_i represents country fixed effects and δ_t year fixed effects. We use the Forecast

Error as our outcome because we are interested in whether senior officials over- or under-predict.

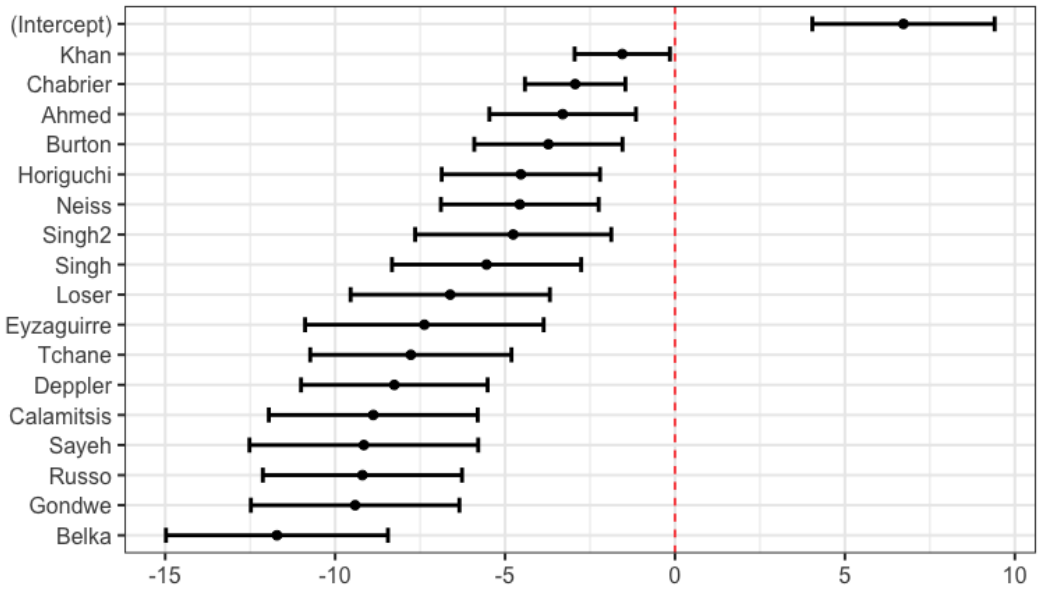

Figure 6: **Heterogeneous Effects on Forecast Error.**

Figure 6 presents results from our main estimation. We observe heterogeneous effects on forecast error by Area Department Director, confirming our hypothesis that area department directors will differ systematically in their forecast error. Area head indicator variables are jointly significant. Given our specification, effect magnitudes can be interpreted only with respect to the reference category.[15] Note that 'Singh' and 'Singh2'—who refer to the first and second appointment of Anoop Singh to different area departments at the IMF—have an almost identical effect size.

Next, we test our hypotheses about bias and ability. We used three specifications to test our hypotheses. The `No Covariates Model` presents results from the baseline model with no control variables. The `With Covariates Model` presents results from the model with all control variables included. The `Imputed Covariates Model` presents results for the model in which we imputed missing values on the control variables.[16]

We did not find evidence in support of our ability hypotheses 2*a* and 2*b*. As the left-hand

[15] Our reference category is George Abed, the area department director with the largest positive average forecast error for the economic indicator GDP.

[16] We mean imputed the control variable `Transparency` using the average of `Transparency` for all countries in the same region.

