Institutional Spillovers and Economic Growth

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Abstract

This paper examines the premise that institutions generate externalities, and as a result, affect the economic performance of neighboring countries. Using country fixed effects and first-order differencing approaches, I estimate changes in neighboring institutions over a 25 year period. My findings suggest that while neighboring institutions do influence home growth rates, the specific channels through which these spillovers function remain largely unobserved. Nevertheless, I conclude that institutional externalities play a significant role in shaping regional growth experiences.

1. Introduction

While an extensive body of recent literature identifies institutions as a primary "deep" determinant of economic growth[1], there is less consensus on the exact mechanisms linking institutions and output performance. Douglass North hypothesizes that economic institutions, such as property rights, lower transaction costs between parties, allowing them to realize gains from trade (North 1991). Alternatively, Dani Rodrik emphasizes the role of institutions in alleviating market failures such as information asymmetries and monopolistic price distortions (Rodrik 2007). Nonetheless, despite these different perspectives, a common theme emerges in which institutions are viewed as country-specific and endogenous to the historical and cultural experience of a nation.

I argue that this narrative is incomplete in ignoring the possibility that institutions not only endogenously affect the growth rates of a home country, but also exogenously influence the growth rates and institutions of *neighboring* countries. A major obstacle in identifying these

[1] See Dawson (1998), Hall and Jones (1999), and Rodrik et al. (2004), among others.

institutional spillovers emerges if both home and neighboring institutions simultaneously affect each other or are jointly determined by a shared set of historical and cultural factors. Therefore, the main objective of this paper is to separately identify the effect of neighboring institutions from that of home institutions, while also examining some of the specific channels through which these institutional externalities might occur.

The potential for institutional spillovers is especially relevant to the current wave of democratic revolutions in the Middle East. On January 14, 2011, Tunisian president Zine al-Abidine Ben Ali was forced to abdicate in the face of popular opposition protesting high unemployment, corruption, and democratic repression (BBC News 2011). This event set off a domino effect of similar uprisings throughout the Middle East, as Egypt, Libya, Syria, Bahrain, and Yemen all experienced similar domestic unrest in the ensuing months. The correlation in timing of these events yields at least two possible interpretations. One is that the each democratic uprising occurred independently, largely in response to the national institutions and policies of the home country. Under this premise, any coincidences in timing were driven by similar domestic conditions across the Middle East, which in turn might have been influenced by an underlying set of shared regional characteristics including petroleum reserves, religious affiliations, or colonial history.[2] An alternative account is that shifts in economic and political institutions actually exert a direct influence on one another through feedback mechanisms. For example, the success of democratic reform in Egypt and Tunisia might have contributed to the likelihood of a revolution in Libya by changing the expected payoffs of demanding democratic concessions.

[2] A variety of literature points to this endogenous development of institutions. Isham et al. (2005) argue that point-source resources such as petroleum create poor institutions while Sokoloff and Engerman (2000) and Acemoglu et. al (2001) suggest colonial history as a possible determinant of present-day institutional quality.

While events in the Middle East suggest a significant role for institutional spillovers, the precise impact of these spillovers remains uncertain. One response to increased democratization in neighboring countries has been to implement limited democratic reforms at home in an effort hedge against protests before they actually occur. For example, following the fall of Hosni Mubarak in Egypt, Jordan's King Abdullah quickly announced his support for political reforms including the possible transition to a constitutional monarchy (Arab News 2011). In contrast, the alternative response (seen in Libya and Syria) has been a "crack-down" mentality in which the political elites have violently crushed protests and placed further restrictions on democratic freedoms. Therefore, while a shift in neighboring democratic institutions appears to be capable of prompting home institutional shifts, the direction of these changes remains ambiguous, as does their effect on growth rates.

In addition to affecting home political institutions, neighboring democratization could produce externalities in other areas as well. For example, if Egypt becomes more democratic, perhaps cross-border trade between Egypt and Israel will increase due to greater ideological similarities. In this manner, Israel could realize gains from trade directly related to an improvement in Egyptian institutions. Another potential source of institutional spillovers is foreign direct investment (FDI). If increased democracy in Egypt were to improve regional stability, corporations might be more likely to not only invest in Egypt, but also in Israel. In this instance as well, Israel economically benefits from better political institutions among its neighbors.

