Aid, Reform, and Interest Groups

Heckelman, Jac C and Wilson, Bonnie

Wake Forest University, Saint Louis University

July 2023

AID, REFORM, AND INTEREST GROUPS*

JAC C. HECKELMAN
Wake Forest University
Department of Economics
Email: heckeljc@wfu.edu

BONNIE WILSON
Saint Louis University
Department of Economics
Email: bonnie.wilson@slu.edu

Abstract

Foreign aid is often granted to encourage market-oriented reform. It is not clear that this approach to reform has been effective. We seek to understand this seeming failure of aid. We ask whether and how political markets for institutions have influenced the impact of aid allocations on reform, and we explore the extent to which the impact of aid on reform is conditional on the influence of a particular player in those markets - special interest groups. In a panel of 92 aid-receiving nations over four decade-long time periods, for several measure of reform, we find evidence that the aid-reform relation is conditional on the influence of interest groups. We find that only under relatively extreme and rare conditions has aid been positively associated with reform. Mostly, we find that aid has been associated with reform backsliding. The effects are economically meaningful in magnitude.

Keywords: aid, reform, institutions, special interest groups

JEL classification: O1, O19, P11

* In honor and memory of Stephen Knack. Steve's scholarship advanced our understanding of institutions, development, and public sector impediments to prosperity and self-actualization for the poorest among us. His excellence is missed. We thank participants in the 2022 Public Choice Society meetings and seminar participants at UMass Lowell for helpful remarks, especially Daniel Bennett, Christian Bjørnskov, Dennis Coates, and Shakil Quayes.

I. Introduction

Foreign aid is often granted to poorer nations in order to encourage market-oriented reform. Unfortunately, empirical work to date has not clearly established that this approach to reform has been effective. We seek to understand this seeming failure of aid and pose two research questions: (1) Is the relation between aid and reform conditional on the influence of special interest groups? (2) If so, do the conditional marginal effects of aid imply that aid promotes reform under some conditions even while it is ineffective or leads to perverse backsliding on reform under other conditions? The first question hypothesizes the existence of a political market for institutions that mediates the relation between aid and reform, and interprets aid as a shock that upsets an existing equilibrium. The second question hypothesizes that prior literature may have failed to reveal a clear positive relation between aid and reform because it focused on average marginal effects that masked important conditional marginal effects.

Foreign aid has long been used as a means to the ends of economic growth and better lives for those living in poorer nations. For example, aid has been used to fund public goods and educational opportunities in an attempt to make workers more productive. In the decades following widespread allocations of aid, research generally found that aid had not promoted growth. Scholars began turning away from the question "Does aid work?" and began asking "Why doesn't aid work?" Burnside and Dollar (2000) produced a landmark study indicating aid can be effective in the right environments. In particular, they found that aid significantly increased growth in those nations with good institutions and policies already in place. These findings implied that improving institutions would not only have a direct impact on growth, but also indirectly increase growth by making aid more effective.

On the heels of the Burnside and Dollar (2000) findings, more emphasis was placed on the granting of aid in exchange for market-oriented reform, such as building courts and training judges to establish and protect property rights, stabilizing currencies to limit rampant inflation, and reducing government reliance on inefficiently high taxes and tariffs. In practice, it is not clear these efforts to exogenously impose market-oriented reform have been successful. While

there is some evidence that aid is associated with reform, there is also evidence that aid is ineffective at producing institutional change. Other evidence even suggests that aid has had perverse effects and led to less, not more, market-orientation in institutions.

While much of the existing literature offers reason to question the effectiveness of aid, a subset that considers the cause(s) of the failure of aid to induce reform has identified more positive outcomes. This work examines conditions under which aid intended to spur institutional reform may be more or less effective and thus identifies ways aid might be better targeted. Kilby (2005), Dutta and Williamson (2016), and Annen and Knack (2021) all identify evidence of positive effects of aid on reform, conditional, respectively, on the initial extent of regulation, political institutions, and past reform. We examine an additional source of conditionality. In particular, we ask whether and how political markets for institutions have influenced the impact of aid allocations on reform, and we explore the extent to which the impact of aid on reform is conditional on the influence of a particular player in those markets - special interest groups.

In order to think about the potential impact of aid on reform, one should first ask: What prevents spontaneous market reform, in the absence of aid? On some accounts, market-oriented reform does not take place because it is costly, literally. The building and maintenance of the state capacity necessary to support market-oriented institutions require real resources. From this perspective, aid relaxes a state's budget constraint and jump-starts reform. In this context, aid works. In contrast, Heckelman and Knack (2008) note that adjustment lending may help reform-minded politicians buy-off powerful groups that are opposed to reform. This political-economy perspective understands aid not as a simple relaxing of a budget constraint that allows reform, but rather as a shock to an existing market for institutions. In such markets, politicians and special interests engage in exchange over policy, regulation, and other institutions (see, e.g., Stigler 1971; Yandle 1983; McChesney 1987). This process of exchange generates an equilibrium set of institutions. Viewed in this light, aid is a shock to the existing market for institutions. The impact of aid will therefore depend on conditions in this market,

and may be positive, null, or perverse, depending on the preferences and relative bargaining strengths of market participants.

The mixed empirical evidence on the impact of aid in the literature is consistent with the political-economy perspective and offers indirect evidence of a market for institutions. We more directly explore the existence of such markets and the impact of aid in their presence. First, we examine whether the effectiveness of aid is conditional on interest group activity, as one would expect if there is a market for institutions. For some measures of reform, we find evidence of such conditionality. Second, we examine the conditional marginal effects of aid, for various levels of interest group activity. For some measures of reform and when there is little market-orientation in institutions, we find evidence that the marginal effect of aid on reform is positive and increasing in interest groups. However, this effect applies at levels of groups and institutions that are observed for only a very small number of observations. Mostly, we find evidence that the marginal effect of aid on reform is negative and increasing (in magnitude) in groups, with larger effects the more market-oriented are institutions. These perverse effects of aid on reform apply to most of the observations in the sample and are economically meaningful in magnitude.

Our prior expectation was that the data would reveal substantial ranges over which the conditional marginal effects of aid were positive and substantial ranges over which effects were negative, offering some potential for better aid targeting. What the data actually reveal is very little opportunity for better aid targeting conditional on interest groups. Moreover, the findings suggest that most aid has been associated with perverse effects on institutional reform, consistent with the presence of political markets for institutions and the influence of special interest groups in that market.

II. Aid, Growth, and Reform

The OECD began measuring resource flows to developing countries in the early 1960s, and "Official Development Assistance" (ODA) was later defined as "government aid designed to promote the economic development and welfare of developing countries." By the mid-1990s,

after some two decades of recorded ODA flows, a consensus emerged that foreign aid had not delivered on its promise to enhance growth and reduce poverty in developing countries. The narrative was at least temporarily changed by Burnside and Dollar (2000), who offered evidence that aid actually had been effective in those countries with good policy (fiscal, monetary, and trade) in place. Although the robustness of these results was immediately challenged (e.g. Dalgaard and Hansen 2001; Easterly, Levine, and Roodman 2004) and a number of aid-growth findings were identified as fragile (Roodman 2007), Arndt, Jones, and Tarp (2016) point out that more recent aid-growth studies have identified a positive impact of aid on growth over extended periods of time.

In contrast to the recent aid and growth literature, much of the literature on aid and institutional reform offers less evidence for optimism about aid's ability to affect change. Using annual data and the Heritage index of economic freedom to measure reform, Knedlik and Kronthaler (2007) identify a positive relation between aid and reform, but an inverse relation between IMF credit and reform. Taking a long-run view and using the Fraser index of economic freedom, Heckelman and Knack (2008) find that aid perversely slowed market-oriented reform on average over the period 1980-2000.[1] Using a panel based on intervals of five years, Young and Sheehan (2014) also find a perverse impact of aid on reform, particularly with respect to human and property rights as well as openness to international trade.

A key challenge to identifying the causal effect of aid on reform is the likely endogeneity of aid due to both omitted variables and simultaneity. A variety of techniques have been used to address this problem, including measuring aid on only the first half of the reform period and 2SLS (Heckelman and Knack 2008), IV (Heckelman and Knack 2009; Dutta and Williamson 2016), IV with fixed effects (Young and Sheehan 2014; Dutta and Williamson 2016) and System GMM (Dutta and Williamson 2016). Using a unique instrument that exploits lags between

[1] In a follow-up study of aid, reform, and medium-run growth, Heckelman and Knack (2009) find no significant effect of the aggregated freedom index. However, using hedonic estimates from a growth model, which acknowledges that not all areas of economic freedom necessarily promote growth, they infer that aid has overall contributed to a growth-enhancing institutional environment by improving institutional scores in those areas that are positively correlated with growth and reducing scores in those areas that are inversely correlated with growth.

loan approvals and disbursements, Dzhumashev and Hailemariam (2021) reveal more positive findings than most others. In particular, they identify a positive relation between aid and the levels of several aspects of market-oriented institutions. Pavlik, Powell, and Young (2022) also advance the literature with a focus on the importance of a plausible identification strategy. In particular, they employ matching methods. In contrast to Dzhumashev and Hailemariam, they find no meaningful causal impact of aid on reform.