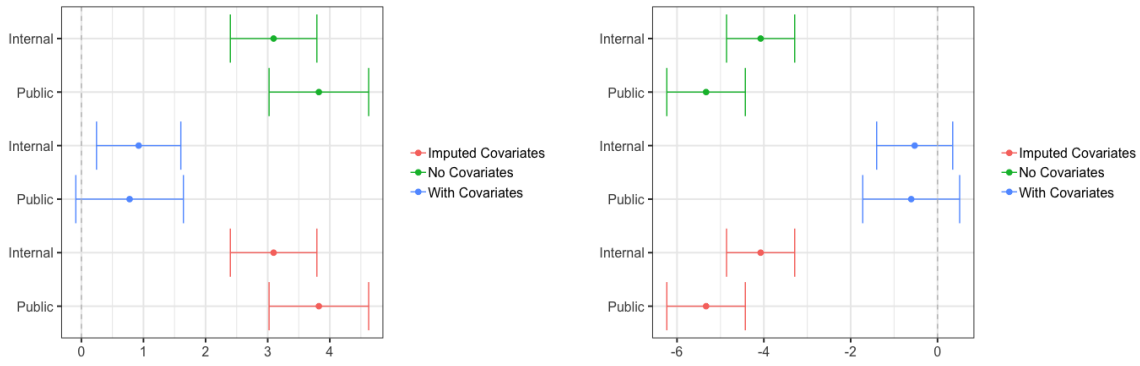

Figure 7: **Previous Experience Effects.** The left-hand side panel shows results for the outcome Absolute Forecast Error. The right-hand side panel shows results for the outcome Forecast Error. The `No Covariates Model` presents results from the baseline model with no control variables. The `With Covariates Model` presents results from the model with all control variables included. The `Imputed Covariates Model` presents results for the model in which we imputed missing values on the control variables. All models include country and year fixed-effects, as well as clustered standard errors.

side panel of Figure 7 shows, we observe a positive effect of both `Internal` and `Public` on the outcome `Absolute Forecast Error`, suggesting that the types of human capital derived from prior experience do not increase the probability senior officials will predict accurately.

We find evidence in support of our bias hypotheses $2c$ and $2d$. As the right-hand side panel of Figure 7 shows, we observe a negative effect of both `Internal` and `Public` on the outcome `Forecast Error`. Across specifications, `Internal` and `Public` prior experience lead to under-prediction, suggesting individual prior experience is indeed consequential for policy outcomes.

6 Conclusion

Do individual bureaucrats matter in international organizations? Existing explanations for IO policy emphasize the preferences of powerful member states as a central determinant of these institutions' policy making. Where credible delegation from member states to IOs exists, principals confer such delegation to the organization itself, conceptualized as the agent. Yet most IOs are highly bureaucratized organizations composed of individuals with motivations and preferences that might diverge not only from those of principals,

but also from those of other bureaucrats. This suggests the potential for impact at the individual bureaucrat level. In contrast to existing literature, we explicitly conceptually and empirically distinguish between the institutional mechanisms which ensure that delegation is credible and the individual bureaucrats whom enact authority conferred by delegation.

We motivate our analysis by developing a theory of insulation in response to a credible commitment problem. The availability of IMF financing leads to potential for moral hazard. Yet member states cannot commit not to intervene in times of crisis either out of concern for friendly regimes or for domestic interest groups. While full commitment is infeasible in international organizations, member states can raise the costs of their own intervention by encouraging delegation within the IMF to senior members of staff. At the same time, incentives to signal the IMF's policy direction to market actors provides an offsetting effect on incentives to delegate to bureaucrats holding divergent preferences.

Where delegation is non-credible, bureaucratic selection can neither incentivize reform nor communicate information about IMF policy preferences. Thus we predict that if delegation is credible, the announcement of new staff appointments will generate significant shifts in investor valuations.

We provide evidence that delegation to senior staff members is credible. Announcements of new staff appointments to area departments result in statistically significant shifts in risk premia for countries in the affected region. We also provide direct evidence of the (heterogenous) impact of individual bureaucrats on policy making, analyzing IMF country-level forecasts of economic indicators and finding significant evidence of systematic forecast errors.

This work contributes to the study of delegation in international organizations by explicitly distinguishing between the mechanisms (international institutions) of delegation and its objects (bureaucratic functionaries). We took a first step in establishing the identity of individual bureaucrats matters, by showing the only way to make delegation credible is to devolve power to a bureaucrat that has discretion over policy making. However, given the model's functional form explicitly incorporates policy functions specific to the IMF, the result is directly generalizable to IOs with jurisdiction over monetary policy making.

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Appendix A: Proofs

Proof of Proposition 1

Proof. For (1) the Member State's problem is,

$$\max_{x\in\mathbb{R}} \ \gamma(x, v(x,\theta,\hat{\omega}))u_m(l(\theta,\omega,r),P) - \phi(x)$$

Re-arranging the first order condition yields,

$$\phi'(x^*) = \left[\gamma_1(x^*, v(x^*,\theta,\hat{\omega})) + \gamma_2(x^*, v(x^*,\theta,\hat{\omega})\left[v_1(x^*,\theta,\hat{\omega})\right]\right] u_m(l(\theta,\omega,r),P) \tag{3}$$

Note that the solution is well-defined since ϕ' and $V_1 > 0$, while u_m, γ_1, and $\gamma_2 < 0$.