1.1. The Importance of Institutions

While the current uprisings in the Middle East underscore the potential for spillovers in political institutions, similar stories could be told with respect to economic, legal, or social

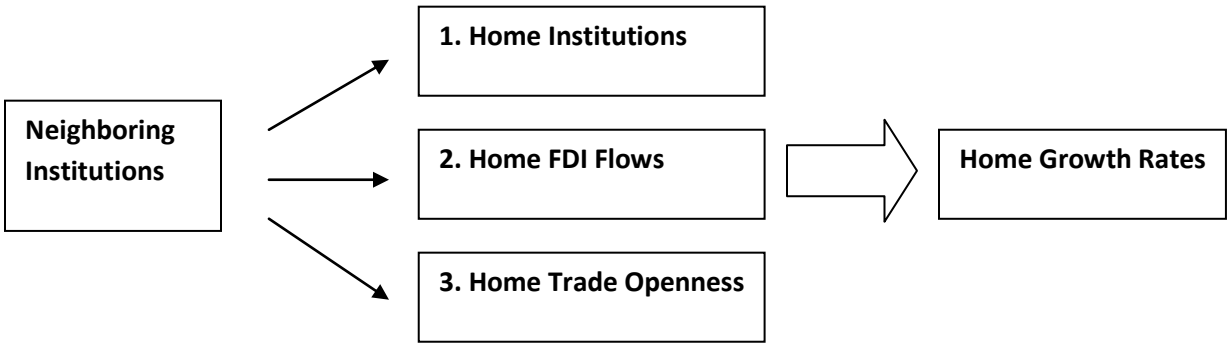
institutions. Douglass North defines institutions as "…the humanly devised constraints that structure political, economic and social interaction, consist(ing) of both informal constraints (sanctions, taboos, customs, traditions, and codes of conduct), and formal rules (constitutions, laws, property rights)" (North 1991, 97). For North, the interaction between political and economic institutions is essential; if property rights are fundamental to realizing gains from trade, strong political institutions are necessary to implement and equitably uphold these property rights. Daron Acemoglu and James Robinson emphasize this point in *Economic Origins of Democracy and Dictatorship*, noting that, "political institutions can influence the allocation of de jure political power in the future by virtue of being *durable* […] If citizens can secure democracy today, they will increase their de jure political power in the future because as long as democracy survives, they will have more say in the determination of economic and social policies" (Acemoglu and Robinson 2006, 174). Political and economic institutions should therefore be viewed as complementary forces with far-reaching implications for economic development.

Once we have answered the question "what are institutions?", an important additional consideration is the set of qualities characterizing "good" institutions. At a basic level, high-quality institutions beneficially shape public and private behavior. For example, positive economic institutions, such as property rights, should theoretically incentivize saving, human capital investment, and trade because they reduce transaction costs and provide individuals with the security to smooth consumption over time. In addition, as Dani Rodrik emphasizes, strong institutions may also work to mitigate market failures. According to this view, lower transaction costs might be a socially optimal byproduct of such institutions, but their ultimate objective should be to facilitate the efficient and undistorted functioning of markets. Rodrik lists five

categories of institutions as necessary to prevent market failures: property rights, regulatory institutions, institutions for macroeconomic stabilization, social insurance, and institutions for conflict management (Rodrik 2007). Therefore, Rodrik's view suggests that a general measure of institutional quality should take into account not only transaction costs, but also the collective action problems inherent in undeveloped markets.

1.2. Channels

If good institutions are defined by their ability to promote economic growth through the alleviation of market failures, it follows that these institutions should exhibit externalities which are manifest as institutional spillover effects. In other words, not only should the native population benefit from lower transaction costs, but surrounding populations should gain as well due to regional economic and political integration. I hypothesize three potential channels through which institutional spillovers effects might function:



In the first channel, neighboring institutions could directly affect home institutions in a game theoretic context if one country's willingness to improve its institutions depends on the institutional quality of its neighbors. On a smaller scale, we can imagine a payoff matrix in which an individual decides whether or not to observe property rights in two different states of the world: one in which property rights are enforced and one in which they are not.