In certain ways, given that the recent literature examining aid and growth identifies positive effects, one might not be especially concerned that aid does not appear to be clearly associated with reform or that it might produce movement away from market-oriented institutions. There are multiple paths to growth and economic development, not all of which require market-liberalizing policy reform. However, Grier and Grier (2021) examine cases of reform using the index and find them to be efficacious. In particular, they find that countries with sustained reform were some 16% richer after 10 years. If the findings in the aid-and-reform literature are correct and it is true that poor nations have gotten very little reform for the aid buck, it may be that reform is simply something that cannot be well or reasonably addressed via the means of aid, and that aid efforts should thus be directed elsewhere. It might also be the case though that aid is only conditionally effective for reform, similar to the conditionality of aid's impact on growth uncovered by Burnside and Dollar (2000), such that the average marginal effects revealed in much of the literature mask important opportunities. As noted above, the subset of literature that examines conditional rather than average marginal effects of aid on reform has decidedly more positive findings. Kilby (2005) finds that aid directed towards more heavily regulated economies leads to deregulation. Dutta and Williamson (2016) identify conditionality with respect to political institutions, finding that aid advances reform in democracies but may have perverse reform-backsliding effects in autocracies. Annen and Knack (2021) find that policy-selective aid, aid that is granted in response to past reform, causes reform, at least for those countries that lack access to non-selective financial flows from China or rents from natural resources. We explore an additional source of conditionality. Specifically, we examine

whether the impact of aid is conditional on the influence of special interest groups, groups that may have a "say" in the reform process as players in political markets for institutions.

III. Groups and the Market for Institutions

À la Stigler's (1971) theory of economic regulation, we suppose that the existing set of economic institutions in a nation may be understood as the outcome of a process of exchange between politicians and special interest groups. Politicians supply institutions demanded by groups, if the "price" is right. The potential for failure in this political market implies that the resulting institutions may not be efficient and may serve narrow rather than broad interests.[2] In contrast to the view that firms and industries are burdened by regulations that address market failure or other societal objectives, on this account, special interests acquire regulation that is designed to benefit them and that is a net burden to society overall (Olson 1982). As further noted by Yandle (1983), we should expect an "equilibrium" state in the market for institutions to persist unless and until there is some outside shock to the system. We interpret aid as just such a shock, intended to upset the status-quo and generate reform.

What is the nature of the political market for institutions and how might an aid shock manifest? To address this question, Svensson (2000) offers a game-theoretic rent-seeking model that demonstrates how aid may lower the provision of public goods and that motivates an empirical analysis of the impact of aid on corruption. Annen and Knack (2021) also offer a game-theoretic model, but examine how policy selectivity by donors may interact with the global aid budget and conduct an empirical analysis of policy-selective aid and reform. Hodler (2007) incorporates a rent seeking contest into the Barro (1990) growth model to explore how institutions that limit rent seeking may determine aid effectiveness. We do not model any particular strategic interactions between potential players in the political market for institutions or presume any particular model of growth. We simply hypothesize and test for (1) the existence of such a market and (2) a role for special interest groups in it. In that context, a

[2] The political failures cited by Stigler are those of democracy, while the governments of many aid-receiving nations are more autocratic than democratic. We presume the political failures of those autocracies are no less significant when it comes to institutions, even if the sources of failure may differ.

description of the potential state of willingness or unwillingness of politicians to supply reform and of the potential preferences of groups regarding reform suffices to construct an empirical model to estimate and to sign its key coefficients.

In some nations, the political market for institutions may feature reform-willing politicians who face opposition to change by interest groups. In others, reform-willing interest groups may face reform-reluctant politicians. Consider first the possibility of reform-willing politicians and reform-opposed interest groups. As Heckelman and Knack (2008) note, a key factor inhibiting aid as a tool of reform is that recipient governments may face opposition from politically powerful groups, groups whose rent-seeking activities have shaped existing regulations, policies, and other institutions. These vested special interests benefit from the status quo and face uncertainty when their environment changes. They are thus likely to be opposed to the new ideas, new polices, and new processes associated with reform (Olson 1982). In this context, while aid may be intended to help a reform-willing government survive, or buy off, their opposition and thus advance reform, the effectiveness of aid is likely conditional on the extent of such opposition.

Imagine, for example, two nations, both of which are granted the same amount of aid. Suppose further that the first nation is characterized by a relatively small amount of rent-seeking relative to the second. All else equal, a dollar of aid in the first nation should be expected to generate more reform, because fewer resources will need to be allocated towards buying-off or mollifying special interest groups who are opposed to reform and wish to maintain the status-quo. The impact of a given amount of aid is thus conditional on the extent of resistance to reform by interest groups. If one fails to account for this conditionality and examines only the average marginal effect of aid, positive marginal effects may be missed. In other words, the null and perverse average marginal effects identified in the literature to date may not imply that aid does not "work." They may in fact mask positive conditional marginal effects.

Next, consider the possibility of reform-reluctant politicians and reform-supporting inter-

est groups. Politicians may be reform-reluctant, for example, if they face populist pressures from voters who are skeptical of market-oriented institutions. Alternatively, à la McChesney (1987), politicians may not wish to advance reform because they benefit from ongoing rent-protection efforts of interest groups or opportunities to credibly engage in rent-extraction threats. Why would groups support reform? Olson (1982) argues that groups are likely to engage in redistributive rather than productive efforts and are expected to oppose reforms that would limit rent-seeking opportunities. However, Doner and Schneider (2000) argue that in especially weak institutional environments, the narrow interests of groups are aligned with the broader social interest of efficiency, such that groups will support market-oriented reform. On this account, when institutions are sufficiently weak, the "pie" may be sufficiently small that groups actually gain more from efforts to enhance its size than from efforts to redistribute. The findings of Heckelman and Wilson (2013) are consistent with Doner and Schneider's claim. In this context, aid may spur groups to increase lobbying efforts in favor of reform. The greater this pressure, the more likely a given amount of aid would generate reform. As in the case of reform-willing politicians and reform-opposed groups, this possibility suggests that aid is conditional on groups. In addition, it indicates that the desires of groups depend on the quality of institutions. If institutions are sufficiently weak, groups are supportive of reform. If institutions are already sufficiently strong, groups favor the status quo and seek to block further reform efforts.

In sum, if a Stiglerian market for reform exists, we expect the impact of aid on reform to be conditional on interest group activity. Further, we expect the conditional effect of aid on reform to be positive and diminishing in groups if institutions are relatively strong and negative and diminishing (in magnitude) in groups if institutions are sufficiently weak. The absence of such a market implies effects are not conditional on groups.

IV. Model, Data, and Methods

In order to explore whether the relation between aid and market-oriented reform is conditional

on the influence of groups, we estimate the following model,

$$\text{Reform}_{i,t} = \alpha + \beta_1 \,\text{Aid}_{i,t} + \beta_2 \,\text{Aid}_{i,t} \cdot \text{Groups}_{i,t} + \beta_3 \,\text{Aid}_{i,t} \cdot \text{Groups}_{i,t} \cdot \text{Inst}_{i,t} + \boldsymbol{\beta_4}' \boldsymbol{X}_{i,t} + \epsilon_{i,t}\,, \quad (1)$$

where Reform is a measure of institutional reform; Aid is a measure of development assistance; Groups is a measure of interest group influence; Inst is a measure of institutions; $\boldsymbol{X}$ is a vector of additional explanatory variables; α, β_1, β_2, and β_3 are parameters to be estimated and $\boldsymbol{\beta_4}$ is a vector of parameters to be estimated; ϵ is an error term; i indexes countries and t indexes time.

Differentiating with respect to Aid implies that the marginal impact of aid on reform is

$$\frac{\partial \text{Reform}}{\partial \text{Aid}} = \beta_1 + \beta_2 \,\text{Groups} + \beta_3 \,\text{Groups} \cdot \text{Inst}\,.$$

In the absence of a political market for institutions, $\beta_2 = \beta_3 = 0$. In this case, the marginal impact of aid on reform does not depend on groups and reduces to β_1. If aid "works," then $\beta_1 > 0$. If, as we hypothesize, there is a market for institutions and groups oppose reform if institutions are sufficiently strong and favor reform if institutions are sufficiently weak, then $\beta_2 > 0$ and $\beta_3 < 0$.

Our data consist of an unbalanced panel of a maximum of 259 observations on a maximum of 92 countries and four time periods, 1975-1985, 1985-1995, 1995-2005, and 2005-2015. Variables used are either changes over each period (e.g., the value of the variable in 1985 minus the value of the variable in 1975), averages over each period, or initial values of the period.

Following much of the aid-reform literature, we measure Reform as the change in the Fraser Institute's index of economic freedom. The extent of market-orientation in institutions, Inst, is captured by the initial level of the index. Fraser tracks five components of economic freedom - size of government, legal system and property rights, sound money, freedom to trade internationally, and regulation (of credit markets, labor markets, and business). We examine reform as measured by the change in the overall index as well as in each of these individual components.