Optimality is confirmed via the second order condition,

$$u_m(l(\theta,\omega,r),P)\left[\gamma_{11}(x, v(x,\theta,\hat{\omega})) + \gamma_{22}(x, v(x,\theta,\hat{\omega}))V_1(x,\theta,\hat{\omega}) + \gamma_2(x, v(x,\theta,\hat{\omega}))V_{11}(x,\theta,\hat{\omega}))\right] - \phi''(x)$$

which is strictly negative by the assumptions on u_m, γ, V, and ϕ. The next step of the analysis relies on the following lemma.

Lemma 1. $0 \geq \frac{dx^*}{d\theta}$

For (2), the principal's problem is,

$$\max_{\theta\in[0,1]} \ \gamma(x, v(x,\theta,\hat{\omega}))u_p(l(\theta,\omega,r),\omega,D)$$

Note that when $\hat{\omega} > \omega$, the strategy $\mu : [0,\bar{\omega}] \to [0,1]$ is one-to-one and invertible, implying investor equilibrium beliefs $\hat{\omega} = \omega$. By equilibrium hypothesis, the Principal's first order condition can be written,

$$\begin{aligned} 0 = \ & \gamma(x, v(x,\theta^*,\omega))u_{p1}(l(\theta^*,\omega,r),\omega,D)l_1(\theta^*,\omega,r) + u_p(l(\theta^*,\omega,r),\omega,D)\times \\ & \left[\gamma_1(x, v(x,\theta^*,\omega)\frac{dx}{d\theta} \ \gamma_2(x, V(x,\theta^*,\omega))\left[V_1(x,\theta^*,\omega)\frac{dx}{d\theta} + V_2(x,\theta^*,\omega) + V_3(x,\theta^*,\omega)\left[\mu^{-1}(\theta^*)\right]'\right]\right] \end{aligned}$$

Noting that $\theta^* = \mu(\omega)$ and $\left[\mu^{-1}(\theta)\right]' = \left[\mu'(\omega)\right]^{-1}$ we can re-write the optimal equilibrium strategy in the form of the following ordinary differential equation,

$$\mu'(\omega) = -\frac{u_p(l(\mu(\omega),\omega,r),\omega,D)\gamma_2(x, V(x,\mu(\omega),\omega))V_3(x,\mu(\omega),\omega)}{\Delta} \tag{4}$$

where,

$$\Delta \equiv \gamma(x, V(x, \mu(\omega), \omega) u_{p1}(l(\mu(\omega), \omega, r), \omega, D) l_1(\mu(\omega), r, \omega) + u_p(l(\mu(\omega), \omega, r), \omega, D) \times \left[\gamma_1(x, V(x, \mu(\omega), \omega)) \frac{dx}{d\mu(\omega)} + \gamma_2(x, V(x, \mu(\omega), \omega)) \left[V_1(x, \mu(\omega), \omega) \frac{dx}{d\mu(\omega)} + V_2(x, \mu(\omega), \omega) \right] \right]$$

Note that the numerator is (4) corresponds to the marginal gain from signalling a higher type and is strictly positive. A sufficient condition for μ to be monotone increasing is simply $0 > \Delta$.

Finally, define $\bar{\omega}$ by the indifference condition,

$$\gamma(x^*(\mu(\bar{\omega})), V(x^*(\mu(\bar{\omega})), \mu(\bar{\omega}), \bar{\omega})) u_p(l(\mu(\bar{\omega}), \bar{\omega}, r), \bar{\omega}, D) \tag{5}$$
$$=$$
$$\gamma \left(x^*(1), V \left(x^*(1), 1, \int_{\bar{\omega}}^{1} t dF t \right) \right) u_p(l(1, \bar{\omega}, r), \bar{\omega}, D)$$

