Does Foreign Investment Really Reduce Repression?

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Cross-national empirical studies have found that foreign investment has beneficial effects for human rights. We argue that these studies poorly operationalize foreign investment to test theoretical predictions and suffer from sampling bias. We demonstrate that investment stock, rather than inflow, is the superior operationalization of structural dependence theory. We construct regression models of government repression of physical integrity rights, include much more data than previous studies, and use a new multiple imputation algorithm for time-series cross-section data to resolve sampling bias. We find no evidence that foreign investment affects repression, contradicting conventional wisdom and suggesting that the political gains from repression frequently dwarf any economic costs for governments.

Keywords: foreign direct investment, globalization, human rights, repression

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[1] Replication data for this paper are downloadable at http://www.acsu.buffalo.edu/~jsorens/.

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The second era of globalization, characterized by extensive multinational investment in developing countries and an explosion of offshore financial markets, spawned much theorizing about the effects of international market penetration on government repression of basic human rights. While fragmentary case evidence suggested either positive or adverse effects of foreign investors, no comprehensive picture emerged until recently. Over the past decade, scholars of these topics have turned to quantitative techniques and global, cross-national datasets to adjudicate theoretical controversies. Blanton and Blanton (2007:143) characterize the previous conventional wisdom from case-based research as holding that "[foreign direct investment] and respect for human rights are inherently contradictory." The overwhelming majority of quantitative research has flatly contradicted this view (Richards, Gelleny, and Sacko 2001; Hafner-Burton 2005a; Eriksen and de Soysa 2009). However, all existing empirical models of foreign investment's effects on repression share certain flaws, which upon correction yield very different results.

This paper critiques the quantitative research on the effects of the globalization of capital on human rights, clarifies the modeling of structural dependence theory, and interprets our findings in light of standard accounts of the reasons why governments repress.[2] We do not single out the human rights literature on these points, as some of our critique bites with equal

[2] We focus especially on physical integrity rights and freedom from political terror, rather than political rights and civil liberties, since the former concepts denote the absence of acts of repression, while the latter concepts refer to legal and institutional regime characteristics. These two different categories of human rights may stand in causal relation to each other and may relate differently to globalization.

force against a substantial body of recent political economy work. However, because human rights is an area of substantive interest for us, it is our focus.

Despite the difficulty of proving a negative, we test the FDI-repression link with many different model specifications, using two measures of human rights, different control variables, and reducing the sample to developing countries only. We find little evidence FDI has a beneficial effect on physical integrity rights; most of the models show an adverse but statistically insignificant effect. The overriding reason our results differ from those of prior studies is that we use much more data.

The paper is ordered as follows. The next section summarizes the arguments, methods, and findings from the state of the art in research on globalization and human rights. We critique the research on two major grounds: operationalization of theory and reliance on incomplete data. The third section summarizes our data and methods, presenting our models of the effects of globalization variables on human rights along several dimensions. The fourth section concludes with implications and directions for further study.

Previous Research: Theories, Methods, and Findings

Early studies of foreign investment and human rights generally took a pessimistic view of the relationship. Dependency theorists such as Hymer (1979) argued that multinational enterprises (MNEs) collaborate with the governments of their host countries to suppress wages and social spending in poorer economies, creating a new form of economic colonialism. In the post-World War II era, dependency thinkers viewed multinational investment as a new colonial system, in which MNEs promoted the interests of their home countries by exploiting their hosts. Dependency theory and classical Marxism more broadly faced severe theoretical and

evidentiary difficulties, causing their fall from favor among mainstream political economists. Even governments of small, poor countries have evinced significant autonomy vis-à-vis MNEs (Fieldhouse 1986). Additionally, the autarkic policy recommendations of dependency theorists may have helped cause economic stagnation across Latin America, South Asia, the Middle East, and Africa (Waterbury 1999).

Mainstream structural dependence arguments today do not derive from classic dependency theory and have rather different implications. Przeworski and Wallerstein (1988) developed a closed-economy model of the state's structural dependence on capital that other scholars later adapted for globalization research. (Przeworski and Wallerstein, meanwhile, were formalizing Lindblom's (1982) earlier insights.) In their model, a state attempting to maximize workers' income will have to curtail transfers from capital in order to maintain sufficiently high output, because capitalists withhold more investment in favor of consumption as investment income is taxed at a higher rate. As foreign investment increasingly occupied a greater share of capital formation worldwide, scholars speculated that as globalization advanced, structural dependence would tighten. In a world of perfectly mobile capital, governments would be unable to maintain different tax rates due to jurisdictional arbitrage (Rodrik 1997:64). Foreign direct investment offers significant economic benefits to developing countries, and there is strong evidence that governments of these countries compete to attract FDI through bilateral investment treaties (Elkins, Guzman, and Simmons 2006). However, FDI is motivated by long-term profitability and is less "footloose" than foreign portfolio investment (FPI). FPI, in contrast, may not generate the economic benefits of FDI, and developing countries have been quite

willing to impose capital controls on liquid investments (Prasad, Rogoff, Wei, and Kose 2003; Bhagwati 2004).