Both the nature of reform, the economic freedom data, and the interest group data drive our choice of time frame. In contrast to our approach of examining decade-long periods (1975-1985, 1985-1995, 1995-2005, and 2005-2015), a number of contributions to the aid and reform literature have examined five-year periods. Knedlik and Kronthalter (2007) and Annen and Knack (2021) examine annual observations. Shorter time periods have the benefit of increasing the number of observations and allowing system-GMM as an empirical strategy for identifying an aid-reform relation. However, if an aid-reform relation actually emerges slowly, five-year or one-year periods may be too short to capture effects. The findings of Arndt, Jones, and Tarp (2016) suggest that this is the case for the aid-growth relation, which may only emerge over long time horizons of multiple decades. One might expect any aid-reform relation to occur more quickly than any aid-growth relation, as reform is a key catalyst for growth. Five-year periods nonetheless seem to us too short, especially given the nature of both reform and the reform data. Some formal reform may occur relatively quickly, within in, say, a span of a few years. Reform that "matters" though, often requires a corresponding transformation in complementary informal norms and understandings. This complementary transformation likely takes time to fully emerge. Actual "full" reform, reform of both formal and informal institutions may well be fairly slow. In key regards, the data underlying the Fraser economic freedom index may capture at least some of the slower aspects of reform. In particular, a number of the components of the index are based not on measures of actual laws and regulations, but on survey questions that reflect respondents' impressions, experiences, or expert judgments, as the market-oriented nature (or lack thereof) of an institutional environment can be difficult to directly observe. These responses likely reflect both the formal and informal aspects of reform. In addition, as noted below, our interest group data are only available at six points in time over the period 1973 - 2002. If we were to use time periods of five-years or less, there would be time-period "holes" in the sample or we would have to interpolate observations for groups.

We measure Aid using net "official development assistance" flows as a share of gross national income from the World Bank's *World Development Indicators*. These data consist

of disbursements of loans made on concessional terms (net of repayments of principal) and grants by official agencies of the members of the Development Assistance Committee (DAC), by multilateral institutions, and by non-DAC countries to promote economic development and welfare in countries and territories in the DAC list of ODA recipients. The measure includes loans with a grant element of at least 25 percent (calculated at a rate of discount of 10 percent).[3]

We measure interest group influence, Groups, using a count of the number of interest groups in a country. The primary data source for these counts is the *World Guide to Trade Associations*. Aggregate group counts from the *Guide* have been used in a number of prior studies (see, for example, Murrell (1984), Heckelman (2000), Bischoff (2003), Heckelman and Wilson (2021)) that explore group formation determinants as well as the impact of groups on macroeconomic outcomes and monetary policy. The *Guide* has been published episodically and only six times, in 1973, 1980, 1985, 1995, 1999, and 2002. As a result, the counts used correspond to initial (rather than average) values for each period, and the counts do not line-up exactly with the start of each period. We use counts from the 1973, 1985, 1995, and 2002 editions, respectively, for the four time periods 1975-1985, 1985-1995, 1995-2005, and 2005-2015. Olson (1982) notes that competition between groups may increase as groups grow in number, even while he supposes that collective action problems preclude sufficient group formation to ensure efficient outcomes through comprehensive bargaining. In this case, one would expect the marginal influence of groups to be diminishing. We therefore use the natural log of the group counts (plus 1, since some countries have zero groups) in the analysis instead of the raw group counts.

Summary statistics for each of the six measures of reform and the associated level of market-orientation in institutions as well as for aid and the group counts are reported in Table 1. The mean of each measure of reform is positive, indicating movement towards more market-oriented institutions on average in the sample. The magnitude of movement on average

[3] We note that there are no observations in these data that take the value 0. Net flows are recorded as either positive, negative, or missing values. We further note that while most studies feature aid as a share of gross national income, some examine aid per capita.

is modest. The level of each measure of market-orientation in institutions can range from 0 to 10. The average change in these levels ranges from 0.397 (for regulation) to 0.855 (for sound money). The minimum and maximum values of reform indicate substantial movement away from and towards market-oriented institutions in some cases. For example, in the case of international trade, one country (Iran) dropped 8.919 points during a period. Another country (Argentina) gained 7.021 points.

The mean level of aid as a share of GDP is just under 5%, with values that range from -0.05% to 34.5%. In the lower portion of Table 1, the distribution of the group counts is indicated. There are substantial numbers of observations in each of the ranges above the minimum.

Control variables included in all specifications are drawn primarily from Heckelman and Knack (2008). The initial value of the economic freedom index (or one of its components) is included, as reform (the change in the index) may depend on the existing extent of market-orientation in institutions. The initial value of a measure of ethnic fractionalization is included, as diversity has been tied to reform. Two measures related to political institutions are included, the change in an index of democracy as well as the level of the index's initial value, as political institutions may influence reform in economic institutions. The initial values of real GDP and population size as well as the average annual growth rate of real GDP per capita are also included. Finally, Groups is included, as it is common practice to allow for lower order effects in models with interactions, to avoid the possibility that an actual lower order effect gets captured by the interaction term.

We do not include country fixed effects in the model. The inclusion of country fixed effects in models estimated using cross-country panel data is quite common, in order to address potential omitted variable bias. It is less commonly acknowledged that this statistical trick is not a free lunch. It comes with a bias-variance trade-off. Namely, all the between-unit variation is discarded and only within-unit variation over time is used to explain outcomes. In the context of aid and reform, while there is variation within countries over time worth

explaining, our research question primarily asks whether and why the impact of aid may be different across countries. In other words, we mainly have a between-unit question and require between-unit variation.

We estimate the model both without and with time fixed effects. Although we may lose some within-unit variation over time that is relevant to our research question, bias due to common macroeconomic or aid-environment shocks could be meaningful and the bias-variance trade-off in this case seems worthwhile.

As earlier noted, endogeneity of aid with respect to reform is possible due to simultaneity. As a result, we estimate the model using IV (in addition to OLS). We treat aid as well as the interactions terms that include aid as endogenous.[4]

Simultaneity between aid and reform is possible because aid grantors may adjust aid allocations based on whether or not past grants proved effective. In principle, the direction of this response and thus of the associated bias is unclear a priori. If aid produces reform, more aid might be forthcoming, or a grantor may decide that more aid is not needed. If aid does not produce reform, less aid might be forthcoming, or a grantor may decide that even more support is needed. In practice, Annen and Knack (2021) identify a positive impact of reform on aid, suggesting that we might anticipate a comparison of OLS and IV estimates to indicate the OLS estimates are upward biased.

For the IV estimation, the perennial challenge is the identification of valid instruments.[5] A number of novel external instruments for aid have been proposed, including a UN voting record indicator that captures how often a nation votes in line with the average G7 country (Barro and Lee 2005; Dreher and Sturm 2012), a dummy variable for whether a nation has a temporary seat on the UN Security Council (Dreher, Eichenauer, and Gehring 2018; Kuziemko

[4] Nizalova and Murtazashvili (2016) show that the OLS estimate on an interaction term between an exogenous and endogenous variable can be consistent, suggesting the possibility that we might at least be able to draw inference based on the OLS estimates of the model's interaction terms. Unfortunately, as Bun and Harrison (2019) note, their result does not apply in the case of endogeneity due to simultaneity - a key concern in the context of aid and reform.

[5] System GMM would allow us to estimate the model in both levels and differences and would make available internal instruments. Unfortunately, our panel is relatively short, with a maximum of $t = 4$, and it is also unbalanced. The incidence of missing observations is such that very few observations are available if the data are differenced. As a result, we do not estimate the model using System GMM.

and Werker 2006), and an interaction of donor government fractionalization with a recipient country's probability of receiving aid (Dreher and Langlotz 2020). While others have used these instruments with seeming success, they did not prove relevant using our data, based on an examination of Sanderson-Windmeijer F statistics in the first-stage. Bun and Harrison (2019) point out that IV can remain a viable strategy even in the absence of relevant external instruments. In particular, as Kelejian (1971) demonstrates, a polynomial of exogenous regressors provides valid instrumental variables. We therefore model the reduced form for endogenous regressors (Aid, Aid·Groups, and Aid·Groups·EF) using a second-order polynomial that implies the following vector of excluded instruments:

$$[x_i^2 \quad x_j^2 \quad x_i \cdot x_j \quad x_i \cdot x_j^2 \quad x_i^2 \cdot x_j], \tag{2}$$

where x_i is GDP per capita or population level and x_j is Groups or Groups·EF. This approach yields a total of 16 instruments for the three endogenous variables. As it turns out, these internal instruments are far more relevant than the external instruments noted above. As further discussed below, instrument weakness may nonetheless be a concern. Following Bun and Harrison, we therefore report the Stock and Wright (2000) heteroskedasticity- and weak instrument-robust AR statistic to test the null hypothesis that the coefficients on the endogenous variables are jointly equal to zero. In addition, we report coefficient p-values from the wild restricted efficient bootstrap of Davidson and MacKinnon (2010), which is reliable with weak instruments.

We treat all the remaining control variables as exogenous. However, endogeneity or weak exogeneity are potential concerns with three of these controls - Growth, GDP, and the level of institutions. Reform (the dependent variable) is the difference between the end-of-period and beginning-of-period values of our various measures of the market-orientation of institutions (the Fraser economic freedom index and its five components). Growth is the average annual growth rate of real GDP per capita over each time period. To the extent that reform takes place in the early years of each period, it may cause growth in the later years of the period.

