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Foreign Direct Investors: Agents of Economic Transition an Instrumental Variables Analysis*

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ABSTRACT

Previous empirical analysis has noted a correlation between Foreign Direct Investment (FDI) and economic reform in Eastern Europe and Former Soviet Union, but has attributed the relationship to investors rewarding countries after reform decisions. Little attention has been paid to the fact that investors' lobbying efforts may actually influence reform choices. This paper finds a positive effect of FDI on reform progress through a panel analysis of investor influence in 27 transition states (1991–2004). To address endogeneity bias, the exogenous portion of a country's exchange rate movement is used as an instrument in a two-stage procedure. The underlying counterfactual comparison that results from this approach is between two similarly situated countries, but where one country experienced a large shift in the share of FDI in its economy as a result of changes in the international economy and the other did not. Further analysis reveals that the relationship is particularly strong in the manufacturing and service sectors, but does not hold for construction, utilities, or natural resource based projects.

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> "Multinational Companies . . . played a direct role in changing attitudes at lower levels of bureaucracy, as a result of their daily interaction with officials, and their long slog to improve laws that were drafted in ignorance or haste" Charles Paul Lewis in *How the East Was Won: The Impact of Multinational Corporations on Eastern Europe and the Former Soviet Union* (2005, p. 185).

The statement above from the *Economist's* Eastern European correspondent is doubly provocative because it challenges current thinking in two distinct political science literatures. Students of international political economy (IPE) will take note of it, because that literature has predominantly studied foreign direct investment (FDI) as an outcome of economic reforms, not as a cause (Gastanaga *et al*. 1998, Wei 2000). Those studying economic transition will take issue with Lewis' focus on a relatively short-run causal impetus and not the deep causal mechanisms, such as the initial political and economic conditions for development, or the middle-range political institutions, partially shaped by historical legacies, that have received pride of place in their work (Frye 2007).[1]

Most scholars of economic transition note the strong correlation between the stock of FDI and total economic reform, as measured by the European Bank of Reconstruction and Development (EBRD), but they generally attribute the association to FDI flowing-in to reward reform progress (Lankes and Stern 1999, Jensen 2002, Campos and Kinoshita 2003, Bevan *et al*. 2004, Dunning 2005). There has been comparatively little study of potential reverse causality — that through lobbying and information provision, investors may have a positive effect on reform choices. While there is little doubt that some portion of the stock of FDI in transition economies was attracted by reform initiatives, the fact that the correlation persists even when the stock of FDI is lagged by a decade or more should raise doubts that the depiction of FDI following reform is the complete story.

Anecdotally, FDI has streamed into countries such as Kazakhstan and Azerbaijan, China, and Vietnam; a process that represents the very antithesis of reform as a determinant. At the time of their respective FDI surges, these regimes had made relatively little reform progress in comparison to their peers, especially in the area of property rights protection. *Ad hoc* explanations are available; opportunities for gains in oil lubricated decision-making in Azerbaijan and Kazakhstan, while China and Vietnam offered cheap labor and export platforms. Nevertheless, these explanations confirm the fact that business opportunities drive a great deal of investment decisions. FDI was willing to assume a certain degree of political risk based on the expected returns of their investment, and investors felt they could even ameliorate that risk by working closely with government actors (Hahn 1999, Hillman and Hitt 1999). As Hewko (2003) puts it, most investors

Jeff Frieden, Michael Hiscox, Lawerence Broz, Craig McIntosh, Bob Keohane, Herbert Kistchelt, Timothy Frye, Scott Gelhbach, Melanie Manion, Helen Milner, Tim Buthe, Pablo Pinto, Quan Li, Layna Mosely, Sara Brooks, Top Pepinsky, Bumba Mukherjee, James Vreeland, Shankar Satyanath, Nathan Jensen, Glenn Biglaiser, Heiner Schulz, two anonymous referees, and the editors of QJPS.

[1] Frye 2006 offers a helpful review of these two branches of the transition research agenda. For more on legacies see (De Melo *et al*. 1997, Kitschelt 2001, and Pop-Eleches 2007). For more on middle-range institutions see (Fish 1999, Frye and Mansfield 2003).

were willing to ignore problems in legislation and legal system if they had a visceral *feel good* perception of the target country.

Furthermore, once invested, multinational investors did not view themselves as passive actors in the reform process. International Chamber of Commerce meetings, foreign investor magazines, and conferences on regulatory and commercial development abound in transition countries (Hewko 2003, Gillespie 2006). Given the IPE literature's focus on investment following reform, the presence of lobbying is puzzling. The costs both in money and organizational efforts of lobbying activities are substantial, so why would investors pursue them if they have no influence on reform in host countries? One reason is that Foreign Invested Enterprises (FIEs) believe that they can indeed shape the policy environment (Desbordes and Vauday 2007).

Two mechanisms appear most critical for their success (Hewko 2003): (1) the ability to provide policy-makers, who are new to technical economic and regulatory issues, with information on laws in other countries in which the FIE operates (Prakash and Potoski 2007, Luo 2002); and (2) the ability to coerce policy-makers by threatening to leave for more hospitable investment environments, depriving the country of employment and tax revenue (Olarreaga 1999). As Lewis notes succinctly, "Since 1989 multinationals have brought their attitudes and economic power, and in doing so have become an effective force . . . (Lewis 2005, p. 185)."

Political economists studying the US have noted the important influence of business lobbying on policy choices (Grossman and Helpman 1994). Gordon and Hafer (2005) find that business lobbying can affect the implementation of national policies, while Blonigen and Figlio (1998) find the evidence of FDI influencing individual legislator behavior in the US. These findings, however, have not been extended to the influence of foreign investors in transition or developing economies, where their aggregate bargaining strength and consequently their impact is even stronger. Due to the present paradigm of economic transition that emphasizes rational payoffs for the development of economic institutions (Weingast 1993, Henisz and Williamson 1999), scholars have neglected to problemmatize the role of foreign investment in shaping economic institutions in the first place.

In this paper, I test the independent effect of FDI on economic reform with a panel analysis of investor influence in 27 transition states. Indicators of economic transition from the EBRD are regressed on changes in the stock of FDI relative to GDP, controlling for alternative hypotheses.

> H1: *Ceteris Paribus,* the more important foreign investment is in a transition country's economy (and therefore the stronger the bargaining power of the FDI community as a whole), the more likely the country is to pursue economic reform.

Despite the straightforward hypothesis, the test is complex due to the endogeneity of FDI. As noted above, the predominant trend in the literature is to portray reform as causing inflows of FDI. In testing the opposite causal relationship, I cannot simply ignore those theories and findings.

Building upon an instrument first identified by Alfaro *et al*. (2004), I use an IV-2SLS approach to address the endogenous relationship by instrumenting FDI with the predicted nominal exchange rate of the transition countries, based on movements in baskets of Organization of Economic Cooperation and Development (OECD) currencies. In short, I take the part of the local exchange rate that is determined by international economic forces, rather than domestic political manipulations. Because the exchange rates of OECD countries are exogenous to political processes in the transition states, this variable makes a useful and statistically strong instrument.[2]

The underlying counterfactual comparison that results from this approach is between two similarly situated countries, but where one country experienced a large shift in the share of FDI in its economy as a result of exogenous changes in the international economy and the other did not. Using this technique, I find that a 1% increase in the stock of FDI is associated with nearly a 6.3% increase in economic reform in the subsequent year. Further analysis reveals that manufacturing and service sector investment are primarily responsible for the reform effect. Investment into natural resources, construction, and utilities is not significantly associated with reform choices.

The analysis focuses on transition states (Eastern Europe and Former Soviet Union) due to the presence of a consistent measure of economic reform among countries and comparable reform goals; however, one contribution of this paper is to demonstrate that predicted exchange rates can be employed in a range of projects involving the political impact of FDI.