State

Individual		Property Rights Enforced	Property Rights Not Enforced
	Property Rights Observed	(**2**,*2*)	(**-2**,*0*)
	Property Rights Not Observed	(**-2**,*2*)	(**0**,*0*)

In this matrix, if property rights are enforced, it would be in the best interest of the individual to observe them, as the costs of transgression would outweigh any gains. In contrast, in a state of no property rights, an individual will actually lose less by not observing property rights than by observing them. Therefore, a Nash equilibrium is reached in which the decisions of the individual are heavily contingent on the overall institutional environment, and a "good" environment can actually yield positive externalities by incentivizing good behavior.

A similar payoff matrix can be imagined at the communal, sub-national, and international levels. For example, if a state has high quality neighboring institutions, it might be in the interests of that state to improve its own institutions in response to its surrounding environment. In this context, neighboring countries with good institutions raise the costs of having bad institutions ("the cost of defection") to a level the home country is unwilling to bear. Furthermore, countries with similar institutions tend to support one another. Therefore, if China were to become more democratic, it would possibly withdraw support from North Korea, thus raising the costs in North Korea of sustaining an autocratic regime.

In addition to game theory, there are also other mechanisms through which neighboring institutions could affect home institutions. One recent example is the rule of law in the border regions between Pakistan and Afghanistan. Pakistan remains unable, or unwilling, to exert

control over its northeast territory. This influences the rule of law in Afghanistan, as an insurgency is able to use these regions as a staging area from which to attack the Afghan government. Therefore, neighboring institutions not only influence the incentive structure of home institutions, but also their effectiveness.

A second channel through which neighboring institutions could affect home growth rates is foreign direct investment. Busse and Hefeker (2007) find that improving the institutions of a home country tends to increase FDI flows. Similarly, if poor neighboring institutions cause a region to be perceived negatively worldwide, it could influence investment decisions, regardless of the institutional quality of the home country. For example, if a company wants to invest in Ethiopia, but is concerned about the unstable institutions and possibility of civil war in Somalia and Sudan, that company might choose to invest elsewhere in an effort to avoid risk related to the regional political climate. With increasing globalization and the interconnectedness of regional trading networks, the establishment of a positive *regional* investment profile perhaps becomes increasingly important in attracting FDI.

The existing literature remains divided on the precise impact of FDI on growth rates, largely due to endogeneity problems; it is difficult to determine whether FDI is a determinant or product of economic growth. Borensztein et al. (1998) argue that FDI has a positive effect on growth, but only when there is a sufficient base level of human capital. Blomstrom et al. (1994) find a similar positive relationship between FDI and growth, but only for higher income countries. Li and Liu (2004) conclude that FDI and growth are endogenously linked and exert complementary effects on one another. Finally, Carkovic and Levine (2002) offer a dissenting view, arguing that with proper identification techniques, there is a negligible direct and independent effect of FDI on economic performance. Nevertheless, with the exception of this

last paper, most of the literature suggests a link between FDI and development. Therefore, if FDI is determined by a regional rather than domestic investment profile, it constitutes a plausible channel through which neighboring spillovers could affect home growth rates.

The third channel through which neighboring institutions can influence home economic growth is trade. A key aspect of "good" institutions is their ability to reduce transaction costs by enforcing property rights. Therefore, if transaction costs in neighboring countries decrease due to better institutions, a home country might increase trade with these neighbors. This increase in trade volume could eventually drive higher growth rates in both countries. As noted in Rodriguez and Rodrik (2000), volume of trade is not necessarily a substitute for trade openness, which remains largely a product of policy (Sachs and Warner 1995). Consequently, while better neighboring institutions *might* increase volume of trade by incentivizing changes in home openness policy, an increase in trade volumes could also occur directly in response to better neighboring institutions.

The plausibility of this channel further rests on the assumption that higher trade shares (volume of trade/GDP) lead to increased growth rates, a conclusion which finds support in a large body of literature. Frankel and Romer (1999) use predicted trade shares based on geography to instrument for volume of trade, determining that trade has a significant and robust impact on growth. Dollar and Kraay (2003) reach a similar conclusion employing an alternative, first-order differencing approach. While others, such as Rodiguez and Rodrik (2000), view this body of literature with skepticism, the consensus does support a positive relationship between trade and growth.