As a result, the coefficient estimate on Growth may be biased and the coefficient estimates of interest (on aid and the two aid interaction terms) may inherit this bias. Generally, we expect this bias to be positive, as reform is generally expected to increase growth. Both Heckelman and Knack (2009) and Heckelman and Wilson (2019) provide evidence that the regulation component of the economic freedom index may be negatively associated with growth. In the case of the regulation component of the economic freedom index, any inherited bias may thus be negative.

Both GDP and the level of institutions are measured as initial period values. Contemporaneous correlation with reform is therefore ruled out by construction. These controls may still be only weakly exogenous as reform in a country in one period may be associated with higher GDP and a higher level of institutions in a future period. In general, we again expect any associated bias in coefficient estimates to be positive. Since these are within-country effects over time, we expect that the findings with time fixed effects will avoid any bias that may affect the findings without time fixed effects.

V. A Market for Institutions

We first test the hypothesis that a market for institutions exists by examining whether the impact of aid on reform is conditional on groups and the extent of market-orientation in institutions. If such a market exists, we expect the coefficients on the aid interaction terms to be statistically significant and of the predicted signs ($\beta_2 > 0$ and $\beta_3 < 0$).

Findings are reported in tables 2a-2f. Each table corresponds to a different measure of reform as the dependent variable. The dependent variable for table 2a is the change in the overall economic freedom index. The dependent variable for the remaining five tables is the change in one of the five components of the overall index - government size (table 2b), legal structure and property rights (table 2c), sound money (table 2d), freedom to trade internationally (table 2e), and regulation (table 2f).[6] Each table features two panels. OLS

[6] In these tables, the initial value of the economic freedom index on the right-hand side corresponds to the component index used to measure reform.

estimates and p-values (in parentheses) are reported in the upper panel (a). IV estimates and p-values (in parentheses) are reported in the lower panel (b). For the IV estimates, two sets of p-values are reported. The second set are from the wild restricted efficient bootstrap. Also reported are Sanderson-Windmeijer F statistics on excluded instruments (to test for instrument relevance), Hansen J p-values (to test for instrument exogeneity), and Stock-Wright p-values (to test for joint significance of endogenous regressors). Results without fixed effects are reported in columns (1a) - (1b) and with time fixed effects in columns (2a) - (2b). Two specifications are considered. Specifications (1a) and (2a) include no interaction terms and serve as a point of comparison. Specifications (1b) and (2b) add a two-way interaction between aid and groups and a three-way interaction between aid and groups and the level of market orientation in institutions (corresponding to the dependent variable). Groups are included as a control in specifications (1b) and (2b). All remaining control variables (the level of market-orientation in institutions, real GDP, population, real GDP per capita growth, the level of democracy, the change in the level of democracy, a measure of ethnic fractionalization) are included in both specifications. To conserve space, coefficient estimates on controls are not reported.

Our two research questions are (1) Is there a market for institutions such that the relation between aid and reform is conditional on the influence of special interest groups? (2) If so, do the conditional marginal effects of aid imply that aid promotes reform under some conditions even while it is ineffective or leads to perverse backsliding on reform under other conditions? Before turning to these questions, we first examine whether there is evidence of an average marginal effect of aid on reform. In tables 2a - 2f, the OLS coefficient estimates on aid in columns (1a) and (2a) reveal almost no evidence of any relation between aid and reform. In contrast, the IV coefficient estimates on aid in columns (1a) and (2a) are all statistically significant (based on both standard and weak restricted efficient bootstrap p-values), all negative, and all substantially larger in magnitude than the OLS estimates.[7] The

[7] To the extent that the OLS estimates are biased by simultaneity between reform and aid, these findings suggest that any influence of reform on aid allocations is positive on average, indicating that aid grantors may reward reform success with additional aid.

Sanderson-Windmeijer F statistics are well above the common rule-of-thumb of 10 as a gauge of instrument relevance. Hansen J p-values indicate we cannot reject the null that instruments for Aid are exogenous.

Next, we turn to our first research question and examine whether there is a market for institutions such that the relation between aid and reform is conditional on the influence of special interest groups. The OLS coefficient estimates in columns (1b) and (2b) reveal evidence of such a market in the cases of changes in the overall economic freedom index (table 2a), in government size (table 2b), in sound money (table 2d), and in regulation (table 2f). In these cases, with the exception of one coefficient estimate (in the case of changes in sound money), the interaction terms are statistically significant and of the expected sign (positive in the case of the two-way interaction and negative in the case of the three-way interaction).

The IV results are largely similar to the OLS results, and also offer evidence consistent with a market for institutions that interacts with aid allocations in the case of changes in the overall economic freedom index (table 2a), in government size (table 2b), in sound money (table 2d), and in regulation (table 2f). In the case of changes in regulation, the coefficient estimates on the interaction terms are not individually significant, with the exception of the three-way interaction when there are no fixed effects. However, the weak-instrument-robust Stock-Wright test p-value implies that we cannot reject joint significance of the coefficient estimates on Aid, Aid·Groups, and Aid·Groups·Reg. The Sanderson-Windmeijer first-stage F statistics are all above 10 in the cases of changes in the overall economic freedom index (table 2a), government size (table 2b), sound money (table 2d), and regulation (table 2f). Notably though, in many cases they are not much larger than 10. As such, the polynomial-based instruments may be weak (especially in the cases of the interaction terms). Inference based on the the wild restricted efficient bootstrap may therefore be preferred, although leads to the same conclusions as inference based on the standard p-values. Also of note, in the two cases in which the first-stage F statistics are less than 10 (change in legal structure and property

This finding is consistent with Annen and Knack (2021), who find that policy improvements lead to more aid.

rights (table 2c) and in international trade (table 2e)) we do not find evidence of a market for institutions based on the coefficient estimates on the interaction terms. In the case of international trade though, we reject the joint hypothesis that the coefficient estimates on all three endogenous variables are equal to zero.[8]

VI. Conditional marginal effects

Finally, we turn to our second research question - Do the conditional marginal effects of aid imply that aid promotes reform under some conditions even while it is ineffective or leads to perverse backsliding on reform under other conditions? In tables 3a - 3d, we present the conditional marginal effects of aid for different levels of groups and of market-orientation in institutions. Specifically, we report all combinations of the minimum, mean minus standard deviation, mean, mean plus standard deviation, and maximum levels of these two variables. To limit the number of tables, we report findings based only on the IV estimates (panel b in tables 2a - 2f). As before, two sets of p-values are listed in parentheses, with the second set from the wild restricted efficient bootstrap. We omit separate tables for the legal structure and property rights as well as the freedom to trade internationally components of the economic freedom index. For these two measures of reform, while estimation by OLS produces some statistically significant conditional marginal effects, estimation by IV does not, with the exception of the case of zero groups.

The findings reveal that most of the conditional marginal effects of aid on reform as measured by change in the overall level of economic freedom, in government size, in sound money, and in regulation are statistically significant. In the case of changes in regulation, the only statistically significant effects are negative. The findings reveal both positive and negative statistically significant conditional marginal effects for changes in overall economic freedom,

[8] We note that the IV coefficient estimates on Aid are uniformly negative in each of the (1b) and (2b) columns, though only sometimes statistically significant. In these specifications, the Aid coefficient represents the effect of aid on reform when there are zero groups. The negative sign is inconsistent with our priors. In particular, we anticipated that in the absence of groups, aid would be effective, such that $\beta_1 > 0$ in our full specification that includes the interaction terms. Implicitly, we assumed that aid grantors would have sufficient information to infer when politicians were reform-willing, and would not, on average, grant aid absent political will or some other reform-supportive coalition. The negative coefficient estimate on Aid suggests the possibility that aid grantors struggle to discern such conditions. However, there are only five observations in our sample which have zero groups.

in government size, and in sound money. Instances of positive, statistically significant effects occur only for the lowest levels of market-orientation in institutions and typically when the number of groups is relatively large. In the case of changes in the overall economic freedom index and with no time fixed effects, the estimated positive, statistically significant conditional marginal effects apply to roughly 10 observations and nine countries (Argentina, Bangladesh, Brazil, Egypt, Nicaragua, Nigeria, Peru, Uganda, and Zimbabwe). With time fixed effects, there are no actual observations that fall into the range of estimated positive, statistically significant conditional marginal effects. In the case of the sound money component of the index and with no time fixed effects, three observations fall into the range of estimated positive, statistically significant conditional marginal effects. All three of these observations are for Brazil. In no other cases do any observations fall into the range of estimated positive, statistically significant conditional marginal effects.

As noted earlier, the coefficient estimates may reflect some inherited bias due to simultaneity between reform and growth. In the case of reform as measured by changes in the overall economic freedom index and in the government size and sound money components of the index, we expect such bias to be positive. As such, in these cases, the already small range of positive conditional marginal effects may be even smaller, and the magnitude of the negative marginal effects even larger. The estimates without time fixed effects may also reflect inherent bias due to weak exogeneity of GDP and the level of market-orientation in institutions. In the cases of changes in the overall index, the government size component, and the sound money components, the negative coefficient estimates are generally larger in magnitude when fixed effects are used, consistent with positive bias in the non-fixed effects estimates. The opposite is true in the case of changes in the regulation component of the index, consistent with findings in the literature that regulatory reform may decrease growth.