The familiar "race to the bottom" thesis is a kind of structural dependence argument. It claims that governments, especially in developing countries, need foreign capital for development and thus must accede to many of the demands for labor repression and stability that investors desire – governments are structurally dependent on foreign capital. Anecdotal evidence of investor friendliness with conservative, authoritarian regimes is plentiful (for a review, see Blanton and Blanton (2007)). Conversely, some political economists argue that governments provide public goods like high-quality education, health care, and labor peace to attract investors, yielding a positive relationship between investment openness and government spending (Garrett 1998). Foreign investors, especially in capital-intensive industries with facilities that are difficult to secure against violence, will prefer less-conflict-prone countries for investment, *ceteris paribus*. If governments seek foreign investment for development, they will want to create an environment unconducive to violence. Therefore, if repression encourages future violence, governments dependent on foreign investment for growth will reduce repression. Labor repression might also be counterproductive if it reduces workers' expected return to human capital investment, thus impeding dynamic gains in productivity, or if it alienates customers in the West (Spar 1999; Blanton and Blanton 2009). Thus, structural dependence arguments can support either adverse or benign relationships between FDI and repression.

Finally, a third possibility is that governments remain relatively autonomous of foreign investors and structural dependence is limited. Regarding repression, we should probably not

expect governments facing threats at home to compromise their fundamental security interests in order to obtain the undeniable but generally modest benefits of foreign investment. Instead, states do what they perceive they must even in the face of international pressure, whenever regime survival or other fundamental goals are at stake.[3] This occurred in Sri Lanka in 2008-2009, when Rajapakse's administration took decisive action and annihilated the Tamil insurgency despite pleas from external actors to stop the conflict given the widespread harm to noncombatants (Shashikumar 2009). If governments are not structurally dependent on foreign capital, we should expect no general relationship between FDI and government repression of human rights.

Recently, scholars have tested these contrasting expectations on cross-country datasets. Timberlake and Williams (1984) find no evidence linking inward FDI stock and repression. Richards et al. (2001) find both FDI and FPI inflows associated with greater respect for physical integrity rights. Hafner-Burton (2005a) conducts an extreme bounds analysis of estimates of the effects of globalization variables on human rights. She finds that gross FDI[4] and net FDI inflows are strongly and robustly negatively associated with repression. Blanton and Blanton (2007), using instrumental-variables regression to correct for reverse causation, find that FDI inflows

[3] This position sees the state as a unitary and autonomous actor with its own interests (Weber 1946; Krasner 1978; Skocpol 1985; Scott 1998).

[4] Gross FDI is measured as the sum of absolute values of net inflows and outflows of FDI.

are associated with better protection of personal integrity rights. The new conventional wisdom, therefore, is that FDI helps developing countries improve their human rights records.[5]

We have two principal methodological concerns about this research. First, these papers – except Timberlake and Williams – use FDI flows as the independent variable operationalizing structural dependence logic. If annual FDI inflows are large, the authors argue, the government should be more sensitive to the interests of MNEs. Therefore, a positive coefficient (if higher values on the dependent variable mean greater respect for basic human rights) indicates that MNEs successfully influence governments of host countries to respect human rights. We will argue inward FDI *stock* is a superior measure for operationalizing this structural dependence logic. Second, all these studies, apart from Timberlake and Williams, rely on the World Bank's *World Development Indicators* (WDI) for FDI and often other economic data. These data are riddled with missing values, which are listwise deleted in multiple regression. Fifty percent or more of relevant country-year observations are thereby dropped in these regressions, risking sampling bias (countries that do not consistently report economic data may also differ systematically in repression and levels of international integration).

The previous literature tested whether a surge or withdrawal of inward foreign investment in one year affects the conflict and human rights situation in the same or next year, depending on whether the variables are lagged. However, investment inflows could be high in a country not very dependent on foreign capital, simply because it is just opening to such investment, or the economic growth rate is high (China in the 1980s and 1990s). Investment

[5] FDI could also influence repression indirectly, through intrastate conflict (Barbieri and Reuveny 2005; Blanton and Apodaca 2007).

inflows could be low or negative in a country that is highly dependent on foreign capital, simply because of a temporary economic downturn (New Zealand 2001, Namibia in the 1990s). Investment inflows do not necessarily track the importance of foreign capital to an economy or even the attractiveness of an economy to foreign investors, but *change* in an economy's attractiveness . When a government undertakes liberalization, for instance, the profitability of investment rises, and inflows will occur. Using investment inflows on the right-hand side of a regression model captures two processes: domestic reforms that attract or repel investment and global technological changes that affect investment flows everywhere.[6]

These processes do not represent structural dependence logic. If foreign capital is more footloose than domestic capital (and therefore holds power to demand changes from a host government), we should expect that what matters for policy changes and other political outcomes of interest is the inward stock, not flow, of foreign capital (as a percentage of GDP or, even better, of gross fixed capital formation, to capture foreign capital's abundance relative to domestic capital).[7] Flows may correlate with stocks, but it is generally implausible for a one-year move in investment to affect government policy significantly the following year.

Next, we present our data and regression models of government respect for basic human rights. We use inward FDI stock to test existing theory and a combination of techniques to deal with missing data.

Data and Models

The Missing Data Problem

[6] Nothing in this argument refutes using inflows as dependent variables.

[7] Financial integration can also be measured with covered interest rate differentials.