1.3. Summary Findings

To test these specific channels as well as the overall impact of neighboring institutions on growth, I compile a neighboring institutions index using data from the PRS group and the Polity IV project.[3] My core specification regresses home growth rates on neighboring institutions, controlling for home institutions and neighboring growth rates among other factors. One endogeneity concern that arises from this specification is the correlation between levels of home and neighboring institutions. If both affect one another through feedback mechanisms or are simultaneously driven by similar regional factors, it would be difficult to isolate an independent neighboring institutions effect. To address this issue, I employ two similar methodologies. The first is a first-order differencing approach in which *changes* in growth rates over a 25 year period are regressed on *changes* in home and neighboring institutions, respectively. The second is a fixed effects approach which exploits higher frequency annual changes these variables. While there remain reverse causality concerns with both specifications, omitted variable bias is lessened by holding constant country-specific factors that do not vary over time, thereby also reducing the likelihood of multicollinearity between home and neighboring institutions.

Using this framework, I find that neighboring institutions exert an economically significant impact on home growth rates, comparable to that of home institutions. However, the specific mechanisms through which this effect occurs remain highly ambiguous. While it appears that medium term (25-year) shifts in institutions are unrelated, there is some evidence to suggest that higher-frequency institutional shocks between neighboring countries *are* correlated. This lends credence to a model of spillovers in the Middle East in which the institutions of one country actively influence the institutions of its neighbors.

[3] For a full description of the data, see Section *3.1.*

Although I find no significant impact of neighboring institutions on home FDI flows, there is a significant and positive relationship between neighboring institutions and home trade shares. Therefore, in addition to home institutions, trade represents a second plausible channel through which neighboring institutions could function. However, when home trade shares and FDI are added to the baseline specification as additional controls, neither are significant, and the neighboring institutions coefficient remains unchanged. Given these results, I conclude that the potential channels through which neighboring institutions influence growth rates remain largely unobserved.

The rest of this paper is organized as follows: Section 2 conducts a brief literature review, specifically focusing on institutions and cross-border spillovers. Section 3 introduces my empirical strategy and includes a discussion of data sources and methodology. Section 4 presents the main results as well as additional robustness specifications. Finally, Section 5 contains the conclusion and relevant policy implications.

2. Literature Review

Early cross-country growth regressions, such as Barro (1991), often included control variables for political instability including political assassinations and coups. However, these measures only proxy indirectly for institutions, and are also highly endogenous since economic performance could have a direct bearing on the probability of political upheaval. Knack and Keefer (1995) partially address these shortcomings by using data from the Political Risk Services (PRS) group and Business Environmental Risk Intelligence to directly proxy for property rights. They argue that this methodology represents a significant improvement over previous institutional measures, and conclude that the property rights exert a significant and positive impact on economic growth.

Hall and Jones (1999) expand this conception of institutions to not only include Knack and Keefer's index of property rights, but also the Sachs Warner index of trade openness (Sachs and Warner 1995). This composite "social infrastructure" index is used to explain cross-country variation in physical capital, human capital, and TFP levels (identified by Mankiw Romer and Weil (1992) as proximate determinants of economic growth). To establish causality, Hall and Jones instrument for institutions using latitude, predicted trade shares, and ethno-linguistic fractionalization. While not entirely exogenous, (all three instruments are potentially directly correlated with output level through non-institutional channels), these imperfect instruments represented an important first step in identifying the causal effect of institutions on economic growth.

In a seminal paper, Acemoglu et al. (2001) apply a similar IV framework, using colonial disease environment to instrument for expropriation risk, itself a component of Knack and Keefer's property rights index. The reasoning behind this instrument is that European colonizers only established permanent settlements (and by extension positive institutions) in areas with favorable disease environments. Acemoglu et al. argue that these positive colonial institutions form the basis of present-day institutions, which in turn determine current income level. One potential flaw in this identification strategy remains the possibility that colonial disease environment is correlated with current disease environment, or an entire range of other geographical factors. If this were in fact the case, the colonial disease environment instrument could actually be proxying for geography rather than institutions. Acemoglu et al. respond to this criticism by arguing native populations have built up immunity to diseases such as malaria and are therefore unaffected by the environment which had such a drastic impact on European settlement patterns. Consequently, this colonial disease environment instrument represents a

significant improvement over those advanced in Hall and Jones (1999), further underscoring the importance of institutions in effecting economic development.[4]