The magnitude of the estimated negative effects is arguably large. Consider, for example, reform as measured by changes in the overall economic freedom index (table 3a), and the fixed effects results. At the mean of groups and the mean of economic freedom, the conditional

marginal effect of aid is -0.127. The standard deviation of aid is 6.129. A one standard deviation increase in aid is thus associated with a change in reform of -0.778 (-0.127*6.129). The sample mean of reform is 0.517, indicating that on average economic freedom is advancing in the sample. However, the results imply that a one standard deviation increase in aid leads to a backsliding in economic freedom that more than offsets that sample average advance in economic freedom. The effect is substantially larger at, say, the mean+sd of groups and the mean+sd of economic freedom (where the conditional marginal effect of aid is -0.234 instead of -0.127).

Overall, the findings suggest that aid intended to encourage market-oriented reform has in fact been largely associated with a perverse decline in the market-orientation of institutions. The exceptions to this rule have only occurred in countries in which institutions are the very least market oriented and in which there are relatively large numbers of interest groups. This finding is consistent with Doner and Schneider's(2000) claim that in especially weak institutional environments, groups support market-oriented reform. Mostly though, the findings suggest that groups oppose reform, and that they are not only successful at blocking reform, but that when they are relatively large in number and when institutions are most market-oriented, they are associated with substantive institutional backsliding.

VII. Concluding Remarks

The literature on aid and market-oriented reform has arguably failed to produce a substantial body of evidence that aid "works." One potential explanation for this seeming failure is that political economy effects impede reform efforts. Our conjecture in particular is that aid may sometimes fail because special interest groups favor existing policy privileges and oppose reform. Implicit in this conjecture is a presumption that a market for institutions exists as well as a presumption that when an allocation of aid shocks this market, the behavioral responses of market participants impede reform efforts. In this paper we sought first to explore the existence of a market for institutions in which interest groups condition the impact of aid on reform and second to estimate the conditional marginal impact of aid on reform.

The idea of a market for institutions is an old one. In a (26th of October 1780) letter to Andreas Holt, Adam Smith characterized his *Wealth of Nations* as a "violent attack" on mercantilism, a system of exchange in the sphere of the state in which interest groups curry favor with politicians in pursuit of monopoly privileges and other favorable regulations. The more modern incarnation of a market for institutions is rooted in Stigler's (1971) economic theory of regulation, which turns on its head the idea that regulation exists as an exogenously imposed policy response to market failure. Instead, Stigler asserts that regulation develops the same way as goods and services - via market processes, albeit in the sphere of the state rather than in commercial society - and that regulation thus exists to benefit both the regulated and the politicians who offer it in exchange for votes and resources. We find evidence of a market for institutions across several measures of institutional reform. The evidence is consistent with the claims that interest groups generally oppose reform, that groups favor reform when there is little to no market-orientation in institutions, and that when groups are sufficiently influential an allocation of aid can enliven them in ways that leads to a substantive perverse backsliding of economic liberalization.

With respect to the conditional marginal effect of aid, our prior hope when we began the project was that we would discover a substantial range of conditions (levels of market-oriented institutions and numbers of groups) over which aid has been positively associated with reform, even while we anticipated a substantial range of conditions over which aid has been negatively associated with reform. In other words, we were optimistic that aid has worked under certain, though not all, conditions. If a substantial range of conditions were associated with positive marginal effects, aid might be better targeted with positive effect in the future. Unfortunately, the findings reveal little scope for such targeting based on conditions in political markets and very few cases in which one might be optimistic that aid could generate reform.

It is increasingly recognized that efforts to impose democratic institutions in autocratic nations often fail (Knack 2004; Coyne 2007; Djankov, Montalvo, and Reynal-Querol 2008). Our findings suggest that efforts to impose market-oriented institutions warrant careful considera-

tion as well. To the extent that institutions themselves emerge via a process of exchange, they reflect an equilibrium of sorts as well as potentially complex underlying norms and traditions. Exogenous efforts to change them should perhaps be expected to fail, except under certain extreme and narrow circumstances. At the same time, it is notable that others, such as Kilby (2005), Dutta and Williamson (2016), Annen and Knack (2021), have identified conditions under which aid appears to have been effective.

We emphasize that evidence here that aid has failed to produce market-oriented reform, except under very limited conditions, should not be interpreted as a blanket failure of aid to advance development and the well-being of those living in poor nations. It might however encourage us to curtail grants of aid in exchange for market-oriented reform and to direct aid to more productive projects. The failure of aid to produce market-oriented reform should arguably also not discourage us from the general project of market-oriented reform. As Grier and Grier (2021) show, the potential for market-oriented reform to enhance economic development and welfare is substantial. The challenge may be one of broad-based persuasion sufficient to prevent those who benefit from the status quo from maintaining it. To the extent that markets for institutions emerge from local ground up, that is perhaps the territory upon and direction from which their liberalization is most effectively achieved.

TABLE 1 - Summary Statistics

	Mean	Std. Dev.	Min.	Max.
Δ Economic Freedom	0.517	0.924	-2.629	4.102
Economic Freedom	5.549	1.198	1.841	7.924
Δ Government Size	0.400	1.224	-2.879	4.267
Government Size	6.027	1.462	1.573	9.307
Δ Legal Structure and Property Rights	0.410	1.089	-1.651	3.724
Legal Structure and Property Rights	4.073	1.312	1.059	7.555
Δ Sound Money	0.855	2.185	-6.402	8.180
Sound Money	6.244	2.242	0.000	9.682
Δ International Trade	0.613	1.793	-8.919	7.021
International Trade	5.619	2.137	0.000	10.000
Δ Regulation	0.397	0.851	-2.699	3.619
Regulation	5.853	1.227	2.346	9.143
Aid	4.849	6.129	-0.052	34.512
Raw Groups	31.868	40.975	0.000	315
ln(1+ Raw Groups)	2.936	1.108	0.000	5.756

ln(1+Raw Groups)	Observations
min	5
(min,mean-sd]	42
(mean-sd,mean]	74
(mean,mean+sd]	94
(mean+sd,max]	44

TABLE 2a - Reform (Δ Economic Freedom) and Aid

	No fixed effects		*Time fixed effects*	
	(1a)	(1b)	(2a)	(2b)
Panel a - OLS				
Aid	0.010	-0.006	-0.003	-0.003
	(0.498)	(0.795)	(0.851)	(0.859)
Aid·Groups		0.045		0.038
		(0.002)		(0.010)
Aid·Groups·EF		-0.008		-0.009
		(0.001)		(0.002)
Panel b - IV				
Aid	-0.079	-0.056	-0.085	-0.057
	(0.005)	(0.030)	(0.002)	(0.014)
	(0.012)	(0.070)	(0.002)	(0.045)
Aid×Groups		0.075		0.069
		(0.000)		(0.000)
		(0.000)		(0.001)
Aid·Groups·EF		-0.017		-0.017
		(0.000)		(0.000)
		(0.000)		(0.000)
SW - Aid	48.16	27.82	47.28	29.35
SW - Aid·Groups		11.09		10.01
SW - Aid·Groups·EF		14.58		14.51
Hansen J p-val	0.567	0.432	0.874	0.242
Stock-Wright p-val		0.016		0.001
Observations	257	257	257	257

Notes: Dependent variable is the change in the overall economic freedom index. Estimation is by OLS (panel a) and IV (panel b), without (columns (1)) and with (columns (2)) time period fixed effects, and with robust, clustered standard errors. P-values are in parentheses. For IV, the second set of p-values are from the restricted efficient bootstrap. Control variables in all specifications include the level of the overall economic freedom index or component, real GDP, population, real GDP per capita growth, democracy, change in democracy, ethnic fractionalization. Groups is included as a control variable in specifications (1b) and (2b). For IV, the Sanderson-Windmeijer F of excluded instruments is reported (to test for instrument relevance), along with the Hansen J p-value (to test for instrument exogeneity) The Stock-Wright p-value is also reported (to test for joint significance of endogenous regressors). Excluded instruments are based on a second-order polynomial. For all columns, instruments include the squares of the log of GDP and the log of population. For columns (1b) and (2b), additional instruments include those indicated in equation (2).

TABLE 2b - Reform (Δ Government Size) and Aid

	No fixed effects		*Time fixed effects*	
	(1a)	(1b)	(2a)	(2b)
Panel a - OLS				
Aid	-0.019	-0.046	-0.032	-0.039
	(0.309)	(0.039)	(0.091)	(0.112)
Aid·Groups		0.051		0.052
		(0.000)		(0.000)
Aid·Groups·G		-0.008		-0.010
		(0.001)		(0.000)
Panel b - IV				
Aid	-0.104	-0.071	-0.094	-0.052
	(0.000)	(0.018)	(0.000)	(0.077)
	(0.000)	(0.089)	(0.000)	(0.245)
Aid×Groups		0.048		0.063
		(0.041)		(0.003)
		(0.064)		(0.010)
Aid·Groups·G		-0.011		-0.014
		(0.004)		(0.000)
		(0.005)		(0.000)
SW - Aid	55.35	21.80	56.06	22.80
SW - Aid·Groups		19.43		19.52
SW - Aid·Groups·G		13.79		15.01
Hansen J p-val	0.235	0.297	0.140	0.125
Stock-Wright p-val		0.017		0.000
Observations	258	258	258	258

Notes: Dependent variable is the change in the Government Size component of the economic freedom index. Estimation is by OLS (panel a) and IV (panel b), without (columns (1)) and with (columns (2)) time period fixed effects, and with robust, clustered standard errors. P-values are in parentheses. For IV, the second set of p-values are from the restricted efficient bootstrap. Control variables in all specifications include the level of the overall economic freedom index or component, real GDP, population, real GDP per capita growth, democracy, change in democracy, ethnic fractionalization. Groups is included as a control variable in specifications (1b) and (2b). For IV, the Sanderson-Windmeijer F of excluded instruments is reported (to test for instrument relevance), along with the Hansen J p-value (to test for instrument exogeneity) The Stock-Wright p-value is also reported (to test for joint significance of endogenous regressors). Excluded instruments are based on a second-order polynomial. For all columns, instruments include the squares of the log of GDP and the log of population. For columns (1b) and (2b), additional instruments include those indicated in equation (2).