Investor interim beliefs follow directly from Bayes' rule. □

Proof of Proposition 2

Proof. For the first part, note that as $r \to 0$, $l(\theta, \omega, r)$ increases but cannot exceed ω. Thus the numerator in (4) is increasing in r but bounded above.[17] At the same time,

$$\lim_{r \to 0} \Delta = 0$$

implies $\mu'(\omega) \to \infty$ and $\bar{\omega} \to 0$. The second part follows from the characterization of beliefs in Proposition 1. □

Proof of Proposition 3

Proof. Recalling that investors update beliefs continuously throughout the game, consider the difference between investor beliefs prior to, and immediately following, the announcement of θ. Prior to the announcement, beliefs are,

$$V^{t-1}(\mathbb{E}_\Omega[\mathbb{E}_\Theta[\mathbb{E}[x|\theta, \omega]|\omega]], \mathbb{E}_\Omega[\mathbb{E}[\theta|\omega]], \mathbb{E}_\Omega[\omega]) \tag{6}$$

[17] γ_2 and V_2 remain unaffected by changes in r.

or by the law of iterated expectations,

$$V^{t-1}(\mathbb{E}[x], \mathbb{E}_\Theta[\theta], \mathbb{E}_\Omega[\omega]) \tag{7}$$

Similarly, beliefs following the revelation of θ are,

$$V^t(x^*, \theta^*, \omega) \qquad \text{if } \bar{\omega} > \omega \tag{8}$$

$$V^t(\mathbb{E}[x|\theta = 1, \omega \in [\bar{\omega}, 1]], 1, \mathbb{E}[\omega|\omega \in [\bar{\omega}, 1]] \qquad \text{otherwise.} \tag{9}$$

As $r \to 0$, $\bar{\omega} \to 0$ by Proposition 2. Thus the change in beliefs approaches,

$$V^{t-1}(\mathbb{E}[x], \mathbb{E}_\Theta[\theta], \mathbb{E}_\Omega[\omega]) - V^t(\mathbb{E}[x|\theta = 1, \omega \in [0, 1]], 1, \mathbb{E}[\omega|\omega \in [0, 1]]$$

or,

$$V^{t-1}(\mathbb{E}[x], \mathbb{E}_\Theta[\theta], \mathbb{E}_\Omega[\omega]) - V^t(\mathbb{E}[x|\theta = 1], 1, \mathbb{E}_\Omega[\omega]$$

But note that by Lemma 1, as $r \to 0$, it must be $\frac{dx}{d\theta} \to 0$ since $l_1 \to 0$. Finally since θ has direct bearing on policy, it must be that $V(x, \theta, \omega) = V(x, 1, \omega)$ for any pair (x, ω). Finally we can write the difference in beliefs as $r \to 0$,

$$V^{t-1}(\mathbb{E}[x], 1, \mathbb{E}_\Omega[\omega]) - V^t(\mathbb{E}[x], 1, \mathbb{E}_\Omega[\omega]) = 0 \tag{10}$$

□

Appendix B: Background on Quota Reforms

Above we provided evidence of the substantive significance of the estimated shifts in risk premia by comparing the results above with those corresponding to changes in the IMF's formal governance structure. Here we provide additional background on the Quota and Voice and Quota and Government Reforms, two packages of IMF reform intended to increase the representation of emerging economies in Fund decision making. These reforms reflect a growing consensus that the representation of countries such as Brazil, China, and Mexico have not kept pace with their growing contribution to the global economy. As noted above, we consider two milestones in the reform process: first, the entry into force of the Voice and Participation Amendment and second, passage of legislation by the United States Congress approving reforms to IMF quota allocations.

First, the Voice and Participation Amendment modified Article XII, Section 5(a), changing how basic votes are allocated among IMF member states. This shifted vote allocation from a fixed number of 250 basic votes to 5.502% of total voting power. The amendment was meant to ensure that "the ratio of total basic votes in total voting power [was] not eroded by quota increases."[18]

Second, in order for the Quota and Government reforms to take effect, the IMF requires official approval from at least three fifths of IMF member countries, accounting for at least 85% of the total vote share. Since the U.S. alone holds over 15% of total votes, its authorization of the reforms was a key prerequisite for enactment. While the Obama administration included requests for authorization for the reforms in its budget requests for several years running, up until 2015 there was little sign of progress (Nelson and Weiss, 2015). At least as late as October 2015 U.S. Treasury officials indicated that the rule would remain in place. Officials relented and agreed to remove the rule only at the 11th hour in time for last-minute inclusion of the necessary language in the 2016 appropriations legislation. Congressional leaders announced that authorization would be included on December 16, just two days before the bill was passed in Congress (Talley, 2016).