Missing data are a common problem in cross-country statistical analysis but are particularly problematic when FDI and FPI variables from WDI are used on the right-hand side. All previous studies of globalization and repression use listwise deletion, dropping observations with missing data on any variable. Poor countries, command economies, very small jurisdictions, and countries gripped by internal turmoil are less likely to report economic data. Because likelihood of reporting data apparently correlates with likelihood of repression, this procedure selects on the dependent variable, a well-known source of bias in regression coefficients (Dubin and Rivers 1989; King, Honaker, Joseph, and Scheve 2001). For instance, Afghanistan, Croatia, Eritrea, Iraq, Somalia, and Yugoslavia are missing from these studies for virtually all of the 1990s.

Blanton and Blanton (2007) use some interpolation techniques to extend data on certain variables, but as King et al. (2001) show, such "single imputation" procedures bias standard errors toward zero because they do not take into account uncertainty about the new estimates. One solution is simply to use sources reporting more data. UNCTAD's data on FDI from 1980 are much more complete than those found in WDI. Furthermore, we can extend the UNCTAD data on stocks and flows back to 1970 for many countries by applying the flows data from WDI. For GDP and trade data, the Penn World Table is more complete than WDI. The variable descriptions at the end of the paper offer more information on how we construct the variables. Without resorting to multiple imputation, we increased the number of observations in our analyses by 112% over Blanton and Blanton (2007) and by more than 1,000% over Richards et al. (2001). Our empirical models cover the years 1976/1981-2003 (depending on the

dependent variable), compared to 1980-2003 for Blanton and Blanton and 1981, 1984, 1987, 1990, 1993, and 1995 for Richards et al.

Honaker and King (2010) present a new algorithm for multiple imputation of time series cross-section (TSCS) data. Multiple imputation of TSCS data works by bootstrap simulation of new in-series values for missing data using the information contained in the variance-covariance matrix of all the variables and of each variable with its own one-year lead and lag. The assumption behind multiple imputation is that data are "missing at random." This means that whether observations are missing or not depends, at most, on the observed values of variables in the model, not on the unobserved values of variables in the model (controlling for the observed values). The assumption behind listwise deletion is much more stringent: data are "missing completely at random," in other words, missingness does not correlate with the values of any other variables in the model. This assumption is false for the available data. However, if we do not have missing-at-random data, we cannot do data analysis at all (Honaker and King 2010:564). Thus, the missing-at-random assumption is relatively safe and, indeed, necessary for us to proceed.

We use the AMELIA II program to generate five new hypothetical datasets for each variable with missing data, run models on each dataset, and combine the parameter estimates of interest using the formulas described in Honaker, King, and Blackwell (2010). Our variable matrix includes every variable in the regressions presented here, as well as other variables thought to correlate with the variables that suffer from missingness and thus to assist in the imputation process: Internet users per 1,000 population, GDP growth rate, a dummy for Soviet-dominated countries, female life expectancy, fuel and mineral exports as a percentage of GDP,

and inward FPI stock. Due to skepticism about the utility of multiple imputation for extending series over a long period of time, we only generate values for the original temporal range of each variable.[8]

To ensure multiple imputation yielded plausible data, we conducted diagnostic checks. First, we tried "overimputing" each variable, dropping real data and allowing the program to generate hypothetical data for these observations, to determine whether the imputed data are a valid construct. Figures 1 and 2 are scatter plots of overimputed data against the corresponding actual observations for the inward FDI stock and GDP per capita variables, respectively. The bars represent 90% confidence intervals. The overimputed data track the actual data quite closely, although the figures draw inordinate attention to truly extreme values.[9]

[Figure 1 about here]

[Figure 2 about here]

Next, we also examined the density distribution of imputed data relative to actual data for each variable to ensure the program did not generate implausible values. Our data appendix includes four figures showing the kernel density distributions of actual and imputed data for the

[8] Even the imputed datasets feature unbalanced panels for two reasons. First, country-years under foreign occupation (Polity code "-66") are dropped, as these cases in a real sense do not represent independent states. Second, countries enter and exit the dataset when they gain and lose independence (for example, Belize, East Germany).

[9] Liberia accounts for the extreme observations on FDI. We have run all our models excluding Liberia, with no substantive differences in results.

variables inward FDI stock, GDP per capita, the Cingranelli and Richards (CIRI) index of physical integrity rights, and the Political Terror Scale (PTS). We found no anomalies in the imputed data for any variable. The human rights data become continuous after multiple imputation. While AMELIA II allows maintenance of ordinal data, the ordinal scales of human rights protection are actually only our attempts to quantify an underlying continuous concept in the first place, and the imputed observations faithfully represent the expected values of those variables. Forcing them into an ordinal coding would throw away useful information for our estimation models.

We present results using both listwise deletion and multiple imputation to illustrate the differences, which are not great in any event.

Models and Results

We now test the effects of FDI on human rights protection. Again, we prefer FDI stock over net inflow for testing structural dependence, but we present results with both, since the choice of variable turns out not to matter.[10] We have run analyses with GDP and gross fixed capital formation in the denominator of the FDI variables, with no substantial differences in results. We report results with gross fixed capital formation here.