TABLE 2c - Reform (Δ Legal Structure and Property Rights) and Aid

	No fixed effects		*Time fixed effects*	
	(1a)	(1b)	(2a)	(2b)
Panel a - OLS				
Aid	-0.003	-0.030	-0.011	-0.028
	(0.886)	(0.173)	(0.515)	(0.215)
Aid·Groups		0.013		0.003
		(0.464)		(0.845)
Aid·Groups·Law		0.002		0.003
		(0.582)		(0.403)
Panel b - IV				
Aid	-0.041	-0.055	-0.037	-0.044
	(0.015)	(0.010)	(0.024)	(0.031)
	(0.079)	(0.085)	(0.071)	(0.115)
Aid·Groups		0.020		0.006
		(0.412)		(0.792)
		(0.427)		(0.801)
Aid·Groups·Law		-0.001		0.002
		(0.861)		(0.792)
		(0.865)		(0.706)
SW - Aid	25.02	20.02	25.36	21.43
SW - Aid·Groups		16.91		15.10
SW - Aid·Groups·Law		7.09		7.83
Hansen J p-val	0.698	0.802	0.875	0.844
Stock-Wright p-val		0.414		0.566
Observations	240	240	240	240

Notes: Dependent variable is the change in the Legal Structure and Property Rights component of the economic freedom index. Estimation is by OLS (panel a) and IV (panel b), without (columns (1)) and with (columns (2)) time period fixed effects, and with robust, clustered standard errors. P-values are in parentheses. For IV, the second set of p-values are from the restricted efficient bootstrap. Control variables in all specifications include the level of the overall economic freedom index or component, real GDP, population, real GDP per capita growth, democracy, change in democracy, ethnic fractionalization. Groups is included as a control variable in specifications (1b) and (2b). For IV, the Sanderson-Windmeijer F of excluded instruments is reported (to test for instrument relevance), along with the Hansen J p-value (to test for instrument exogeneity) The Stock-Wright p-value is also reported (to test for joint significance of endogenous regressors). Excluded instruments are based on a second-order polynomial. For all columns, instruments include the squares of the log of GDP and the log of population. For columns (1b) and (2b), additional instruments include those indicated in equation (2).

TABLE 2d - Reform (Δ Sound Money) and Aid

	No fixed effects		*Time fixed effects*	
	(1a)	(1b)	(2a)	(2b)
Panel a - OLS				
Aid	-0.030	-0.044	-0.028	-0.045
	(0.213)	(0.208)	(0.259)	(0.162)
Aid·Groups		0.033		0.032
		(0.085)		(0.118)
Aid·Groups·Money		-0.007		-0.006
		(0.009)		(0.016)
Panel b - IV				
Aid	-0.125	-0.070	-0.147	-0.080
	(0.011)	(0.105)	(0.003)	(0.050)
	(0.009)	(0.351)	(0.002)	(0.296)
Aid·Groups		0.063		0.052
		(0.033)		(0.074)
		(0.050)		(0.088)
Aid·Groups·Money		-0.019		-0.018
		(0.000)		(0.000)
		(0.001)		(0.002)
SW - Aid	22.37	32.29	23.98	30.09
SW - Aid·Groups		12.42		10.63
SW - Aid·Groups·Money		10.67		10.35
Hansen J p-val	0.528	0.602	0.487	0.544
Stock-Wright p-val		0.010		0.004
Observations	259	259	259	259

Notes: Dependent variable is the change in the Sound Money component of the economic freedom index. Estimation is by OLS (panel a) and IV (panel b), without (columns (1)) and with (columns (2)) time period fixed effects, and with robust, clustered standard errors. P-values are in parentheses. For IV, the second set of p-values are from the restricted efficient bootstrap. Control variables in all specifications include the level of the overall economic freedom index or component, real GDP, population, real GDP per capita growth, democracy, change in democracy, ethnic fractionalization. Groups is included as a control variable in specifications (1b) and (2b). For IV, the Sanderson-Windmeijer F of excluded instruments is reported (to test for instrument relevance), along with the Hansen J p-value (to test for instrument exogeneity) The Stock-Wright p-value is also reported (to test for joint significance of endogenous regressors). Excluded instruments are based on a second-order polynomial. For all columns, instruments include the squares of the log of GDP and the log of population. For columns (1b) and (2b), additional instruments include those indicated in equation (2).

TABLE 2e - Reform (Δ International Trade) and Aid

	No fixed effects		*Time fixed effects*	
	(1a)	(1b)	(2a)	(2b)
Panel a - OLS				
Aid	0.031	-0.015	0.007	-0.002
	(0.146)	(0.709)	(0.698)	(0.948)
Aid·Groups		0.024		0.002
		(0.354)		(0.930)
Aid·Groups·Trade		-0.000		0.001
		(0.893)		(0.868)
Panel b - IV				
Aid	-0.109	-0.094	-0.087	-0.057
	(0.039)	(0.120)	(0.060)	(0.263)
	(0.056)	(0.186)	(0.077)	(0.314)
Aid·Groups		0.009		-0.009
		(0.740)		(0.743)
		(0.765)		(0.779)
Aid·Groups·Trade		0.001		0.001
		(0.814)		(0.735)
		(0.843)		(0.764)
SW - Aid	28.38	14.47	33.06	16.81
SW - Aid·Groups		8.13		7.76
SW - Aid·Groups·Trade		6.36		5.95
Hansen J p-val	0.720	0.354	0.763	0.174
Stock-Wright p-val		0.064		0.085
Observations	245	245	245	245

Notes: Dependent variable is the change in the International Trade component of the economic freedom index. Estimation is by OLS (panel a) and IV (panel b), without (columns (1)) and with (columns (2)) time period fixed effects, and with robust, clustered standard errors. P-values are in parentheses. For IV, the second set of p-values are from the restricted efficient bootstrap. Control variables in all specifications include the level of the overall economic freedom index or component, real GDP, population, real GDP per capita growth, democracy, change in democracy, ethnic fractionalization. Groups is included as a control variable in specifications (1b) and (2b). For IV, the Sanderson-Windmeijer F of excluded instruments is reported (to test for instrument relevance), along with the Hansen J p-value (to test for instrument exogeneity) The Stock-Wright p-value is also reported (to test for joint significance of endogenous regressors). Excluded instruments are based on a second-order polynomial. For all columns, instruments include the squares of the log of GDP and the log of population. For columns (1b) and (2b), additional instruments include those indicated in equation (2).

TABLE 2f - Reform (Δ Regulation) and Aid

	No fixed effects		*Time fixed effects*	
	(1a)	(1b)	(2a)	(2b)
Panel a - OLS				
Aid	0.003	0.008	0.001	0.007
	(0.837)	(0.756)	(0.932)	(0.756)
Aid·Groups		0.047		0.044
		(0.022)		(0.054)
Aid·Groups·Reg		-0.009		-0.009
		(0.009)		(0.020)
Panel b - IV				
Aid	-0.067	-0.051	-0.067	-0.048
	(0.021)	(0.077)	(0.013)	(0.078)
	(0.028)	(0.120)	(0.019)	(0.122)
Aid·Groups		0.039		0.032
		(0.154)		(0.265)
		(0.221)		(0.332)
Aid·Groups·Reg		-0.009		-0.008
		(0.042)		(0.086)
		(0.069)		(0.132)
SW - Aid	48.75	18.82	49.59	24.24
SW - Aid·Groups		14.25		14.18
SW - Aid·Groups·Reg		16.36		17.17
Hansen J p-val	0.226	0.741	0.251	0.450
Stock-Wright p-val		0.072		0.016
Observations	250	250	250	250

Notes: Dependent variable is the change in the Regulation component of the economic freedom index. Estimation is by OLS (panel a) and IV (panel b), without (columns (1)) and with (columns (2)) time period fixed effects, and with robust, clustered standard errors. P-values are in parentheses. For IV, the second set of p-values are from the restricted efficient bootstrap. Control variables in all specifications include the level of the overall economic freedom index or component, real GDP, population, real GDP per capita growth, democracy, change in democracy, ethnic fractionalization. Groups is included as a control variable in specifications (1b) and (2b). For IV, the Sanderson-Windmeijer F of excluded instruments is reported (to test for instrument relevance), along with the Hansen J p-value (to test for instrument exogeneity) The Stock-Wright p-value is also reported (to test for joint significance of endogenous regressors). Excluded instruments are based on a second-order polynomial. For all columns, instruments include the squares of the log of GDP and the log of population. For columns (1b) and (2b), additional instruments include those indicated in equation (2).