[18] See IMF Report SM/11/44, March 3, 2011 for more details.

Appendix C: Robustness to Alternative Estimation Strategies

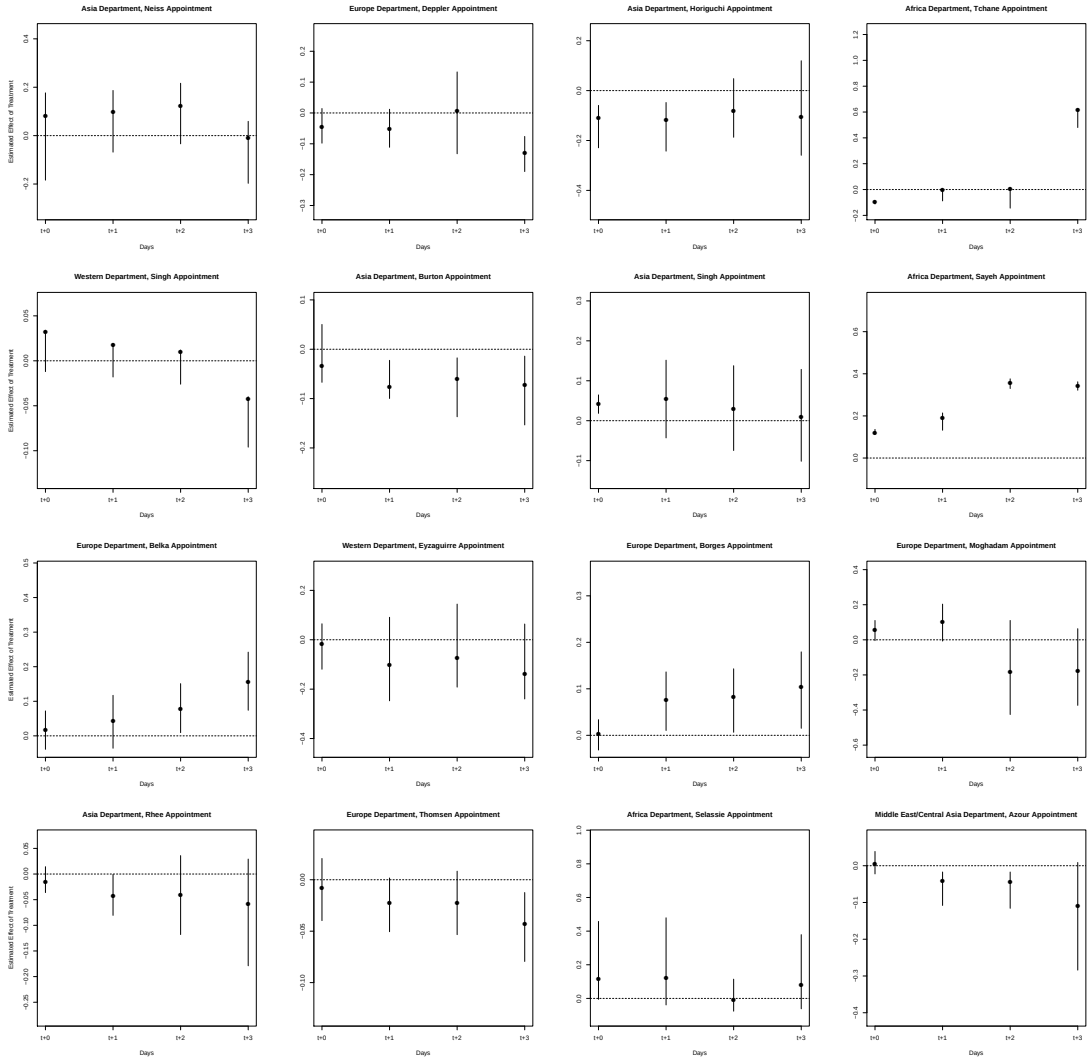

Figure 8: **Estimated Average Treatment on the Treated For Each Area Director Appointment.** Estimated effects are shown for a period of 3 days after an area department head appointment, with 95% bootstrapped confidence intervals. Here we use Covariate Balance Propensity Score Weighting.