DATA, MEASUREMENT, AND SPECIFICATION

The dependent variable for this analysis is the EBRD ranking of countries from 1 to 4.3 on 8 different economic reform policies between 1992 and 2004, the most commonly used indicator of reform in the transition literature (Falcetti *et al*. 2005). A score of 4.3 is thought to denote the level of a typical advanced industrial economy. The eight reform policies include price liberalization, foreign exchange and trade liberalization, privatization of small state owned enterprises (SOEs), privatization of large SOEs, enterprise reform/corporate governance, competition policy, bank reform, and reform of nonbank financial institutions.[3] In the panel analysis, I standardize the aggregate score to a 0–100 point scale and take the first difference in reform progress as my dependent variable in order to avoid spurious correlation caused by nonstationarity.[4]

Key Causal Variable — Foreign Direct Investment: I measure the cumulative stock of foreign direct investment as a percentage of GDP in the economy based on the United

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[2] The technique of using the predicted values from a previous analysis in a subsequent IV-2SLS was utilized by Blanchard *et al*. (1992) in their well-known assessment of employment adjustments across US States.

[3] For a complete discussion of these see the annual EBRD Transition Reports (1994–2004).

[4] Transition indicators were first presented in 1994, but backdating to 1992 did not occur until the year 2000. This means that early years of transition must be treated with caution.

Nations Conference on Trade and Development database, after confirming that the data was highly correlated with other commonly-used measures (UNCTAD 2005). My key causal variable is annual change in the stock of FDI as a percentage of GDP.[5] Note that, the annual change in the stock of FDI/GDP is not necessarily equivalent to measuring FDI flows. The goal is to examine changes in the relative bargaining power of foreign investors over-time. While a decline in FDI flows can lead to a decrease in the ratio, so too can increases in domestic production and investment. In either case, FDI should have a weaker voice in relevant policy debates. Flows have been used as a measure of FDI attraction in many of the studies cited above, but they are not appropriate for this analysis because they capture only single-year surges and not relative changes in existing bargaining power.[6]

Figure 1 demonstrates the strong bivariate correlation (0.50) between the stock of foreign direct investment (FDI) over GDP in 1994 and total economic reform,

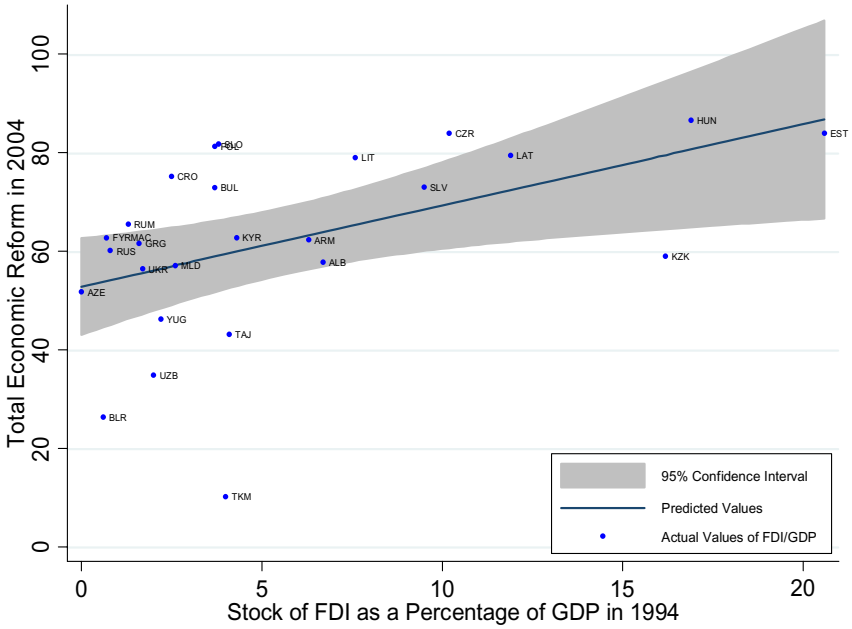

Figure 1. Relationship between FDI in 1994 and economic reform in 2004.

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[5] The natural log of FDI/GDP is taken to minimize the skewness caused by outliers, such as Estonia, Hungary, Azerbaijan and Kazakstan. Without the natural log the relationship would appear to be much stronger.

[6] I further test the robustness of the bargaining power argument, by analyzing whether the same relationship holds when cumulative stocks of FDI are standardized by the size of the population and fixed capital formation. Please see the *Online Appendix* Table A5.

as measured by the European Bank of Reconstruction and Development (EBRD) in 2004 — a full decade later.[7]

Data on FDI in transition countries is only available in aggregate for all countries over the entire time period due to significant reporting differences across countries (IMF 2006). In a further test of differences in investor behavior across sectors, I rely on a dataset from the *Wiener Institute fur Internationale Wirtshaftsgleiche* (WIIW 2006). The WIIW set is limited to 14 countries with varying time-series, but it offers some support for the generalizability of the core findings.

Control Variables: Because FDI is likely to be correlated with other reform determinants, omitted variable bias is a possibility, necessitating the addition of variables capturing alternative theories from the transition literature.[8] First, Hellman (1998) theories that because coalition-oriented political systems, such as parliaments, must include a wide cross-section of society in order to win election, they are more likely to include some potential losers from particular reform sequencing and are less likely to be captured by early winners. Witold Henisz (2000) creates a useful measure for Hellman's argument by combining the number of political constraints on executive decision-making, modified by the party composition of the different branches of government. These *Political Constraints* are analogous to the concept of veto points pioneered by Tsebelis (2002) and used by Frye and Mansfield (2003) in their analysis of trade reform in the region.[9] A third political control (*Postcommunists*) is the number of seats in the highest branch of the national parliament occupied by unreformed communist successor parties. If high numbers of communist leaders continue to be elected, it is an indication that significant portions of the population, including pensioners and former state owned enterprise employees, support a slower approach to economic reform (Norgaard 2000). High seat proportions held by communists can also exacerbate polarization, which also has been shown to waylay reform efforts (Frye 2002). At the extremes, communists average over 30% of the seats in Moldova and Uzbekistan between 1992 and 2004 (Armingeon and Careja 2004).

Incentive for accession to European Union provides another medium-term pathway for economic reform (Vachudova 2005) by providing a reward structure and *off-the-shelf* regulatory models for reformers (Mattli and Ploemper 2000). A four-point categorical variable is used to track the level of association or membership a country has with the

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[7] This figure is confirmed by multiple regression analysis that includes controls for initial conditions and a range of time varying factors. These can be found in the *Online Appendix* Table A3.

[8] Please see the Online Appendix accompanying this article for details about the variables. *Online Appendix* Table A1 offers a complete breakdown of the descriptive statistics and source data for variables used in the analysis. *Online Appendix* Table A2 reports the bivariate correlations between all variables used in the analysis.

[9] While political constraints and democracy may be seen as conceptually distinct, in practice their measurement is highly correlated. The Polity measure of democracy and political constraints are significantly correlated at the 0.73. Moreover, one of the three component variables of the polity democracy score is constraints on executive decision-making (xconst). In Table 4, I show that results do not change when the polity democracy score is substituted for political constraints.

European Union and measures the impact of pull the EU has on economic reform motivations. The scale includes (4) Member, (3) Candidate/Application Received, (2) Associate Member/Potential Member, (1) Leadership has expressed formal desire to join, and (0) Neither the country or the EU has formally expressed a desire for the country to join.[10] Robustness tests employ dummy variables for all levels of EU accession status in order to compare countries at similar levels of pull. *War* is a dichotomous variable measuring whether the country was at war during a particular year in the time series (Horowitz 2003).