Our two dependent variables are the standard measures of repression in the literature: the CIRI index of physical integrity rights, available online at ciri.binghamton.edu (Cingranelli and Richards 1999; Cingranelli and Richards 2010), and the Political Terror Scale (Wood and Gibney 2010), which averages the annual country scores produced by Amnesty International

[10] The FDI variables are always logged (for inflows, on either side of zero) to reduce skewness: if *Rawinflows*=0, *Newinflows*=0; if *Rawinflows*>0, *Newinflows*=ln(*Rawinflows*); if *Rawinflows*<0, *Newinflows*=-ln(-*Rawinflows*). This construction does not materially affect the results.

and the U.S. State Department. While both indicators use these same sources, they scale the components differently, and CIRI focuses on governmental actors only. CIRI data are available for 1981-2003 and PTS data for 1976-2003. On the CIRI index, higher scores indicate greater respect for physical integrity rights, while on the PTS, higher scores indicate greater repression. We invert the signs on the coefficients in the PTS models, so that positive coefficients consistently imply beneficial effects for human rights.

We run our models on the original dataset with listwise deletion and the multiply imputed dataset. In the former case, we use Tobit estimation because the dependent variables have upper and lower corner solutions (Wooldridge 2002:517). We favor this equation over ordered probit/logit because the coefficient estimates are easily interpretable and can be compared to the OLS results from the multiply imputed data. The imputed data are continuous, allowing use of OLS with panel-corrected standard errors (Beck and Katz 1995). In the Tobit models, we include dummies for each value of the lagged dependent variable to capture nonlinearities (repression cannot increase at its maximum or decrease at its minimum), while in the OLS models we simply include the value of the lagged dependent variable. Our control variables are logged real GDP per capita in Purchasing Power Parities, logged trade as a percentage of GDP, the Polity IV 21-point scale of democracy,[11] logged population, external armed conflict presence from the UCDP/PRIO Armed Conflict Dataset (Gleditsch, Wallensteen, Eriksson, Sollenberg, and Strand 2002) (dummy), intrastate armed conflict presence from UCDP/PRIO (dummy), civil war presence from Fearon and Laitin (2003) (dummy), interregnum

[11] Interregnums (Polity code "-77") are recoded zero.

(lack of a central government) from Polity IV (dummy), and a year counter.[12] We include two different measures of intrastate conflict because we expect higher-intensity conflicts (civil wars) to produce more repression, and because the two sources conceptualize civil war and armed conflict slightly differently.

Table 1 presents the results of four estimated equations: two for each dependent variable and two for each sample (listwise deleted and multiply imputed).[13]

[Table 1 about here]

FDI stock is not close to statistical significance in any of these models. However, all the standard results from the literature obtain in these models: democracy, development, internal peace, and small population promote human rights. The strength of some of these relationships is remarkable, with democracy and GDP exceeding statistical significance at the 99.99% confidence level. This is exactly what we would expect to see if including more relevant data caused our estimated equations to describe reality more accurately and precisely. The penultimate row reports variance inflation factors (VIFs) for FDI, showing that multicollinearity is not responsible for its insignificance (the square root of the VIF represents the inflation in a variable's standard error due to multicollinearity).

Next, we rerun these models with different controls and samples, using multiply imputed data. Table 2 shows the results. First, we use FDI inflows rather than stocks and add FPI inflows for good measure (its inclusion does not materially affect the coefficient on FDI). FDI

[12] All independent variables are lagged one year, except in first year of independence.

[13] We have also tried running these models with country and/or year fixed effects, and in no case was FDI close to statistically significant and positive.

inflows are not significant, but FPI inflows are. Thus, the difference between our results and virtually all prior results on FDI and repression is caused by a larger sample, not the choice of FDI stock over inflows. Second, we run the models on developing countries only, defined as country-years with GDP per capita below the sample mean. FDI stock is now negative. The only notable changes in the results are that trade correlates with better human rights, and GDP per capita no longer reduces repression, leading us to suspect that in the global sample there is some kind of "liberal democracy" syndrome among rich democracies that correlates with less repression, which the democracy variable alone does not pick up. Finally, we add FPI stock, with negligible effects.

[Table 2 about here]

The research suggests that FDI might be endogenous to the human rights situation. Following Blanton and Blanton (2007) in part, we try instrumenting FDI stocks with resource exports divided by GDP, female life expectancy, capital account openness (Chinn and Ito 2008), and Internet users per 1,000 in a two-stage least squares regression. These instruments are expected to correlate strongly and positively with FDI. We expect them to affect human rights only through the FDI channel. Table 3 gives the first-stage regression results for four models: both dependent variables, global and developing-country samples. The results are as expected, except that contra Blanton and Blanton, female life expectancy is strongly *negatively* associated with FDI, all else equal. What matters for the validity of the instruments, however, is that they are exogenous and strongly predict FDI, and the very good R-squareds (considering that no lagged dependent variable, which would be endogenous, is included) and strong statistical significance of the instruments support their validity.

[Table 3 about here]

Table 4 shows the second-stage results of these four models. FDI stock is insignificant in three models and wrongly signed in one of these. However, it does achieve statistical significance in one model. How could a negative partial correlation between FDI stock and human rights in Tables 1 and 2 become positive once FDI is instrumented? FDI is strongly *negatively* correlated with lagged human rights protection in the first-stage equation. In other words, to believe the result in the second model of Table 4, one would have to maintain simultaneously that repression attracts FDI, but FDI reduces repression. It would be difficult to construct a coherent story to explain this combination of findings, and the result is extremely fragile in any case (it also disappears completely if FPI stock is added).