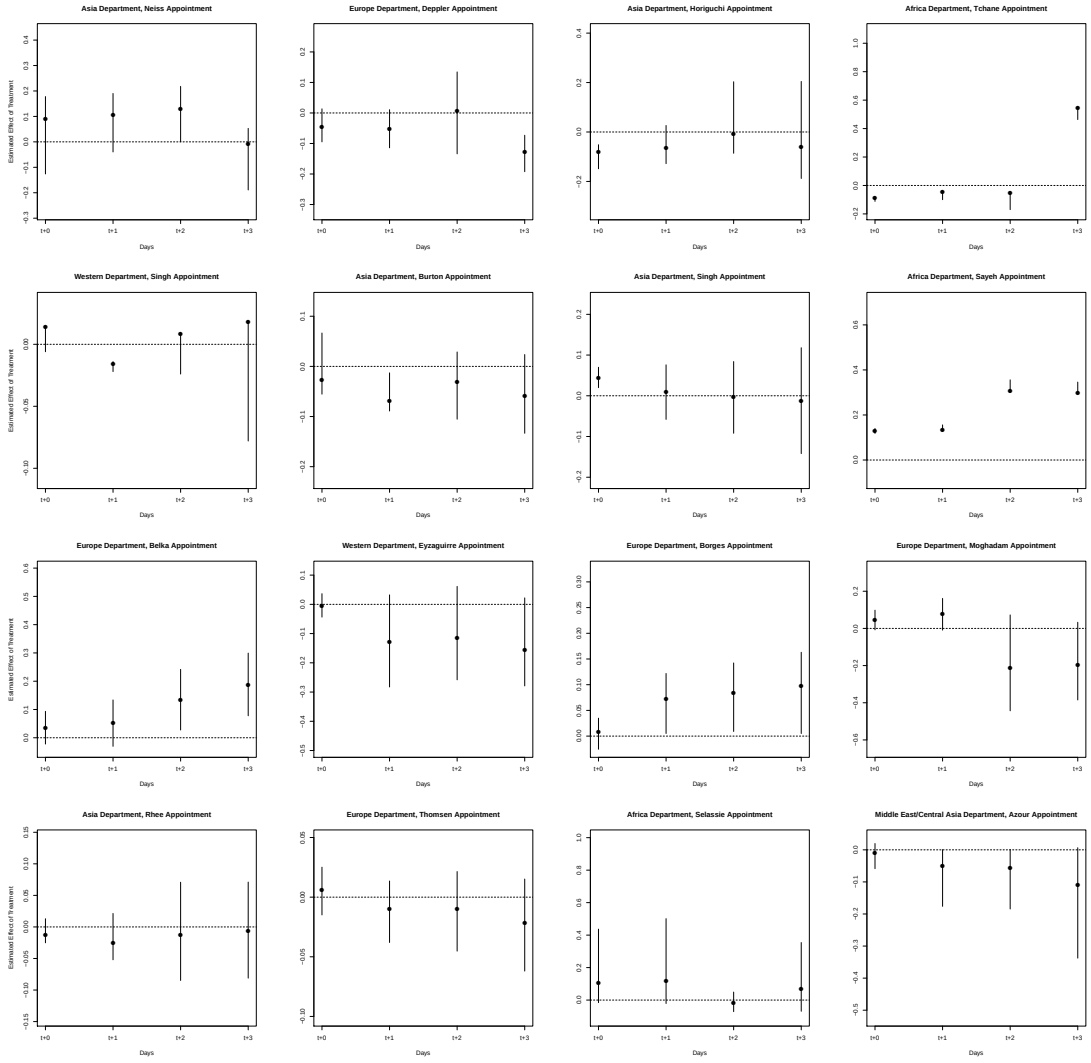

Figure 9: **Estimated Average Treatment on the Treated For Each Area Director Appointment.** Estimated effects are shown for a period of 3 days after an area department head appointment, with 95% bootstrapped confidence intervals. Here we use Mahalanobis Distance Matching.