One dilemma of analyzing changes in economic reform is illustrated by the bar graph in Figure 2. Notice that the major leaps in economic reform were made very early in the transition period and decline over-time. This problem is an artifact of both the transition process and the EBRD coding methodology. As Kitschelt (2001) shows, transition countries raced ahead on easy, politically palatable reforms such as price and trade liberalization, but were slower to enact deeper, institutional changes, which were blocked by entrenched interests — who, in some cases, rose to prominence as a result of the earlier reform efforts (Hellman 1996). A second problem is the EBRD's ceiling of 4.3, marking a country's rise to the level of a *western, capitalist system*. Obviously, the closer a country is to that ceiling in a particular reform area the more difficult subsequent reforms become. I capture this ceiling effect by using fixed effects for the number of years that a country

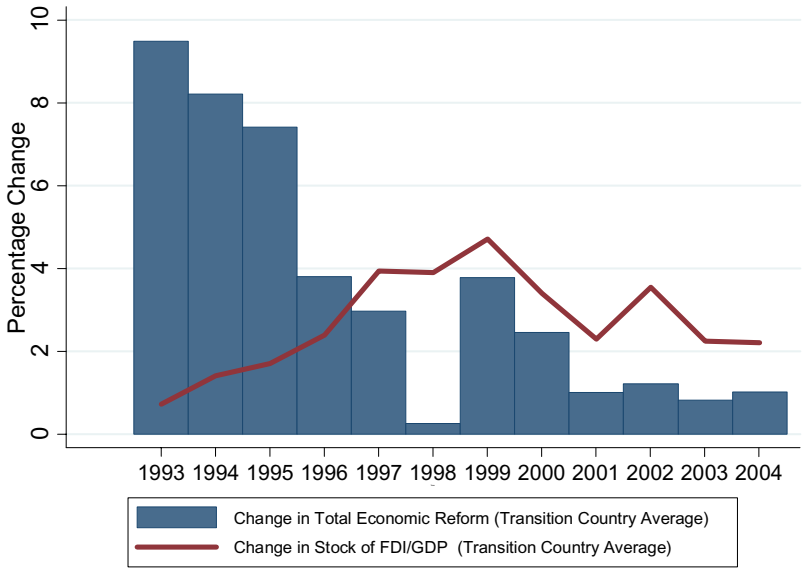

Figure 2. Changes in FDI stock and economic reform (1992–2004).

[10] Milestone dates obtained from the European Commission website on Enlargement. http://ec.europa.eu/enlargement/index_en.htm (Accessed on June 7, 2007).

has been engaging in reform under the assumption that latter years should be significant and negative. A robustness test achieved similar results by using a lagged measure of reform to capture previous reform progress as a proxy for ceiling effects.

Specification: All panel specifications make use of country fixed-effects. These models analyze the impact of annual changes in the relative bargaining power of FDI within individual countries. Cross-sectional variation in initial conditions and unobservables is absorbed by the country dummies included in the model.

Second, many of the variables in the model demonstrate trending over-time. There is a good reason for this; economic reform was only one of the multiple transitions taking place in these countries. Many countries were becoming more democratic, hardening institutions, and shedding the strength of communist successor parties over the same period. Because these changes were taking place simultaneously with economic reform, there is a strong possibility of confusing such trending with a causal relationship — a mistake known as spurious correlation. The standard econometric approach is to de-trend nonstationary variables by using the first difference. I ran the Hadri Lagrange Multiplier Test for panel models on all variables (Hadri 2000).[11] In cases of significant nonstationarity, I use the first difference of the variable, as noted in the regression tables.[12] When tests revealed auto-correlation of residuals in the models, violating standard OLS assumptions, a panel-specific AR1 process was applied.

NAÏVE MODELS WITHOUT ACCOUNTING FOR ENDOGENEITY

I begin the analysis by first demonstrating the correlation of FDI and economic reform in a straightforward panel framework. Some scholars interested in the political impact of FDI have tried to address the problems of FDI and omitted variable bias by simply lagging FDI one year (Li and Reuvny 2003, Rudra 2003), assuming that FDI at time $t - 1$ will influence reform at time t. Following their lead, I employ this technique in pooled, time-series model with Panel Corrected Standard Errors (PSCE) to account for contemporaneous (spatial) correlation of the errors across panels, as it is possible reforms in one country may affect reform progress in neighbors (Beck and Katz 1995).

Results are shown in Table 1, where Model 1 demonstrates the bivariate relationship, Models 2 and 3 add time varying controls, and Model 4 employs year fixed-effects to address the ceiling effects discussed above. Models 5 insures that results are not an artifact of the PCSE framework by using an OLS analysis with errors clustered at the panel level. Finally, Model 6 drops Azerbaijan due to its extraordinarily high oil investment (FDI stocks were over 150% of GDP in 2004). In the fully specified Model 4, a 1% change in FDI is associated with about a 1% increase in economic reform progress.

I am careful to use the term associate rather than affect or influence because using a simple lag is insufficient for three reasons. First, the lag cannot eliminate the possibility

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[11] Implemented in *STATA* using *hadrilm*.

[12] Engle-Granger two step tests demonstrated that cointegration of the de-trended variables did not pose significant problems for this analysis.

Table 1. Panel analysis of the effect of foreign direct investment on economic reform.

Dependent variable: Annual change in total economic reform (measured by EBRD scores)	Panel corrected standard errors				Robust	
	FDI	Controls	Controls 2	Time FE	Clustered errors	Dropping Azerbaijan
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Change in stock of FDI/GDP (In, lag)	1.164	1.524	1.477	1.044	1.283	1.262
	(0.568)	(0.560)	(0.552)	(0.512)	(0.777)	(0.556)
Seats held by communist successor parties (d)		−0.530	−1.283	−2.038	−1.907	−1.602
		(1.799)	(1.686)	(1.997)	(2.582)	(2.023)
Political constraints (d)		2.165	1.692	0.275		0.355
		(2.675)	(2.722)	(2.228)		(2.209)
War dummy		3.468	3.400	0.167	0.542	0.166
		(0.696)	(0.695)	(0.631)	(1.570)	(0.655)
Relationship with EU (d)		0.599	0.488	−0.361	−0.362	−0.271
		(0.461)	(0.448)	(0.272)	(0.792)	(0.277)
GDP per Capita (In, d)			−15.102	8.622	8.411	11.594
			(6.285)	(6.193)	(9.213)	(7.023)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	No	No	No	Yes	Yes	Yes
Drop Azerbaijan	No	No	No	No	No	Yes
EU fixed effects	No	No	No	No	No	No
Observations	111	277	277	277	244	268
Number of panels	27	27	27	27	26	26
R-squared	0.082	0.117	0.129	0.425	0.421	0.431
Root mean squared error (RMSE)	4.132	4.094	4.083	3.413	3.597	3.419

Note: Panel Corrected Standard Errors in Parentheses (Models 1–4, 6). Models 1–4 and 6 fit an AR1 process to the model to address serial correlation. Model 5 clusters robust standard errors at provincial level. Due to nonstationary (tested using the Hadri (2000) Lagrange Multiplier), the first difference of some variables was taken. This is denoted by (d). One-year lagged variables denoted by (lag). If natural log taken, (In) is used.

Due to some missing observations this is an unbalanced panel model. Because the lagged first difference of FDI is employed, the analysis begins with reform in the year 1993. In a perfect world this would leave 11 years of observations for 27 countries and therefore 297 total observations. Unfortunately, seven countries (including Azerbaijan) do not have sufficient FDI data in 1992, three countries lack FDI data in 1993, and Bosnia and Serbia do not receive EBRD scores until the year 2000. These omissions reduce the total number of observations to 277. Because fixed effects are employed, missing data should does not affect analysis.

that measured FDI at time t responded to reforms in previous periods, or even more problematic, the promise of reforms at previous periods. Granger causality tests do not satisfactorily resolve the problem, as lagged values of FDI and reform are both correlated with future reform and FDI, respectively. Second, there is a problem of strategic interaction. Foreign investors may select one country over another based on an *ex-ante* perception of their ability to influence reforms (Henisz and Delios 2004), so that actual reforms in the future ($t + 1$) actually influence investment in the present (t). Third, even in a well-specified model, there is a clear danger of omitted variable bias. Some unobserved factor may influence both the decisions of investors and government actors, leading to correlation between the two that has little to do with a direct causal relationship. Country fixed effects address this unobserved heterogeneity somewhat, but not entirely.