A major shortcoming of both Hall and Jones (1999) and Acemoglu et al. (2001) is that both IV strategies are highly correlated with other "deep" determinants of economic growth, namely geography and trade. In response to this limitation, Easterly and Levine (2003) test for the effects of geography ("tropics, germs and crops") on GDP per capita, both directly and indirectly through the channel of institutions. They conclude that although geography is positively correlated with income, most of this relationship functions through institutions rather than other channels. This primacy of institutions over other "deep" factors is further reinforced by Rodrik et al. (2004). As Rodrik et al. note, the direct effects of geography on institutions should be viewed as a practical instrument, but not construed as a validation of geographic determinism. In other words, the impact of geography on growth rates is largely secondary, and although these geographical characteristics represent a useful source of exogeneity, their influence on institutions should be interpreted as evidence of institutional, rather than geographical, primacy.

Rodrik et. al (2004) also explore the relationship between institutions and trade, using the Acemoglu (2001) instrument for institutions and the Frankel and Romer (1999) predicted trade share instrument for volume of trade. In a 2-stage OLS regression, Rodrik et al. conclude that institutions rather than trade are a primary determinant of income level, and that the less significant direct effects of trade represent an alternative channel through which institutions manifest themselves. This finding of institutional primacy is countered in Dollar and Kraay

[4] Feyrer and Sacerdote (2009) introduce a new identification technique by using islands as natural experiments for the effect of European presence. They instrument for institutions using wind patterns, as these patterns are both exogenous and correlated with the length of European colonization. Their findings are largely in line with Hall and Jones (1999) and Acemoglu et al (2001), implying a positive and significant impact of institutions on development.

(2003), who argue that high correlation between geographical/colonial determinants of trade and institutions creates difficulties in isolating the partial effects of each through an IV approach. To mitigate these concerns, they regress *changes* in institutions and trade share over time on changes in decadal growth rates, concluding in contrast to Rodrik et al. (2004) that trade exerts a significant and independent effect on growth.[5] Therefore, while the literature is by no means conclusive, the consensus is that institutions play an important role in long-term development, while trade is more significant in affecting short-term changes in growth rates.[6]

Another limitation of the early IV literature is its tendency to treat institutions as a "black box", failing to identify the specific mechanisms through which institutional change can influence growth. While evidence from Rodrik et al. (2004) implies that trade could represent one such mechanism, a variety of recent literature has further explored this area of uncertainty. Taking a broad perspective, Dawson (1998) finds that institutions affect growth both indirectly by incentivizing investment and directly through increases in total factor productivity. Furthermore, he argues that institutions play an important role in human capital investment. More recently, literature has established a connection between FDI and institutional quality. Busse and Hefeker (2007) argue that an improvement in institutions can lead to increased FDI flows, while Papaioannou (2008) concludes that better institutions increase international lending and bank flows. Finally, Clausen et al. (2009) determine that high levels of corruption negatively influence growth by diminishing confidence in public institutions. These channels are

[5] I employ a similar fixed effect approach to wash out any shared geographical factors between home and neighboring countries. This is useful in isolating the independent effect of changes in neighboring institutions on home growth rates.

[6] For a dissenting view on the role of institutions in economic development, see Sachs (2003) and Glaeser et al. (2004). In contrast to Acemoglu et al. (2001), Sachs argues that geographical factors, especially malaria, directly influence income per capita while Glaeser et al. attribute variation in growth rates to human capital rather than political institutions.

indicative of the growing emphasis in literature on not only establishing the importance of institutions, but also in revealing the specific channels through which they function.

One mechanism which has remained conspicuously absent from this body of literature is the possibility of institutional externalities. While, there *has* been some acknowledgement of spillovers between countries, these cross-border interactions have not been attributed to institutions. Easterly and Levine (1995) include an index of neighboring growth rates among other variables in an early series of regressions intended to explain the poor growth performance of Sub-Saharan Africa. They find this variable to be highly significant, implying the existence of growth externalities between neighboring countries. The measurement of these spillover effects through a multiplier calculation allows Easterly and Levine to conclude that a policy shift implemented jointly by two neighboring nations will have a much greater impact than policy shifts enacted unilaterally.