[Table 4 about here]

Our results depart dramatically from the "new conventional wisdom" (that FDI is conducive to human rights protection) mainly because of more data. Even without multiple imputation, we expanded our dataset by roughly 100% or more over previous efforts by using new, better data sources and omitting certain variables with high missingness that some other studies included (education, aid, debt, etc.). Previous samples selected heavily on more developed and politically stable countries, whose governments may be more sensitive to MNEs' demands for stability and social peace. More representative samples include more years from highly peripheral countries, in which FDI's effects are at best unpredictable.

Conclusion

There is no evidence of a universal ameliorative effect of foreign investment on repression, and some results suggest an adverse effect of FDI. There may be some range of

circumstances under which foreign investment does have ameliorative effects. Future research might examine particular forms of FDI more likely to have beneficial consequences for physical integrity rights.[14] Blanton and Blanton (2009) find that U.S. MNEs in higher-skilled and more socially integrated sectors (chemicals and finance) are more likely to invest in countries with low repression, since repression can harm their labor force and create political instability. Given Western consumer interest in working conditions abroad, dependence on export to the West should also matter. MNEs with location substitutability (for example, most light manufacturing) should then have the greatest leverage over their host countries' governments. The combination of these traits may be necessary to encourage improvements in human rights.

Furthermore, as Blanton and Blanton (2007:149) assert, the direct effects of globalization on conflict or repression are not the whole story. To the extent that liberalization of markets and growth in global commerce encourage economic development, and economic development in turn promotes repression-reducing democracy, global market integration will have strong, positive, but highly indirect effects on human rights protection.

This paper's methodological lessons are twofold. First, we have argued that hypotheses linking foreign investment to government policy through a structural dependence mechanism should eschew investment flows variables in favor of some other indicator that more accurately reflects the importance of foreign investment in the domestic economy. Investment stocks are an obvious candidate; these data are now available. Studies of the political economy of the welfare state and taxation frequently test these kinds of hypotheses. Second, we have shown

[14] We thank an anonymous reviewer for stimulating our thinking on this point. Mihalache-O'Keef (2009) does something similar with FDI and internal violence.

that selection bias is an important concern in the literature on globalization's effects on domestic conflict and repression. The biases from listwise deletion, identified theoretically by statisticians, are realized in these studies. Fortunately, new data on foreign investment and other economic indicators are available, reducing our need to rely on the World Bank data. New tools for multiple imputation of data in TSCS models should also prove useful.

Governments repress when faced with political dissent that challenges their hold on power (Lichbach 1987; Davenport 2007). Scholars have investigated whether foreign economic ties can "disincentivize" repression, particularly through the withdrawal or withholding of valuable investment. Hafner-Burton (2005b) finds that preferential trade agreements with penalties for human rights violations successfully pressure governments to reduce repression, while Eriksen and de Soysa (2009) find that IMF and World Bank lending correlate with better human rights performance in indebted countries. If future studies confirm these results, then, faced with evidence from this paper, we must infer that governments are better able than private investors to monitor and sanction other governments for human rights violations. Another possibility is that only certain governments, perhaps those most sensitive to citizen demands for long-term growth, will pursue internal peace in order to attract FDI. For many other governments, the benefits of repression are worth whatever costs foreign investors in general might impose.

Variable Descriptions

- Ongoing civil war: Fearon and Laitin (2003), variable "war," updated via the source immediately below (war intensity only).

- Ongoing internal conflict: UCDP-PRIO Armed Conflict 2008 dataset (Gleditsch et al. 2002), conflict type "3" or "4," plus unclear cases
- External conflict: UCDP-PRIO Armed Conflict 2008 dataset, conflict type "1" or "2."
- FDI inflows: The FDI inflows data come from UNCTAD and are used to extend the stocks data.
- FDI stock as % of GDP: Inward FDI stock data come from UNCTAD. When extending the FDI and FPI stocks data with the inflows data, we progressively code as "0" any country-years for which estimated stocks are negative and allow formulas to update.
- FPI inflows: World Bank (2008), used to extend FPI stock data. The World Bank FPI inflow data can be constructed in two ways. The first is to take the "Portfolio Investment, LCFAR" variable and divide it by GDP. The second is to add "Portfolio investment, equity" and "Portfolio investment, bonds" and divide by GDP. The data are apparently derived from different sources and correlate highly but do not always match. We take the former measure and replace it with the latter when the former is missing. The World Bank measure of FPI, "portfolio investment excluding LCFAR (liabilities constituting foreign authorities' reserves)," is negative the larger are net portfolio investment inflows, because it is conceived as a balance of payments measure (investment inflows generate liabilities on the national balance sheet). We invert the measure so that the highest values represent the largest net inflows. The flows data are added to the stocks data for missing values of the latter prior to dividing by GDP.
- FPI stocks: Inward FPI stock data (portfolio investment liabilities) come from the IMF's International Financial Statistics unit. There are a few countries for which FPI stock data are missing altogether, and we code these countries by assuming zero FPI stock in 1970 (a fair

assumption in the late Bretton-Woods period for most countries) and then progressively summing over time the net FPI inflows data.