To truly resolve this dilemma, we need a measure of FDI that is independent of economic reform — FDI that can be shown to have entered an economy for reasons other than reform progress. If we can find such a measure, we can then identify the exogenous effect of FDI on the economic reform decisions of local governments.

IDENTIFICATION STRATEGY — PREDICTED EXCHANGE RATES AS AN INSTRUMENT FOR FDI

Isolating an exogenous change in FDI stocks requires a two-stage approach with an instrumental variable. The basic strategy in instrumental variable estimation in a panel framework is to find an estimator (z) that is both contemporaneously uncorrelated with the error term from the original model and that is correlated (preferably highly so) with the regressor for which it is to serve as an instrument (Bound *et al*. 1995). In panel models, finding this variable is made more difficult by the fact the instrument must also vary over time. If a researcher is able to identify such a variable, however, it is possible to solve a modified form of the traditional regression equation and thereby identify the ϕ parameter — the independent effect of FDI on Reform.[13]

Fortunately, the economics literature provides a useful instrument — exchange rates (Alfaro *et al*. 2004). Empirically, one of the most consistently robust determinants of FDI inflows in developing countries is real exchange rate fluctuations. Froot and Stein (1991) demonstrated that imperfect capital markets mean that the multi-national corporations (MNCs) in developed countries face internal capital costs that are lower than borrowing from external sources. Currency appreciation relative to another country leads to increased firm wealth that provides MNCs with greater low-cost funds to invest relative to counterpart firms in the foreign country experiencing the devaluation. More recently, Blonigen (1997) hypothesizes a similar change in investment resulting from currency

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[13] $\hat{\varphi} = \dfrac{\sum_{i=1}^{n} (z_i - \bar{z})(\text{Reform}_i - \overline{\text{Reform}})}{\sum_{i=1}^{n} (z_i - \bar{z})(\text{FDI}_i - \overline{\text{FDI}})}$

appreciation, but offers a different causal story. He tests the proposition that a depreciation of a foreign country's currency will lower the price of the asset for an MNC, but will not necessarily lower the nominal returns. In short, currency depreciation will lead to a *fire sale* of transferable assets to foreign firms operating in the global markets as opposed to domestic firms without international access. Other economic studies confirm Blonigen's hypothesis that inward FDI responds to short-run real exchange rate movements (Klein and Rosengren 1994, Kogut and Chang 1996). Recent history also offers compelling evidence. As the US dollar slid in 2007, foreign investors poured $414 billion into US factories, companies, real estate, and equities, a 90% increase from 2006. Some companies, like German ThysenKrupp, specifically cited the exchange rate as motivation for their investment decisions (Goodman and Story 2008). As a result, Alfaro *et al*. (2004) employ real exchange rate fluctuations in their general equilibrium model of FDI and economic growth to great effect.

Separating the Politics Out of Exchange Rate Fluctuations

It would be convenient if we could simply take movements in real exchange rates as the instrument and move directly to the analysis, but unfortunately exchange rates are not solely exogenous to economic reform. A wealth of political science literature has shown that exchange rates are the product of domestic political manipulation in order to achieve economic goals and therefore highly related to the dependent variable of economic reform (Leblang 1999, Frieden 1997). Most relevant to this paper is the work of Christina Bodea in illustrating that reformed communist parties in transition countries use currency commitments to signal their willingness to become respectable, mainstream political organizations that can carry out stabilization policy (Bodea 2004).

Fortunately, only a portion of the exchange rate is set by domestic processes. Exchange rates are also set by demand for currencies on international markets. This is especially true of floating exchange rate regimes, but intermediate mechanisms (i.e., crawling pegs and fluctuation bands), which are the dominant regime choices among transition countries, must also respond to shifts in international prices with a slight lag in the response time. Pegged rates are the least responsive and should theoretically be least correlated with FDI, but even under this regime devaluations will take place to accommodate international pressure on the currency (Eichengreen *et al*. 1995). Slovakia, for instance, devalued its currency five times between 1993 and 1998 (Bodea 2004, p. 37). Foreign investors are just as likely to respond to fire sales based on international price pressure as they are domestic exchange rate manipulation. Thus, by separating the exchange rate movements based on international price movements from the domestic determinants of exchange rate choice, it is possible to create an instrument for FDI exogenous to economic reform that can be used in the IV-2SLS procedures.

Step 1: To separate local determinants of exchange rate movements from international price fluctuations, I first need a proxy for changes in international prices. Alfaro *et al*. (2004) use only the dollar exchange rate, but the international business cycle

co-movement literature has shown persuasively that the dollar is only one of several international price drivers over the past two decades (Kose *et al*. 2008, Faia 2007). It is possible, therefore, that a transition country currency may be strongly influenced by international price movements but uncorrelated with the dollar. In these cases, using the dollar would explain little variance in particular currency and would therefore be unhelpful in identifying exchange rate influenced foreign investment in the next stage of analysis, even though such FDI actually exists in those cases.

Rather than coding currency baskets exactly as the co-movement literature has established, I use the underlying correlations in monthly exchange rate data to derive the baskets, by running factor analysis on the nominal exchange rates of 17 OECD countries that invested in the transition states between 1992 and 2004. Data was obtained from *United Nations Economic Commission for Europe* (UNECE 2006). All currencies were measured as the average nominal rate of each month in the Local Currency Unit (LCU) over US Dollars. The currencies loaded onto three components, explaining 92% of the variance (see Table 2). The first component is currencies of countries that are members of the European Monetary Union (EMU). These countries created a common currency in

Table 2. Generation of OECD currency baskets (using factor analysis).

Variable	OECD1 (Euro)	OECD2 (Dollar)	OECD3 (Yen)	Uniqueness
Germany	0.997	0.024	−0.039	0.004
Netherlands	0.996	−0.036	−0.047	0.004
France	0.968	−0.211	−0.105	0.007
Austria	0.964	−0.232	−0.095	0.007
Belgium	0.961	−0.243	−0.098	0.008
Finland	0.946	−0.151	−0.198	0.043
United Kingdom	0.159	0.801	−0.439	0.140
Switzerland	0.123	0.888	0.219	0.149
Denmark	−0.143	0.950	0.175	0.046
Canada	−0.158	0.818	−0.254	0.242
USA	−0.183	0.938	0.218	0.039
Norway	−0.189	0.959	0.015	0.044
Sweden	−0.401	0.868	−0.051	0.084
Korea	−0.324	0.664	0.371	
Japan	−0.423	0.185	0.821	0.113
Taiwan	−0.801	0.256	0.423	0.114
Eigen value	9.75	4.8	1.1	
Cumulative variance	42%	83%	92%	

Note: Varimax rotated principal components factors using monthly exchange rates between 1991 and 2004 from United Nations Economic Commission for Europe 2006.

1999, leading to perfect correlations after that date, and they were coordinating exchange rates for nearly the entire period covered by this time series. The second basket of currencies represents European and North American countries that are not members of the EMU. And the third basket represents predominantly Asian currencies.[14] These OECD groupings (EURO, US, and Yen) are nearly the same baskets identified in the co-movements literature.