The major shortcoming of Easterly and Levine (1995) is that they treat neighboring growth rates as a "black box," leaving undetermined the exact mechanisms through which these growth spillovers occur. Other authors have attempted to isolate more specific sources of spillovers, including civil wars (Murdoch and Sandler 2002), regional instability (Ades and Chua 1997), and spatial proximity (Ramirez and Loboguerro 2002). In identifying cross-border spillovers, these papers also highlight possible channels through which institutional externalities could function. For example, if poor neighboring institutions contribute to regional instability and civil war, the negative spillovers emphasized in Murdoch and Sandler (2002) and Ades and Chua (1997) could ultimately be a manifestation of neighboring institutions. Therefore, the body of literature suggests that institutions could function as a "deep" determinant of spillover effects between neighboring countries.

3. Data and Empirical Strategy

3.1. Data

I use a cross-sectional panel of 148 countries over the time period 1984-2009 in estimating my core series of regressions. Island nations are omitted from the sample given the likelihood that spatial proximity plays at least some role in determining spillover effects. While island nations might be affected by major trading partners or other nearby landmasses, the mechanisms through which these effects occur potentially differ from those observed between countries sharing land-based borders.[7] As a result, islands are treated as outliers and are excluded from the sample. In addition, countries with poor data availability were also omitted, creating the potential for selection bias. If the excluded countries also had institutions which were getting progressively worse over time, my empirical results would not be robust to the inclusion all countries. However, given the relatively few number of observations dropped due to data constraints, sample selection bias is not likely to substantially influence my results.

The dependent variable in my main specification is the change in home growth rates of GDP per capita (2000 US$).[8] The use of growth rates instead of levels is significant in that growth rates represent a "flow" rather than a "stock" measurement of income level. In other words, the average growth rate is a function of the flow of inputs, or how much these inputs change over time. In contrast, average level of income is the net aggregate of past input levels. Therefore, even if institutions improved rapidly over a 25 year period, the overall level of institutions might remain fairly poor. By extension, although institutional "flow" has increased,

[7] For example, regional stability and FDI spillovers would probably not strongly affect islands due to their geographical isolation. Conversely, the institutional quality of major trading partners could be highly influential if trade is relatively more important to island economies.

[8] Source: *World Bank*, World Development Indicators

the change in levels might not be particularly large over this relatively short time period. Consequently, I use the changes in growth rates rather than levels of income as my left-hand side variable. This approach has some precedent in recent growth literature. Haussman et al. (2004) elaborate upon this framework of "growth accelerations", noting that such occurrences are surprisingly frequent and often correlated with regime change characteristics. Additionally, Dollar and Kraay (2003) use changes in growth rates as the dependent variable in identifying the independent effects of changes in trade share and institutions.

Institutional data came from two sources, the Political Risk Services (PRS) Group and the Polity IV Project. The PRS group is a company which sells political and economic risk assessments to both private investors and academic researchers. The Polity IV project contains a dataset of regime stability characteristics jointly managed by the Center for Systematic Peace and Center for Global Policy. A wide body of literature uses both PRS and Polity IV data to proxy for institutional quality, including Knack and Keefer (1995), Hall and Jones (1999), Acemoglu et al. (2001), and Busse and Hefeker (2007).

For the purposes of this project, I use five separate indexes of political risk from the PRS group: Bureaucracy, Corruption, Investment Profile, Law and Order, and Democracy. Bureaucratic Quality is measured on a 0 to 4 scale and awards high scores to countries in which "the bureaucracy has the strength and expertise to govern without drastic changes in policy or interruptions in government services" (PRS Group, "ICRG Methodology"). Corruption is measured on a 0 to 6 scale and takes into account "excessive patronage, nepotism, job reservations, 'favor-for-favors', secret party funding, and suspiciously close ties between politics and business" (PRS Group, "ICRG Methodology"). Corruption levels are an important proxy for institutional quality, as informal institutions associated with corruption such as rent-seeking can

generate deadweight losses and presumably hinder the efficient allocation of resources. Investment Profile is measured on a 0 to 12 scale and consists of three sub-components: contract viability/expropriation risk, profits repatriation, and payment delays. Law and Order is measured on a 0 to 6 scale and takes into account both the quality of the legal system as well as overall crime rates. Finally, Democratic Accountability is measured on a 0 to 6 scale, and quantifies the responsiveness of the regime to public opinion (PRS Group 2011). These five PRS components are augmented by an index of Executive Constraint from the Polity IV dataset, which is measured on a 1 to 7 scale and takes into account institutional checks and balances as well as accountability for executive decision-making.