- GDP per capita: From Penn World Table 6.3, in 2005 international dollars. For defunct countries, we have used prior versions of the Penn World Table and converted to 2005 international dollars. However, for Czechoslovakia, we were able to create the needed data by creating a weighted average for Czech Republic and Slovakia for the years for which data were available prior to these countries' independence, then going back in time by using GDP growth data from Penn World Table 5.6. This procedure was necessary because of a marked discontinuity in PWT estimates for Czechoslovakia between versions.
- Log of country population: From Penn World Table 6.3.
- Democracy: From Polity IV 2007 update, 21-point measure of regime type (variable name "polity2"). Cases of foreign occupation (Policy code "-66") are treated as missing.
- Trade as % of GDP: Penn World Table 6.3, variable "openk." Earlier versions of PWT were used for defunct countries.
- Interregnum: dummy variable for regime type "-77" in Polity IV regime scale.
- Internet users: World Bank (2008), per 1,000 population. Assumed zero in 1990 and before.
- Capital openness: Chinn and Ito (2008).
- Resource exports: Fuel and mineral exports divided by GDP, from World Bank (2008).
- Female life expectancy: World Bank (2008).
- PTS (Political Terror Scale): for description see text, downloaded from www.politicalterrorscale.org.

- CIRI (Cingranelli and Richards physical integrity rights): for description see text, downloaded from ciri.binghamton.edu.

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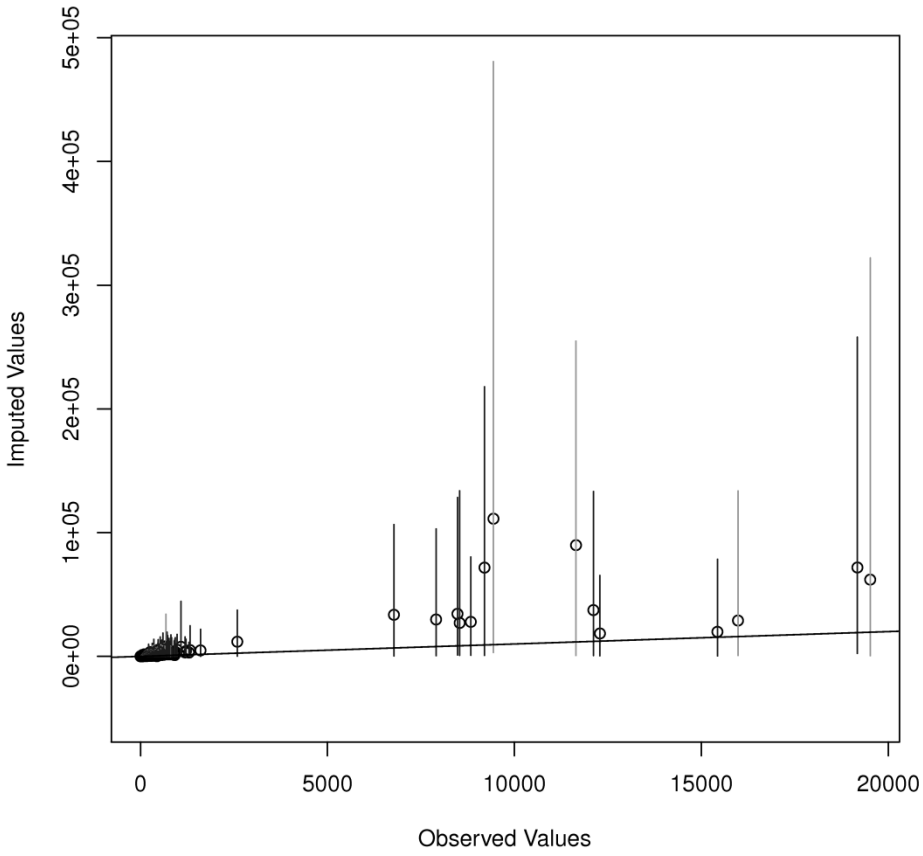