Step 2: Next, I took the factor scores (analogous to predicted values in regression) from this operation and used them as the causal variables in a model estimating the nominal exchange rates of the transition states. The nominal rate was chosen rather than the real rate employed by Alfaro *et al*. (2004), because price liberalization strategy is counted as an economic reform by the EBRD and is highly politically malleable. Nominal rates offer a rate divorced from any domestic political strategy — they are simply the price of that currency on international markets. I use a Prais-Winsten Model with an AR1 process to deal with autocorrelation in the data. The resulting model was $y_{it} = \beta_0 + \beta_1(\text{OECD})_{it} + u_{it}$, where y is the average exchange rate, and i and t are the country and month indices, respectively, OECD represents the nominal exchange rates for the three baskets of OECD countries, and the error term is a residual capturing the domestic determinants of the exchange rate.[15]

These results are shown in Table 3. Each coefficient represents the impact in LCU of a one-unit change in one of the three currency baskets (or synthetic currencies). Because these three baskets are standardized to a common scale using factor analysis, the size of the coefficients can be compared directly — a one-unit change is equivalent to a standard deviation change in each currency basket.

There are a few other things to notice about Table 3. First, some countries did not have convertible currencies (notably Belarus, Uzbekistan, and Ukraine) until late in the reform process. Estimations of their currencies are available for these periods, but they are unreliable. Analysis begins the month that convertible currencies were available, but all estimations were re-run with a full set of estimated currencies to ensure that results were not influenced by this decision. This robustness check is imperative, because currency convertibility is a reform decision and the timing could affect the decision process of investors.

Second, it is also important to note that the extreme variation in the R^2 column listed in Table 3. Thankfully, most countries have at least one strongly significant association with an OECD basket, which provides some confidence in the technique. Some currencies are strongly determined by international price movements, as evidenced by their

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[14] While it is theoretically possible that changing exchange rates could reflect the exposure of the central bank in a particular OECD country to the transition economies, it is very unlikely. The economies represented are some of the largest, most open economies in the world. None of their central banks, particularly after the creation of the European Central Basket hold disproportionate reserves in transition country currencies. Evidence for this can be found in the strong influence of the EMU-geography on the currency loadings. Economic relations with other OECD countries are most important for determining individual, country rates.

[15] A time trend was included in the analysis to avoid spurious correlation.

Table 3. Regression of currency baskets on transition country currencies.

COEFFICIENT	OECD1-Euro		OECD2-Dollar		OECD3-Yen		Constant		N	R^2
Albania	7.94	(2.79)	−3.61	(3.32)	1.45	(2.54)	108.3	(10.5)	176	0.01
Armenia	6.97	(3.60)	−8.28	(4.02)	−1.25	(3.16)	412.2	(94.5)	144	0.14
Azerbaijan	−0.001	(0.005)	0.001	−0.01	−0.01	−0.004	0.145	(0.25)	152	0.05
Belarus	−70.0	(48.4)	151.8	(172)	−14.1	(20.8)	2000	(318)	62	0.03
Bulgaria	0.22	(0.061)	−0.09	(0.076)	0.076	(0.057)	1.073	(0.42)	192	0.07
Croatia	0.83	(0.10)	−0.203	(0.12)	0.021	(0.092)	4.652	(1.48)	168	0.28
Czech Republic	4.06	(0.21)	−1.10	(0.24)	0.339	(0.18)	28.7	(1.44)	167	0.78
Estonia	1.79	(0.044)	−0.416	(0.050)	0.243	(0.040)	13.7	(0.099)	174	0.95
FYR Macedonia	7.22	(0.48)	−1.61	(0.53)	0.509	(0.42)	50.0	(2.21)	156	0.69
Georgia	0.07	(0.025)	−0.166	(0.027)	0.036	(0.022)	1.63	(0.13)	134	0.30
Hungary	18.9	(1.51)	−5.75	(1.92)	1.62	(1.39)	149.4	(60.6)	203	0.47
Kyrgyz Republic	0.01	(1.35)	3.56	(4.57)	−0.275	(0.60)	46.0	(5.33)	91	0.64
Kazakhstan	2.53	(4.19)	−3.40	(14.1)	0.05	(1.86)	125.6	(18.9)	91	0.41
Latvia	0.03	(0.007)	0.00	(0.009)	0.001	−0.007	0.586	(0.026)	178	0.35
Lithuania	0.11	(0.052)	−0.02	(0.060)	−0.050	(0.046)	3.42	(0.34)	168	0.17
Moldova	−0.50	(0.48)	1.90	(1.61)	−0.139	(0.21)	14.5	(1.85)	91	0.51
Poland	0.25	(0.033)	−0.097	(0.041)	−0.008	(0.030)	2.32	(0.99)	203	0.23
Romania	0.14	(0.020)	−0.093	(0.026)	0.023	(0.019)	1.47	(1.20)	202	0.12
Russia	0.297	(0.35)	−0.958	(0.41)	−0.836	(0.32)	8.72	(10.7)	157	0.10
Slovenia	23.8	(0.99)	−6.16	(1.21)	2.06	(0.90)	144.5	(40.0)	181	0.79
Slovakia	4.58	(0.25)	−1.21	(0.29)	−0.084	(0.22)	33.6	(1.75)	167	0.73
Tajikistan	0.48	(0.14)	−0.240	(0.17)	0.003	(0.13)	1.61	(0.58)	179	0.05
Turkmenistan	10.5	(10.9)	−45.1	(18.7)	−9.26	(18.0)	5153	(22.5)	99	0.76
Ukraine	−0.21	(0.17)	0.712	(0.58)	−0.095	(0.076)	5.45	(0.84)	92	0.22
Uzbekistan	−18.8	(44.8)	2.3	(156)	4.28	(19.6)	990	(206)	50	0.04
Bosnia and Herzegovina	0.23	(0.006)	−0.050	−0.007	0.017	(0.006)	1.71	(0.017)	119	0.96
Serbia and Montenegro	4.68	(0.72)	−1.68	(0.81)	−0.020	(0.63)	36.5	(25.1)	150	0.16

Note: Standard errors in parentheses. Each row represents an individual Prais-Winsten Time Series Estimates regressing the real exchange rate of the transition country on the three OECD factors. OECD scores derived from factor analysis (see Table 2). N = The number of months that data on convertible currency was available. Model fitted with ARl process to address autocorrelation and time trends to avoid spurious regression.

high explained variances. These are predominantly countries with pegged exchange rate regimes. Other currencies have low R^2 values, indicating that OECD movements are poor determinants of their movements. When I take predicted values from this model and apply them in the next stage, we must keep this in mind. One possible outcome is that my identification technique will only accurately predict investment in countries with relatively high R^2 scores. I return to this issue below.

Step 3: Because the predicted exchange rates are recorded monthly, I save the predicted values from the Prais-Winsten regression, take the average exchange rate for each country year, and calculate the annual changes in the mean value of predicted exchange rates.[16] It is these values that will serve as the instrument (z) in the two-staged least squares procedure.

Note that, the bivariate correlation (0.21) between changes in predicted exchange rates and changes in the stock of FDI/GDP is significant at the 0.05 level, while the relationship with the dependent variable EBRD Reform is virtually indistinguishable from 0.[17] By way of comparison, actual exchange rates show the reverse relationship: they are more strongly correlated with Reform (0.26) than FDI (0.17). Figure 3 further explores the utility of taking predicted exchange rates as opposed to actual exchange rates. Panel 1 shows that the average change in nominal exchange rates among all transition countries, Panel 2 replicates changes in economic reform from Figure 2, and Panel 3 depicts the annual change in the predicted exchange rate from Table 3. Here, we see that actual rates demonstrate a lot more instability than the predicted rates, ranging from small declines to 400 point increases. Moreover, economic reform is closely mirrored by changes in actual exchange rates. One could quite easily superimpose Panel 1 onto Panel 2. Panel 3, however, follows a different trajectory, because these rates are determined solely by the international economy and not by domestic processes.

This examination is only illustrative of the theoretical logic of the instrumentation. The exclusion criteria require that the instrument not be associated with the error term in the second-stage model. Tests of these criteria are addressed in the IV-2SLS diagnostics below.