TABLE 3a - Conditional Marginal Effects of Aid on Reform (ΔEF), IV

Groups(↓) EF(→)	min	mean-sd	mean	mean+sd	max
Panel a - no fixed effects					
min	-0.056	-0.056	-0.056	-0.056	-0.056
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
	(0.091)	(0.065)	(0.082)	(0.074)	(0.080)
mean-sd	0.026	-0.050	-0.087	-0.123	-0.159
	(0.296)	(0.030)	(0.001)	(0.000)	(0.000)
	(0.318)	(0.048)	(0.003)	(0.001)	(0.001)
mean	0.075	-0.047	-0.105	-0.164	-0.221
	(0.024)	(0.082)	(0.002)	(0.000)	(0.000)
	(0.059)	(0.096)	(0.005)	(0.002)	(0.001)
mean+sd	0.125	-0.044	-0.124	-0.205	-0.284
	(0.005)	(0.191)	(0.004)	(0.000)	(0.000)
	(0.025)	(0.211)	(0.003)	(0.002)	(0.000)
max	0.201	-0.039	-0.153	-0.268	-0.380
	(0.002)	(0.401)	(0.010)	(0.001)	(0.000)
	(0.018)	(0.429)	(0.017)	(0.002)	(0.000)
Panel b - time fixed effects					
min	-0.057	-0.057	-0.057	-0.057	-0.057
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
	(0.043)	(0.050)	(0.043)	(0.048)	(0.047)
mean-sd	0.012	-0.064	-0.101	-0.137	-0.173
	(0.614)	(0.004)	(0.000)	(0.000)	(0.000)
	(0.642)	(0.003)	(0.000)	(0.000)	(0.000)
mean	0.054	-0.068	-0.127	-0.186	-0.243
	(0.105)	(0.012)	(0.000)	(0.000)	(0.000)
	(0.161)	(0.012)	(0.000)	(0.000)	(0.000)
mean+sd	0.097	-0.073	-0.153	-0.234	-0.313
	(0.032)	(0.035)	(0.001)	(0.000)	(0.000)
	(0.065)	(0.033)	(0.000)	(0.000)	(0.000)
max	0.162	-0.079	-0.194	-0.309	-0.422
	(0.012)	(0.098)	(0.002)	(0.000)	(0.000)
	(0.061)	(0.105)	(0.002)	(0.001)	(0.000)
Panel c - observation counts					
min	0	0	0	4	1
(min, mean-sd]	0	7	18	13	4
(mean-sd, mean]	1	10	28	25	10
(mean, mean+sd]	0	14	21	36	23
(mean+sd, max]	0	10	10	19	5

Notes: Conditional marginal effect of Aid associated with specifcations 1c (no fixed effects) and 2c (fixed effects) in Panel b (IV estimates) of table 2a are reported, for all combinations of the minimum, mean - standard deviation, mean, mean + standard deviation, and maximum values of Groups and the Economic Freedom index. P-values are in parentheses. The second set of p-values are from the restricted efficient bootstrap.

TABLE 3b - Conditional Marginal Effects of Aid on Reform (ΔG), IV

Groups(↓) G(→)	min	mean-sd	mean	mean+sd	max
Panel a - IV, no fixed effects					
min	-0.071	-0.071	-0.071	-0.071	-0.071
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
	(0.076)	(0.075)	(0.094)	(0.092)	(0.080)
mean-sd	-0.015	-0.074	-0.103	-0.132	-0.168
	(0.704)	(0.007)	(0.000)	(0.000)	(0.000)
	(0.745)	(0.020)	(0.000)	(0.000)	(0.000)
mean	0.019	-0.076	-0.122	-0.169	-0.227
	(0.725)	(0.033)	(0.000)	(0.000)	(0.000)
	(0.747)	(0.052)	(0.001)	(0.001)	(0.000)
mean+sd	0.054	-0.078	-0.142	-0.206	-0.285
	(0.467)	(0.098)	(0.002)	(0.000)	(0.000)
	(0.519)	(0.126)	(0.004)	(0.001)	(0.000)
max	0.107	-0.080	-0.171	-0.263	-0.376
	(0.306)	(0.234)	(0.010)	(0.001)	(0.001)
	(0.361)	(0.285)	(0.012)	(0.006)	(0.000)
Panel b - IV, time fixed effects					
min	-0.052	-0.052	-0.052	-0.052	-0.052
	(0.077)	(0.077)	(0.077)	(0.077)	(0.077)
	(0.245)	(0.216)	(0.201)	(0.224)	(0.228)
mean-sd	0.023	-0.053	-0.091	-0.128	-0.175
	(0.479)	(0.025)	(0.000)	(0.000)	(0.000)
	(0.518)	(0.060)	(0.000)	(0.000)	(0.000)
mean	0.068	-0.055	-0.114	-0.174	-0.249
	(0.143)	(0.085)	(0.000)	(0.000)	(0.000)
	(0.171)	(0.143)	(0.000)	(0.000)	(0.000)
mean+sd	0.114	-0.056	-0.138	-0.221	-0.323
	(0.074)	(0.203)	(0.001)	(0.000)	(0.000)
	(0.100)	(0.254)	(0.005)	(0.000)	(0.000)
max	0.184	-0.057	-0.175	-0.292	-0.439
	(0.045)	(0.378)	(0.007)	(0.000)	(0.000)
	(0.079)	(0.411)	(0.017)	(0.000)	(0.000)
Panel c - observation counts					
min	0	2	2	1	0
(min, mean-sd]	0	4	14	17	7
(mean-sd, mean]	1	11	30	24	9
(mean, mean+sd]	0	11	24	38	20
(mean+sd, max]	0	9	16	14	6

Notes: Conditional marginal effect of Aid associated with specifcations 1c (no fixed effects) and 2c (fixed effects) in Panel b (IV estimates) of table 2a are reported, for all combinations of the minimum, mean - standard deviation, mean, mean + standard deviation, and maximum values of Groups and the Government Size component of the Economic Freedom index. P-values are in parentheses. The second set of p-values are from the restricted efficient bootstrap.

TABLE 3c - Conditional Marginal Effects of Aid on Reform (Δ Money), IV

Groups(↓) Money(→)	min	mean-sd	mean	mean+sd	max
Panel a - no fixed effects					
min	-0.070	-0.070	-0.070	-0.070	-0.070
	(0.105)	(0.105)	(0.105)	(0.105)	(0.105)
	(0.336)	(0.313)	(0.359)	(0.382)	(0.365)
mean-sd	0.044	-0.091	-0.166	-0.242	-0.282
	(0.416)	(0.051)	(0.001)	(0.000)	(0.000)
	(0.409)	(0.097)	(0.009)	(0.001)	(0.001)
mean	0.113	-0.104	-0.225	-0.347	-0.411
	(0.154)	(0.115)	(0.002)	(0.000)	(0.000)
	(0.152)	(0.138)	(0.006)	(0.001)	(0.000)
mean+sd	0.183	-0.116	-0.284	-0.452	-0.541
	(0.094)	(0.192)	(0.004)	(0.000)	(0.000)
	(0.103)	(0.224)	(0.009)	(0.001)	(0.000)
max	0.290	-0.136	-0.375	-0.614	-0.741
	(0.065)	(0.290)	(0.008)	(0.000)	(0.000)
	(0.066)	(0.276)	(0.011)	(0.001)	(0.000)
Panel b - time fixed effects					
min	-0.080	-0.080	-0.080	-0.080	-0.080
	(0.050)	(0.050)	(0.050)	(0.050)	(0.005)
	(0.275)	(0.281)	(0.263)	(0.277)	(0.290)
mean-sd	0.015	-0.116	-0.189	-0.263	-0.302
	(0.769)	(0.012)	(0.000)	(0.000)	(0.000)
	(0.769)	(0.017)	(0.000)	(0.000)	(0.000)
mean	0.074	-0.138	-0.256	-0.374	-0.437
	(0.350)	(0.038)	(0.000)	(0.000)	(0.000)
	(0.368)	(0.028)	(0.002)	(0.001)	(0.000)
mean+sd	0.132	-0.160	-0.323	-0.486	-0.573
	(0.224)	(0.078)	(0.001)	(0.000)	(0.000)
	(0.259)	(0.099)	(0.002)	(0.000)	(0.000)
max	0.222	-0.194	-0.426	-0.659	-0.783
	(0.157)	(0.139)	(0.003)	(0.000)	(0.000)
	(0.166)	(0.146)	(0.001)	(0.001)	(0.000)
Panel c - observation counts					
min					
(min, mean-sd]	0	0	0	1	4
(mean-sd, mean]	0	6	7	27	3
(mean, mean+sd]	3	5	29	28	10
(mean+sd, max]	3	7	11	21	3

Notes: Conditional marginal effect of Aid associated with specifcations 1c (no fixed effects) and 2c (fixed effects) in Panel b (IV estimates) of table 2a are reported, for all combinations of the minimum, mean - standard deviation, mean, mean + standard deviation, and maximum values of Groups and the Sound Money component of the Economic Freedom index. P-values are in parentheses. The second set of p-values are from the restricted efficient bootstrap.