Similar to Hall and Jones (1999)[9], I also construct a composite institutions index as an equally weighted average of Bureaucratic Quality, Corruption, Investment Profile, Law and Order, and Executive Constraint.[10] This aggregation proxies for overall institutional quality, as it captures not only the effects of each individual sub-component, but also the interactions between these sub-components. For example, a company might not want to invest in a country with a favorable investment profile but high levels of corruption or vice versa. However, a joint improvement in both characteristics could lead to a "tipping point" and a consequent increase in foreign investment. Therefore, the composite index allows for the possibility of synergies between the individual measures of institutions. Democracy is notably left out of this index as it most directly pertains to the political, rather than economic, decisions of the individual. The body of literature has recognized this fundamental disunity and has treated democracy as a

[9] The Hall and Jones (1999) social infrastructure index consists of two equally weighted measures: an "institutions" component and a "trade" component. The "institutions" component comprises of five PRS indicators: law and order, bureaucratic quality, corruption, risk of expropriation, and government repudiation of contracts. The "trade" component is the Sachs-Warner Trade Openness Index (Sachs and Warner 1995).

[10] To mitigate distortion, I normalize all six sub-components on a 0 to 1 scale before averaging.

distinctly separate institution.[11] So I can interpret my results in the context of the existing literature, I retain democracy is as an independent proxy for institutional quality but do not include it in the composite institutions variable.

When examining changes in this aggregate institutions index over a 25 year period, several notable features are evident. First, as shown by Table 1, scores range from a minimum of .235 to a maximum of 1. Also institutions for all countries have tended to improve over time, increasing by an average of .104 between 1984 and 2009. Table 2 displays average within-country institutional variation by region. Unsurprisingly, institutions in the developing world show twice as much variation over the specified time period as advanced countries, while Latin American countries in particular show three times as much variation. Therefore, while the institutions of developed countries are high-quality and relatively stable, developing countries have on average lower scores and illustrate greater fluctuations over time.

Figure 1 further illustrates this point in examining the institutional experiences of five specific countries. Over the 25-year period, the United States and India show fairly constant institutions, while Chile experiences large gains, most likely a reflection of increased liberalization and democratization in the post-Pinochet era. In contrast, Zimbabwe shows a precipitous decline, largely a result of high corruption and expropriation risk under the Mugabe regime. Finally, Russia also exhibits a deterioration of institutional quality during the initial wave of post-Soviet privatization before recovering somewhat in the early 2000's. Therefore, not only is there substantial variation in institutional experiences between 1984 and 2009, but

[11] Knack and Keefer (1995) and Hall and Jones (1999) both exclude democracy from their composite indexes of institutions. Furthermore, Acemoglu et al. (2001) uses expropriation rates from the PRS group as a proxy for institutional quality (eg. property rights), exploring the relationship between income and democracy separately in Acemoglu et al. (2008).

these experiences correspond to tangible policy shifts and regime characteristics over the same period.

One issue related to this variation is measurement bias. To generate political risk forecasts, the PRS group uses an objective set of standards which are applied independently to each country for which coverage is available. Nevertheless, there is necessarily some subjectivity in assigning numerical scores to gauge specific institutional qualities. This subjectivity could bias the data in a number of ways. First, because the PRS group sells its data to private investors, there might be an inherent incentive to overstate policy shifts. If these relatively small changes in institutional quality are magnified by the PRS group to attract customers and justify data collection efforts, transient political changes could be misrepresented as pivotal institutional transitions.[12] Another source of potential bias involves the actual standards used by the PRS group to determine the relative quality of institutions. If these standards are in fact a relative assessment, (and they seem to be given the discrete nature of the scaling), one might worry that the PRS group is assuming institutions in advanced countries are definitionally "good". Therefore, development levels could be informally driving institutional scores, generating a misleading correlation between institutions and growth.