Step 4: After removing the domestic determinants of changes in exchange rates in Step 3, I now have an exogenous instrument to be used in the IV-2SLS model. The IV-2SLS model is implemented in *STATA's xtivreg2* procedure utilizing an HAC estimator to ensure that results are robust to arbitrary heteroskedasticity and autocorrelation.[18]

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[16] As a robustness test, the change rates were also calculated with the minimum, median, and maximum monthly exchange rates with very little substantive difference.

[17] This can be confirmed in the *Online Appendix* 2.

[18] HAC stands for a Heteroskedasticity and Autocorrelation Consistent estimator. I use the two-step, efficient Generalized Method of Moments (GMM) estimator in *xtivreg2*. A bandwidth (BW) of two is selected along with robust standard errors, but the findings are robust to other specifications. In two-step efficient GMM, the efficient or optimal weighting matrix is the inverse of an estimate of the covariance matrix of orthogonality conditions. The efficiency gains of this estimator relative to the traditional IV-2SLS estimator derive from the use of the optimal weighting matrix, the over-identifying restrictions of the model, and the relaxation of the i.i.d. assumption Schaffer (2007).

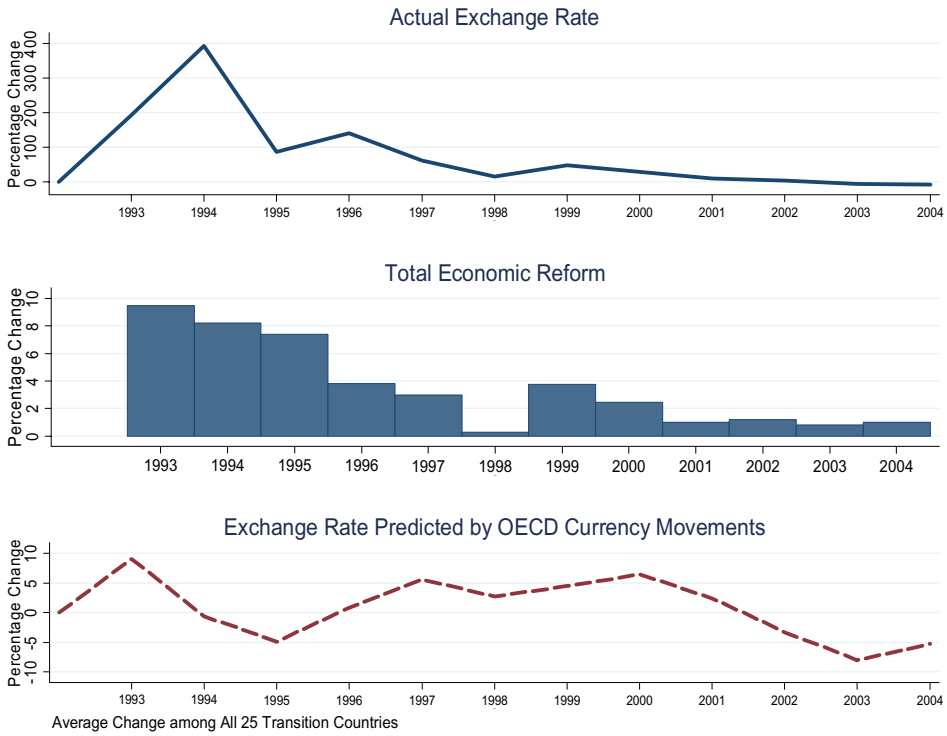

Figure 3. Relationship between economic reform and predicted exchange rates.

With this identification strategy, I move on to explore the impact of changes in the stock of cumulative FDI on economic reform progress. This is shown in the following equation

$$\Delta \text{EBRD}_{i,t} = \alpha_i + \delta_t + \beta_1(\Delta \text{FDI/GDP}_{i,t-1}) + \beta_2 \text{TV}_{i,t-1} + u_{it}, \quad (1)$$

where *i*and *t* index country and year, respectively. The variable ΔEBRD represents the change in total economic reforms between the year $t-1$ and year t. The coefficients α_i and δ_t signify the presence of country and year fixed effects, respectively. The variable $\Delta(\text{FDI/GDP}_{i,t-1})$ denotes the instrumented change in the stock of FDI (lagged one-year) as a percentage of GDP. Finally, time-variant controls are denoted by TV.

The results of the IV-2SLS procedure are depicted in Table 4. Four models are shown to demonstrate the validity of the instrumental variables approach. Model 1 is a reduced form equation that simply regresses economic reform on the instrumental

I report the GMM results in the tables, but readers should rest assured that all models have been re-run using the traditional IV-2SLS procedure with no additional options. Results are very similar and robust. The main result of GMM is to provide more efficient standard error estimations.

variable, as recommended by Murray (2006, p. 119), in order to check the intuition of the instrumental story. The instrument is positively correlated with economic reform and has the correct sign. Model 2 provides a baseline for comparing the first stage with and without the instrumental variable. Here, we find that only war is significantly correlated with yearly changes in FDI and that the resulting R^2 statistics is 0.17.

Model 3 provides the first stage results. As is standard in the IV-2SLS approach, control variables to be used in the second-stage of economic reform must also be used in the first stage as well. This specification is not intended to be the definitive model of FDI in the transition context; rather, it is meant to convey the conditional correlation between predicted exchange rates and FDI/GDP. Here, we see that the impact of exchange rate depreciation (a rise in the exchange rate relative to the dollar) has a very strong independent effect on changes in the stock of FDI/GDP. Predicted changes in exchange rates are significant and robust to specifications and additional control variables. Moreover, adding the change in exchange rates increases the explained variance of the model by over 30%. Because weak instruments can lead to severe bias in the second-stages, a Cragg-Donald *F*-Test was performed to test the strength of the instrument in Model 3. The *F*-statistic, which is well above the 10-point rule of thumb, is also larger than the critical values for a 5% Wald Test, indicating that the instrument is sufficiently strong (Cragg and Donald 1993, Stock and Yogo 2002).[19] An Anderson canonical correlation likelihood ratio test confirmed that the equation is not underidentified. Finally, an exogeneity test confirms that the regressor created by the instrumentation process can be treated as exogenous.

Model 4 displays the second-stage results. The instrumented stock of FDI has a significant and positive impact on yearly changes in reform. A 1% increase in exogenous FDI yields nearly a 6.3% increase in economic reform. Control variables all have the predicted signs, though many are not robustly significant across different specifications. The negative effect of communists in the legislature is just shy of standard levels of significance.

Model 5 through Model 9 demonstrates that the analysis is robust to several changes in specification. Dropping the Azerbaijan outlier, EU fixed effects, replacing political constraints with the well-known Polity IV measure of democracy, and using lagged reform as an alternative proxy for ceiling effects have virtually no impact on significance or substantive effects.

EXPLORATION OF THE LOCAL AVERAGE TREATMENT EFFECT

It is critical to note that R^2 values are smaller and Root Mean Squared Errors (RMSE) slightly larger in the second-stage models than their equivalent in the naïve models. These indicators help confirm that the identification process has been effective in reducing endogeneity bias by eliminating the explained variance in reform accounted for by the endogenous portion of FDI.

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[19] Model 9 is significant for a 15% Maximal IV size and Model 13 is significant for a 10% maximal IV size. All diagnostics are included in STATA's *xtivreg2* procedure by Schaffer (2007).

On the other hand, the size of the coefficient on FDI is larger in the IV model than in the naïve specifications. Instrumentation was expected to lead to lower correlations between dependent and independent variables. A larger coefficient in the second-stage is extremely common in IV-2SLS estimations and is generally attributed to attenuation bias in the naïve models, resulting from measurement error (Wooldridge 2006). In these cases, the instrument is seen as a corrective to measurement error and therefore leads to a larger substantive effect. This might be the case in the above estimations. Rules for the collection and reporting of FDI statistics vary tremendously across transition states, generating over-estimation in some contexts and underestimation in others (IMF 2006) — a classic case of stochastic measurement error.