FIG 1. Overimputed versus Observed Values of Inward FDI Stock

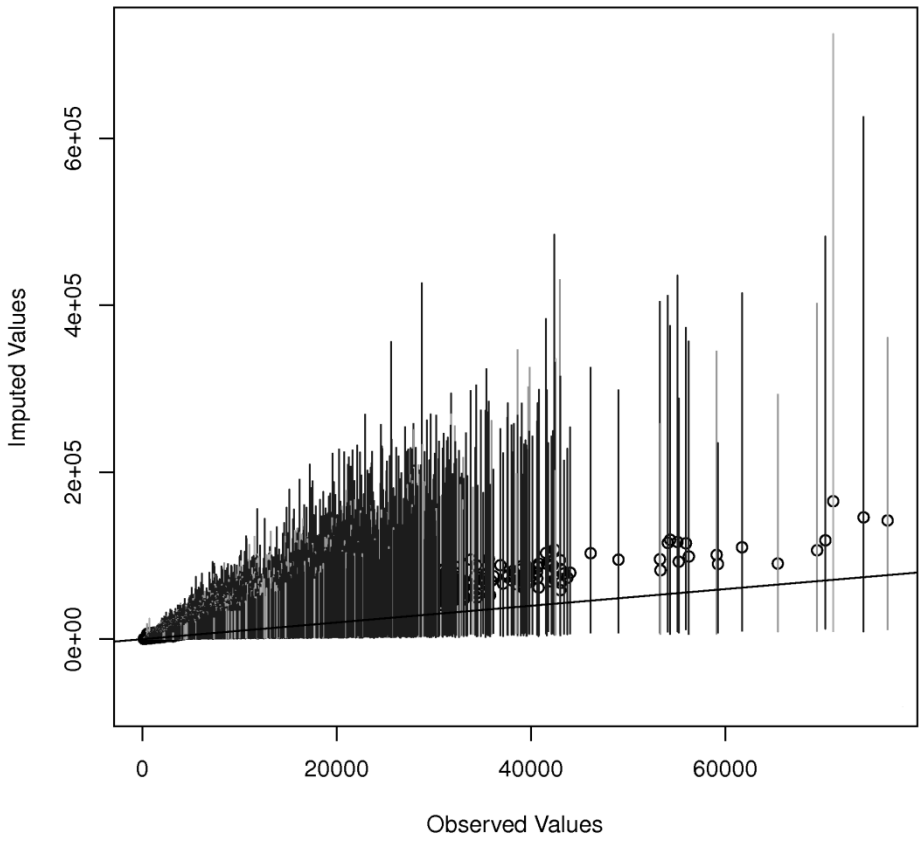

FIG 2. Overimputed versus Observed Values of GDP Per Capita

TABLE 1. Foreign Direct Investment and Repression

	Model 1	*Model 2*	*Model 3*	*Model 4*
Sample:	Listwise del.	Listwise del.	Imputed	Imputed
Dependent var.:	PTS	CIRI	PTS	CIRI
Equation:	Tobit	Tobit	OLS PCSE	OLS PCSE
Variable	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*
FDI stock, %GFCF	0.002 (0.007)	0.003 (0.019)	-0.004 (0.006)	-0.009 (0.014)
GDP per capita	0.06 (0.01)***	0.19 (0.03)***	0.06 (0.01)***	0.15 (0.03)***
Trade	-0.016 (0.019)	-0.03 (0.05)	0.007 (0.014)	0.02 (0.04)
Democracy	0.014 (0.002)***	0.038 (0.005)***	0.012 (0.002)***	0.037 (0.005)***
Internal conflict	-0.21 (0.04)***	-0.47 (0.10)***	-0.18 (0.04)***	-0.43 (0.10)***
Civil war	-0.18 (0.04)***	-0.50 (0.11)***	-0.15 (0.04)***	-0.46 (0.11)***
External conflict	0.004 (0.06)	-0.13 (0.16)	-0.008 (0.05)	-0.21 (0.14)
Population	-0.044 (0.009)***	-0.15 (0.02)***	-0.041 (0.008)***	-0.14 (0.02)***
Interregnum	-0.24 (0.09)**	0.5 (0.8)	-0.10 (0.08)	-0.15 (0.22)
Year	-0.007 (0.002)***	-0.020 (0.005)***	-0.006 (0.002)**	-0.015 (0.006)*
N (countries)	3598 (160)	2972 (157)	3869 (163)	3326 (163)
VIF (FDI)	1.24	1.21	1.23	1.21
R^2/Pseudo R^2	48.86%	28.96%	79.30%	71.95%

(*Notes*: ****p*<.001 ***p*<.01, **p*<.05, two-tailed tests. Results on constant and lagged dependent variable(s) not reported. Robust standard errors with clustering on countries reported in the Tobit models.)

TABLE 2. Sensitivity Analyses

	Model 5	*Model 6*	*Model 7*	*Model 8*	*Model 9*	*Model 10*
Sample:	Global	Global	Developing	Developing	Developing	Developing
Dependent var.:	PTS	CIRI	PTS	CIRI	PTS	CIRI
Variable	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*
FDI stock, % GFCF			-0.011 (0.007)	-0.020 (0.017)	-0.013 (0.007)	-0.028 (0.018)
FDI inflow, %GFCF	-0.006 (0.007)	0.015 (0.017)				
FPI stock, %GDP					0.023 (0.014)	0.086 (0.045)
FPI inflow, %GDP	0.52 (0.13)***	1.1 (0.4)**				
GDP per capita	0.05 (0.01)***	0.14 (0.03)***	-0.017 (0.016)	-0.041 (0.044)	-0.023 (0.017)	-0.065 (0.047)
Trade	0.007 (0.014)	0.01 (0.04)	0.05 (0.02)**	0.10 (0.05)*	0.05 (0.02)*	0.087 (0.050)
Democracy	0.012 (0.002)***	0.036 (0.005)***	0.011 (0.002)***	0.036 (0.006)***	0.011 (0.002)***	0.036 (0.006)***
Internal conflict	-0.18 (0.04)***	-0.43 (0.10)***	-0.19 (0.05)***	-0.33 (0.10)**	-0.19 (0.05)***	-0.33 (0.10)**
Civil war	-0.15 (0.04)***	-0.47 (0.11)***	-0.20 (0.05)***	-0.66 (0.12)***	-0.20 (0.05)***	-0.66 (0.12)***
External conflict	-0.01 (0.05)	-0.22 (0.14)	0.03 (0.06)	-0.09 (0.15)	0.03 (0.06)	-0.09 (0.15)
Population	-0.04 (0.01)***	-0.14 (0.02)***	-0.05 (0.01)***	-0.18 (0.03)***	-0.06 (0.01)***	-0.20 (0.03)***
Interregnum	-0.11 (0.08)	-0.14 (0.22)	-0.23 (0.09)**	-0.38 (0.23)	-0.23 (0.09)**	-0.38 (0.23)
Year	-0.006 (0.002)***	-0.018 (0.006)**	-0.007 (0.002)**	-0.018 (0.007)**	-0.007 (0.002)**	-0.020 (0.007)**
N (countries)	3869 (163)	3326 (163)	2455 (120)	2102 (118)	2455 (120)	2102 (118)
VIF (FDI)	1.22	1.25	1.18	1.16	1.23	1.21
R^2	79.35%	72.01%	72.02%	63.90%	72.05%	63.97%