TABLE 3d - Conditional Marginal Effects of Aid on Reform (Δ Reg), IV

Groups(↓) Reg(→)	min	mean-sd	mean	mean+sd	max
Panel a - no fixed effects					
min	-0.051	-0.051	-0.051	-0.051	-0.051
	(0.077)	(0.077)	(0.077)	(0.077)	(0.077)
	(0.127)	(0.134)	(0.122)	(0.101)	(0.127)
mean-sd	-0.019	-0.056	-0.076	-0.097	-0.131
	(0.612)	(0.039)	(0.004)	(0.001)	(0.001)
	(0.627)	(0.056)	(0.008)	(0.003)	(0.004)
mean	0.001	-0.059	-0.092	-0.125	-0.179
	(0.984)	(0.080)	(0.004)	(0.001)	(0.002)
	(0.986)	(0.124)	(0.008)	(0.007)	(0.005)
mean+sd	0.021	-0.063	-0.108	-0.153	-0.228
	(0.769)	(0.152)	(0.008)	(0.002)	(0.003)
	(0.818)	(0.221)	(0.020)	(0.003)	(0.010)
max	0.052	-0.068	-0.132	-0.197	-0.305
	(0.608)	(0.273)	(0.021)	(0.004)	(0.005)
	(0.695)	(0.391)	(0.067)	(0.014)	(0.007)
Panel b - time fixed effects					
min	-0.048	-0.048	-0.048	-0.048	-0.048
	(0.078)	(0.078)	(0.078)	(0.078)	(0.078)
	(0.117)	(0.140)	(0.141)	(0.136)	(0.133)
mean-sd	-0.023	-0.056	-0.074	-0.092	-0.122
	(0.523)	(0.028)	(0.002)	(0.001)	(0.002)
	(0.577)	(0.049)	(0.006)	(0.010)	(0.007)
mean	-0.009	-0.062	-0.090	-0.119	-0.167
	(0.873)	(0.028)	(0.002)	(0.001)	(0.003)
	(0.887)	(0.086)	(0.002)	(0.002)	(0.006)
mean+sd	0.006	-0.067	-0.107	-0.146	-0.213
	(0.933)	(0.113)	(0.005)	(0.001)	(0.005)
	(0.945)	(0.195)	(0.007)	(0.007)	(0.012)
max	0.029	-0.076	-0.132	-0.189	-0.284
	(0.779)	(0.209)	(0.013)	(0.003)	(0.008)
	(0.792)	(0.311)	(0.048)	(0.013)	(0.016)
Panel c - observation counts					
min	0	0	0	2	3
(min, mean-sd]	0	7	17	12	6
(mean-sd, mean]	1	12	23	24	12
(mean, mean+sd]	0	10	31	36	12
(mean+sd, max]	0	8	17	15	4

Notes: Conditional marginal effect of Aid associated with specifcations 1c (no fixed effects) and 2c (fixed effects) in Panel b (IV estimates) of table 2a are reported, for all combinations of the minimum, mean - standard deviation, mean, mean + standard deviation, and maximum values of Groups and the Regulation component of the Economic Freedom index. P-values are in parentheses. The second set of p-values are from the restricted efficient bootstrap.

References

Annen, K. and S. Knack. 2021. Better Policies from Policy-Selective Aid? *The World Bank Economic Review* 35(4): 829-844.

Arndt, C., S. Jones, and F. Tarp. 2016. What is the aggregate economic rate of return to foreign aid? *World Bank Economic Review* 30(3): 446-474.

Barro, R. 1990. Government spending in a simple model of endogenous growth. *Journal of Political Economy* 98: S103-S125.

Barro, R. and J. Lee. 2005. IMF programs: Why is chosen and what are the effects? *Journal of Monetary Economics* 52: 1245-1269.

Bischoff, I. 2003. Determinants of the Increase in the Number of Interest Groups in Western Democracies: Theoretical Considerations and Evidence from 21 OECD Countries. *Public Choice* 114: 197-218.

Bun, M. and T. Harrison. 2019. OLS and IV estimation of regression models including endogenous interaction terms. *Econometric Reviews* 38(7): 814-827.

Burnside, C. and D. Dollar. 2000. Aid, policies, and growth. *American Economic Review* 90(4): 847-868.

Coyne, C. 2007. *After war: The political economy of exporting democracy.* Stanford University Press.

Dalgaard, C. and H. Hansen. 2001. On aid, growth and good policies. *Journal of Development Studies* 37(6): 17-41.

Davidson, R. and J. MacKinnon. 2010 Wild bootstrap tests for IV regression. *Journal of Business and Economic Statistics* 28: 128-144.

Djankov, S., J.G. Montalvo, and M. Reynal-Querol. 2008. The curse of aid. *Journal of Economic Growth* 13: 169-194.

Doner, R. and B. Schneider. 2000. Business associations and economic development: Why some associations contribute more than others. *Business and Politics* 2(3): 261-288.

Dreher, A., V. Eichenauer, and K. Gehring. 2018. Geopolitics, Aid, and Growth: The impact

of UN security council membership on the effectiveness of aid. *The World Bank Economic Review* 32(2): 268-286.

Dreher, A. and S. Langlotz. 2020. Aid and growth: New evidence using an excludable instrument. *Canadian Journal of Economics* 53(3): 1162-1198.

Dreher, A. and J. Sturm. 2012. Do the IMF and the World Bank influence voting in the UN General Assembly? *Public Choice* 151: 363-397.

Dutta, N. and C. Williamson. 2016. Aiding economic freedom: Exploring the role of political institutions. *European Journal of Political Economy* 45: 24-38.

Dzhumashev, R. and A. Hailemariam. 2021. Foreign aid and the quality of economic institutions. *European Journal of Political Economy* 68: 1-17.

Easterly, W., R. Levine, and D. Roodman. 2004. Aid, policies, and growth: Comment. *The American Economic Review* 94(3): 774-780.

Grier, K. and R. Grier. 2021. The Washington consensus works: Causal effects of reform, 1970-2015. *Journal of Comparative Economics* 49(1): 59-72.

Heckelman, J.C. 2000. Consistent estimates of the impact of special interest groups on economic growth. *Public Choice* 104: 319-327.

Heckelman, J.C. and S. Knack. 2008. Foreign aid and Market-liberalizing reform. *Economica* 75: 524-548.

Heckelman, J.C. and S. Knack. 2009. Aid, economic freedom, and growth. *Contemporary Economic Policy* 27(1): 46-53.

Heckelman, J.C. and B. Wilson. 2013. Institutions, lobbying, and economic performance. *Economics & Politics* 25: 360-386.

Heckelman, J.C. and B. Wilson 2019. The growth-maximizing level of regulation: Evidence from a panel of international data. *European Journal of Political Economy* 59: 354-368.

Heckelman, J.C. and B. Wilson. 2021. Targeting inflation targeting: The influence of interest groups. *Public Choice* 189: 533-554.

Hodler, R. 2007. Rent seeking and aid effectiveness. *International Tax and Public Finance*

14(5): 525-541.

Kelejian, H. 1971. Two-stage least squares and econometric systems linear in parameters but nonlinear in the endogenous variables. *Journal of the American Statistical Association* 66: 373-374.

Kilby, C. 2005. Aid and regulation. *The Quarterly Review of Economics and Finance* 45: 325-345.

Knack, S. 2004. Does foreign aid promote democracy? *International Studies Quarterly* 48(1): 251-266.

Knedlik, T. and F. Kronthaler. 2007. *Journal of Development Perspectives* 3(1): 116-135.

Kuziemko, I. and E. Werker. 2006. How much is a seat on the Security Council worth? Foreign aid and bribery at the United Nations. *Journal of Political Economy* 114(5): 905-930.

McChesney, F. 1987. Rent extraction and rent creation in the economic theory of regulation. *The Journal of Legal Studies* 16(1): 101-118.

Murrell, P. 1984. An Examination of the factors affecting the formation of interest groups in OECD countries. *Public Choice* 43: 151-171.

Nizalova, O. and I. Murtazashvili. 2016. Exogenous treatment and endogenous factors: Vanishing of omitted variable bias on the interaction term. *Journal of Econometric Methods* 5(1): 71-77.

Olson, M. 1982. *The Rise and Decline of Nations: Economic Growth, Staglation, and Social Rigidities.* New Haven: Yale University Press.

Pavlik, J., B. Powell, and A. Young. 2022. Does aid cause changes in economic freedom? *Southern Economic Journal* 89(1): 90-111.

Roodman, D. 2007. The anarchy of numbers: Aid, development, and cross-country empirics. *World Bank Economic Review* 21(2): 255-277.

Stigler, G. 1971. The theory of economic regulation. *The Bell Journal of Economics and Management Science* 2(1): 3-21.

Stock, J. and J. Wright. 2000. GMM with weak identification. *Econometrica* 68: 1055-1096.

Svensson, J. 2000. Foreign aid and rent seeking. *Journal of International Economics* 51(2): 437-461

Yandle, B. 1983. Bootleggers and Baptists: The education of a regulatory economist. *Regulation*: 12-16.

Young, A. and K. Sheehan. 2014. Foreign aid, institutional quality, and growth. *European Journal of Political Economy* 36: 195-208.

Data Availability Statement

Data underlying this article is currently available upon request and will be made available in a repository if accepted for publication.