A second possible reason for the larger substantive effect is that the instrumental procedure can sometimes eliminate portions of the full distribution of the key causal variable. Whenever a researchers use an instrumental variables approach, they must be conscious of the fact that not every observation is affected by the instrument. For instance, when Angrist (1990) uses the Vietnam-era draft lottery numbers to estimate the impact of military service on later earnings, he expressly notes that most of those who served in Vietnam where volunteers, who would have served regardless of their lottery number. As a result, his analysis only applies to those whose decision to serve was affected by the draft and not military service in general. This acknowledgement of the sub-population that is affected by the instrument is known as the Local Average Treatment Effect (LATE).

There are two entry points for LATE in my identification strategy. First, Table 3 revealed that some transition currencies were strongly determined by OECD exchange rate movements while others were barely affected at all. Is it possible that currencies more strongly impacted by OECD movements have a disproportionate influence in the first-stage of the IV-2SLS procedure, leading to biased results in the second-stage? To test this, I re-run Model 3 of the IV-2SLS procedure for countries with an R^2 above and below 0.3 in Table 4. The results shown in the *Online Appendix* (Table A4) reveal that explained variation in the prediction of exchange rates has little impact on the first-stage results. The coefficients for the two sets of countries are not significantly different.[20]

The second entry point for a LATE is in the impact of exchange rates on investor decisions. Not all investors are motivated enough by differential exchange rates to make multi-million dollar bets on location decisions. Blonigen (1997) and Guo and Trivedi (2002) have shown that exchange rates only have an impact on investment attractiveness where firm-specific assets are important; in particular, manufacturing investment is significantly influenced by exchange rate movements, but other sectors are less sensitive. These findings imply that by using the predicted exchange rate as an instrument, I may only identify a particular type of investor who is sensitive to exchange rates. To this end, I use the WIIW data on fourteen transition economies to create six large sectors for investment (manufacturing, services, construction/utilities, natural

[20] The results hold even if we increase the R^2 threshold to 0.4, despite the sizable differences in the number of panels in each group.

Table 4. Instrumental variable analysis of the effect of foreign direct investment on economic reform.

Dependent variable: Annual change in total economic reform (measured by EBRD scores)	IV-2SLS				IV-2SLS robustness tests				
	Reduced form	Baseline	First-stage	Second-stage	Dropping Azerbaijan	Democracy	EU fixed effects	Lagged reform	Lagged reform and time FE
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in stock of FDI/GDP (ln, lag)				6.289	7.086	6.932	6.333	5.126	5.849
				(3.219)	(3.281)	(3.245)	(3.171)	(2.922)	(2.844)
Exogenous change in exchange rate (lag 2)	0.058		0.010						
	(0.030)		(0.002)						
Seats held by communist successor parties (d)	−3.427	0.234	0.131	−5.371	−5.048	−5.104	−4.926	−2.269	−3.176
	(3.425)	(0.260)	(0.253)	(3.467)	(3.358)	(3.680)	(3.429)	(3.011)	(3.070)
Political constraints (d)	−2.233	−0.343	−0.326	0.371	−0.447		0.416	1.008	0.313
	(2.208)	(0.189)	(0.183)	(2.740)	(2.530)		(2.743)	(2.402)	(2.399)
War dummy	−1.205	−0.254	−0.224	1.978	1.904	3.023	2.102	0.388	0.370
	(1.037)	(0.091)	(0.089)	(1.412)	(1.341)	(1.778)	(1.411)	(1.299)	(1.302)
Relationship with EU (d)	0.572	0.032	0.037	−0.274	0.089	−0.425		−0.107	−0.266
	(0.548)	(0.044)	(0.043)	(0.588)	(0.567)	(0.757)		(0.502)	(0.515)
GDP per capita (ln, d)	8.584	−0.067	0.011	4.712	8.810	3.468	6.186	4.599	1.563
	(7.202)	(0.608)	(0.590)	(7.961)	(8.532)	(8.540)	(7.985)	(7.034)	(7.021)
Democracy (Polity IV)						−0.308			
						(0.178)			
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes
Drop Azerbaijan	No	No	No	No	Yes	No	No	No	No
EU fixed effects	No	No	No	No	No	No	Yes	Yes	Yes

(*Continued*)

Table 4. (*Continued*)

Dependent variable: Annual change in total economic reform (measured by EBRD scores)	IV-2SLS				IV-2SLS robustness tests				
	Reduced form	Baseline	First-stage	Second-stage	Dropping Azerbaijan	Democracy	EU fixed effects	Lagged reform	Lagged reform and time FE
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observations	309	286	286	277	268	244	277	277	277
Number of panels	27	27	27	27	27	27	27	27	27
R-squared	0.418	0.170	0.221	0.235	0.290	0.236	0.244	0.361	0.411
Root mean squared error (RMSE)	3.705	0.280	0.271	3.595	3.489	3.767	3.575	3.285	3.154
Anderson Canonical LR statistic			16.91		20.59	17.47	17.25	16.49	16.82
Cragg-Donald *F* statistic			14.35		17.57	14.62	14.46	14.91	14.21
Exogeneity test			4.082		3.674	4.469	4.207	4.536	6.065

Note: Standard Errors in parentheses. Due to nonstationary (tested using the Hadri (2000) Lagrange Mlultiplier), the first difference of some variables was taken. This is denoted by (d). One-year lagged variables denoted by (lag). If natural log taken. (ln) is used. All IV-2SLS results are OLS models implemented in using the STATA *xtivreg2* procedure. The two-step, efficient General Method of Moments (GMM). Three IV-2SLS diagnostic tests are presented: (a) The Anderson Canonical Correlation test is a likelihood-ratio test of whether the equation is identified. Because the statistics is significant at the 0.01 level, we can reject the null hypothesis that the equation is underidentified. In short, the excluded instrument (*x*-rate) is correlated with the endogenous regressors (FDI). (b) The Cragg-Donald *F* Statistic is a test of instrument strength. According to Stock and Yogo (2002), the critical values to perform a 5% Wald Test with this statistic for one endogenous regressor, one instrumental variable, and a 10% maximal IV size is 16.38. The second IV-2SLS is clearly above the 10% hurdle and the first specification is well above the critical value fora 15% maximal IV size (8.96). Therefore, we can be confident that predicted exchange rates are a strong instrument for change in the stock of investment. (c) Finally, the Exogeneity test gauges whether the new regressor created by instrumentation can be treated as exogenous. The test is significant at the 0.05 level, meaning we can reject the null hypothesis of endogeneity. The instrument is a valid one.

resources).[21] The calculation of these sectors from the European Commission's NASCE statistical classifications and full statistical analysis is shown in Table 5. Construction of large sectors is necessary as many countries do not possess all of the one-digit investment categories.

Next, I replace the cumulative stock of FDI in the first-stage regression (Table 4, Model 3) with these specific sector measures of FDI/GDP. The coefficients on the FDI variables are shown in Table 5, while Figure 4 provides a summary depiction of the analysis. The results are quite similar to Blonigen's original findings; two general types of investment are affected by exchange rate movements — manufacturing and service sector investment. Natural resource exploitation and construction/utilities, which are more dependent on access to resources and local markets are less influenced and are generally impervious to other factors (Hahn 2002). By contrast, much manufacturing investment and recently many types of services, such as law and financial operations, are efficiency-seeking, looking to gain cost advantages through geographically dispersed activities. Small movements in exchange rates can trigger enormous cost savings for these types of investors. The exchange rate advantage for export oriented manufactures is obvious; depreciating exchange rates enhance the competitiveness of their products.[22]

As a result, in the empirical section above, we must take care to interpret the regression coefficients as the effect of FDI into the manufacturing and service investment (the LATE) — not FDI as a whole. Natural resource and construction based investment, because they are not influenced by the exchange rate, are essentially dropped by the predicted effects in the first-stage model. This has important implications for the size of the FDI coefficient in the second-stage. As Table 5 reveals, when we re-run the entire IV-2SLS operation by sector-type, we find that service and manufacturing sector have effects on economic reform that are, like total investment, about seven times the size of the naïve specifications. Once again, diagnostics of the instrument illustrate that for these sectors the instrument is sufficiently strong and the predicted regressor is exogenous.