(*Notes*: ****p*<.001 ***p*<.01, **p*<.05, two-tailed tests. Results on constant and lagged dependent variable(s) not reported. OLS estimates with panel-corrected standard errors.)

TABLE 3. Two-Stage Least Squares Models: First-Stage Results

	Model 1	*Model 2*	*Model 3*	*Model 4*
Sample:	Global	Global	Developing	Developing
Dependent var.:	FDI stock	FDI stock	FDI stock	FDI stock
Variable	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*
PTS_{t-1}	0.11 (0.03)***		0.12 (0.04)**	
$CIRI_{t-1}$		-0.042 (0.016)**		-0.052 (0.020)**
GDP per capita	-0.29 (0.04)***	-0.29 (0.04)***	-0.02 (0.06)	-0.001 (0.07)
Trade	0.20 (0.04)***	0.25 (0.05)***	0.16 (0.05)**	0.23 (0.06)***
Democracy	0.064 (0.004)***	0.059 (0.004)***	0.039 (0.005)***	0.037 (0.006)***
Internal conflict	-0.06 (0.09)	-0.14 (0.09)	-0.12 (0.11)	-0.23 (0.11)*
Civil war	-0.10 (0.09)	-0.02 (0.10)	-0.12 (0.11)	-0.04 (0.12)
External conflict	-0.06 (0.13)	-0.34 (0.15)*	0.23 (0.16)	0.02 (0.17)
Population	-0.12 (0.02)***	-0.08 (0.02)***	-0.15 (0.03)***	-0.11 (0.03)***
Interregnum	-0.46 (0.18)*	-0.33 (0.19)	0.04 (0.21)	0.21 (0.22)
Year	0.042 (0.004)***	0.038 (0.005)***	0.047 (0.005)***	0.042 (0.006)***
Internet users	0.12 (0.02)***	0.13 (0.02)***	0.068 (0.040)	0.084 (0.042)*
Capital openness	0.18 (0.02)***	0.20 (0.02)***	0.18 (0.03)***	0.20 (0.03)***
Resource exports	0.27 (0.02)***	0.26 (0.02)***	0.28 (0.02)***	0.27 (0.02)***
Female life exp.	-0.015 (0.004)***	-0.016 (0.004)***	-0.016 (0.004)***	-0.018 (0.004)***
N (countries)	3869 (163)	3326 (163)	2455 (120)	2102 (118)
R^2	26.22%	24.85%	23.02%	21.91%

(*Notes*: ****p*<.001 ***p*<.01, **p*<.05, two-tailed tests. Results on constant not reported. OLS estimates.)

TABLE 4. Two-Stage Least Squares Models: Main Results

	Model 1	*Model 2*	*Model 3*	*Model 4*
Sample:	Global	Global	Developing	Developing
Dependent var.:	PTS	CIRI	PTS	CIRI
Variable	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*	*Coef. (SE)*
FDI stock, %GFCF	0.023 (0.016)	0.10 (0.05)*	-0.001 (0.021)	0.069 (0.054)
GDP per capita	0.060 (0.011)***	0.17 (0.03)***	-0.017 (0.016)	-0.04 (0.04)
Trade	-0.002 (0.014)	-0.020 (0.045)	0.05 (0.02)**	0.064 (0.051)
Democracy	0.011 (0.002)***	0.033 (0.005)***	0.011 (0.002)***	0.034 (0.006)***
Internal conflict	-0.18 (0.04)***	-0.42 (0.10)***	-0.19 (0.05)***	-0.31 (0.10)**
Civil war	-0.15 (0.04)***	-0.47 (0.11)***	-0.20 (0.05)***	-0.66 (0.12)***
External conflict	-0.008 (0.05)	-0.19 (0.14)	0.03 (0.06)	-0.09 (0.15)
Population	-0.04 (0.01)***	-0.13 (0.02)***	-0.05 (0.01)***	-0.17 (0.03)***
Interregnum	-0.09 (0.08)	-0.12 (0.22)	-0.23 (0.09)**	-0.39 (0.23)
Year	-0.007 (0.002)**	-0.022 (0.007)**	-0.008 (0.002)**	-0.024 (0.008)**
N (countries)	3869 (163)	3326 (163)	2455 (120)	2102 (118)
R^2	79.30%	71.98%	71.99%	63.90%

(*Notes*: ****p*<.001 ***p*<.01, **p*<.05, two-tailed tests. Results on constant and lagged dependent variable(s) not reported. 2SLS estimates with panel-corrected standard errors.)