On the other hand, the impact of resource and construction-based FDI is not significantly different from zero with large standard errors and the small Cragg-Donald *F*-statistics reveal that exchange rates are not a sufficiently strong determinant of resource or construction-oriented FDI. This result makes sense; access to resources and bidding for construction/utilities projects may force some investors to actually lobby against general economic reform, in favor of favored policies that benefit their business in particular.

We only have a limited number of countries and a constrained timeframe for analyzing the LATE caused by investor-type, but it appears that this effect is the most likely culprit

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[21] Dataset includes 14 countries: Bosnia (2003–2004), Bulgaria (1998–2004), Croatia (1993–2004), Czech Republic (1997–2004), Estonia (1997–2004), Macedonia (1997–2004), Hungary (1998–2004), Latvia (1992–2004), Lithuania (1995–2004), Poland (1996–2004), Russia (1995–2004), Slovenia (1996–2004), Slovakia (1994–2004), and Ukraine (2001–2004).

[22] The fact that just over half the countries have reliable industry-level data is a concern, as one might suspect that these countries may be the high achievers in economic reform. Indeed the laggard countries of Central Asia are neglected by this analysis, but it is the best that can be achieved with present data.

Table 5. Analysis by industrial sector.

Differences in analysis by FDI sector	Stock of manufacturing FDI/GDP		Stock of services FDI /GDP		Stock of construction & utilities FDI/GDP		Stock of natural resources FDI/GDP	
Dependent variables	1st Stage Change in Sectoral Stock of FDI/GDP (In, lag)	2nd Stage Annual Change in Total Economic Reform	1st Stage Change in Sectoral Stock of FDI/GDP (In, lag)	2nd Stage Annual Change in Total Economic Reform	1st Stage Change in Sectoral Stock of FDI/GDP (In, lag)	2nd Stage Annual Change in Total Economic Reform	1st Stgge Change in Sectoral Stock of FDI/GDP (In, lag)	2nd Stage Annual Change in Total Economic Reform
	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Change in exogenous exchange rate (lag 2)	0.015 (0.002)		0.014 (0.003)		0.003 (0.003)		0.000 (0.001)	
Predicted change in stock of FDI/GDP (In, lag) from		7.367 (2.071)		8.34 (2.469)		32.187 (28.438)		766.4 (7531)
Observations	96	96	96	96	96	96	96	96
Number of countries	14	14	14	14	14	14	14	14
R-squared	0.449	0.492	0.488	0.435	0.254	−4.047	0.252	−624.8
root mean squared error (RMSE)	0.175	2.187	0.189	2.305	0.206	6.892	0.100	76.75
Anderson Canonnical LR statistic Cragg-Donald *F* statistic Exogeneity test		31.75 24.30 13.89		22.29 16.20 13.26		1.437 0.935 17.39		0.011 0.007 17.20
Author's calculation of sectors using NASCE codes	NASCE D (Manufacturing)		NASCE E (Electricity, Gas, and Water Supply) + F (Construction)		NASCE G (Wholesale, Retail, Trade) + H (Hotels and Restaurants) + 1 (Transport, Storage, Communication) + J (Financial Intermediation) + K (Other Services) + M (Education)+ N (Health)		NASCE A (Agriculture, Hunting, Forestry) + B (Fishing) + C (Mining, Quarrying)	

Note: Robust Standard Errors in parentheses.
All models are OLS models implemented in STATA using the *xtivreg2* procedure. All models replicate Models 3 and 4 in Table 4. Control variables remain and specifications remain the same.
Due to nonstationary (tested using the Hadri (2000) Lagrange Multiplier), the first difference of some variables was taken. This is denoted by (d).
One-year lagged variables denoted by (lag). Natural Log of variable used denoted by (In).
Nomenclature générale des activités économiques dans les Communautés Européennes (NASCE Rev. 1) is the statistical classification of economic activities in the European Community.

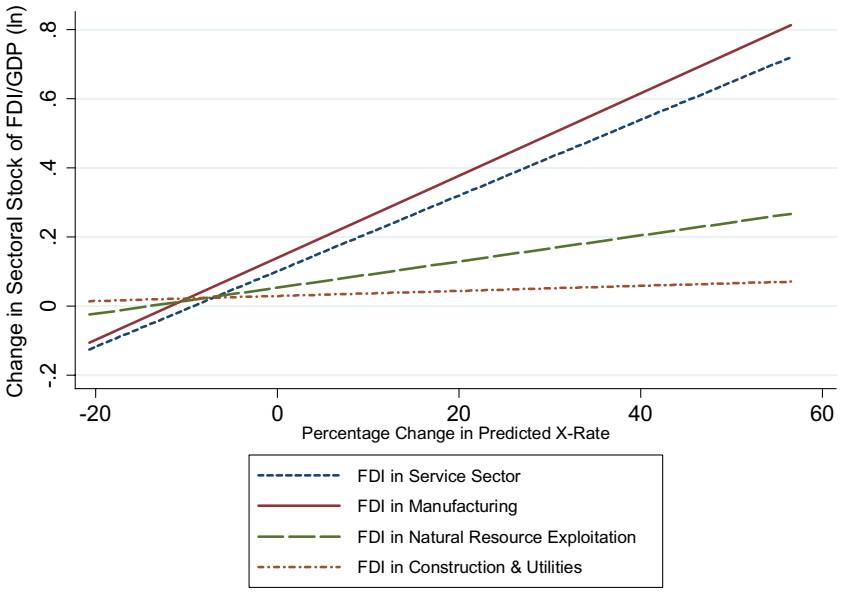

Figure 4. Impact of changes in exchange rates on changes in FDI stock (by industrial sector).

along with measurement error in FDI in explaining the larger substantive effect of FDI in the two-stage models compared to the naïve versions.

CONCLUSIONS

When an earlier generation of political scientists looked into the interaction of FDI and developing states in the 1960s and 1970s, they were explicitly interested in how FDI impacted local institutions and policy making processes. Though their work was focused on natural resource extracting industries, they took seriously the notion that foreign investors could influence political institutions of the host country, primarily because of the bargaining advantage that MNCs possessed over very poor and isolated developing countries. For a while these arguments provided fuel for the raging *dependencia* fires, because they confirmed developing countries' fears that industrialized countries manipulated local institutions in order to exploit them for raw materials (Biersteker 1978, Moran 1974). The notion of multinationals as *agents of change* lost its steam in the early 80's due to the Vernon's (1980) illustration that bargains struck with developing countries can dissipate over-time, and the perception that evidence marshaled by *dependistas* was biased by their own political agendas against MNCs.

In this paper, I find that the agents of change hypothesis may have been dismissed too soon. Instrumental variable regressions over a 12-year time-span found evidence for

an exogenous effect of FDI on the transition trajectories of host countries. Thus, even countries cursed by poor legacies, have a possible route to transition, if they are the recipients of large stocks of manufacturing and service sector investment.

More work certainly remains to be done. This paper had an explicit macro-focus, but the micro-logic of how coalitions of investors actually go about influencing transition countries' policies remains to be demonstrated in future work. One particular promising area that was slightly beyond the scope of this piece is to explore the interaction of FDI with local endowments. This will help us understand whether FDI acts as a complement or substitute for existing factors of production within a given country, which would provide even more insight into the political impact of foreign investors. Similarly, one could explore the differential impact of investors given local political institutions. Whether the lobbying of investors is more successful in democracies or more authoritarian regimes is an open question.

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