The variable of interest for my core series of regressions is an index of neighboring institutions[13], constructed by averaging the institutional scores of each neighboring country weighted by shared border length with the home country[14]. This border weighting mechanism accounts for variation in the relative impact of institutions among neighboring countries. France, for example, shares borders with both Luxembourg and Germany, but we would not expect both

[12] Glaeser et al. (2004) express the similar concern that these measures of institutions reflect political volatility and outcomes rather than "deeper" determinants.

[13] Figure 2 shows changes in this index of neighboring institutions for five countries over a 25-year period.

[14] Shared border length data is from the CIA World Factbook and Ashraf et al. (2010).

countries to exert an equal influence on France. Therefore, if France shares a total border of 2828 km with its neighbors and a border of 73 km with Luxembourg, Luxembourg's institutional score provides only 2.6% of the neighboring institutions index for France. I also consider GDP, total land area, and population size as alternative weighting mechanisms. However, given the relative exogeneity of shared border percentage compared to these other factors, I use borders as the baseline weighting mechanism for my empirical strategy.[15]

In addition to the aforementioned measurement issues, missing values are another source of potential bias in the neighboring institutions index. A useful example to consider in this respect is the case-study of Poland. Poland is surrounded entirely by ex-Soviet satellite states, including East Germany, for which no institutional data exist before 1991. With the fall of the Berlin wall in 1990, East and West Germany were again unified, allowing East Germany to at least nominally "adopt" many of West Germany's positive institutions. Therefore, in 1991, Germany's corruption score was a 5 (out of 6) on the PRS scale. However, in this same year, 1991, no PRS data existed for any of Poland's other neighbors, resulting in Poland receiving a 5 for its neighboring corruption score by default. In ensuing years, as more data became available, the other Soviet satellite states surrounding Poland were slowly incorporated into the Poland's index of neighboring institutions. These newly open countries had relatively inferior corruption levels compared to Germany, ranging from 1 to 4 on the same 6-point scale. This led to a drop in Poland's neighboring corruption score from 5 in 1991 to 2.27 in 2001.

While some of this drop can be attributed to a legitimate worsening of Poland's neighboring institutions over the time period, a large part of this decline was simply driven by the addition of observations. In other words, Poland's neighbors had poor institutions in 1991 as well, but because no data was available, these poor institutions were not factored into the index

[15] For a further discussion of weighting mechanisms, refer to section *4.6. Robustness*.

until subsequent years. While Poland represents an extreme example, the possibility remains that shifts in the neighboring institutions index are biased by these missing values. To combat this issue, I employ a first order differencing approach using the average of neighboring institutions over two 5-year periods: 1984-1988 and 2005-2009. This technique smoothes much of the year-to-year noise which could be driven by the addition of observations.

In my main specification, I also include several fundamental control variables including home saving rates, home population growth rates, and home level of educational attainment.[16] Summary statistics for these parameters can be found in Table 1. These controls are directly derived from the augmented Solow growth model introduced by Mankiw et al. (1992), where savings and population growth are determinants of physical capital accumulation and educational attainment levels are used to proxy for human capital. As important proximate causes of economic growth, these variables are all potential sources of omitted variable bias, particularly with respect to the home institutions index. For example, if rising levels of human capital in the home country cause an increase in institutional quality, it could bias the home institutions coefficient upwards. This would make the neighboring institutions coefficient seems relatively smaller in comparison, underestimating the relative importance of neighboring institutions. Consequently, controlling for omitted variable bias with respect to home institutions is essential in interpreting the relative importance of the neighboring institutions index.

An additional control variable in my core specification is an index of neighboring growth rates, which I compiled using the same border-weighted structural framework as the neighboring institutions index. Given the earlier work of Easterly and Levine (1995) on the spillover effects

[16] The data for population growth rates and net national savings rates (%GNI) comes from the World Bank's "World Development Indicators". To proxy for educational attainment, average years of total schooling are used from the Barro-Lee Educational Attainment dataset (Barro and Lee 2000). As this data only exists for five year intervals, missing years are interpolated.

of growth rates, it seems likely that a broad range of spillovers exist unrelated to neighboring institutions. If these other factors are correlated with neighboring institutions through their effects on neighboring growth rates, the coefficient estimate for neighboring institutions could be biased upwards. Therefore, the inclusion of neighboring growth rates may help control for other possible cross-border externalities functioning independently of institutions. Furthermore, it seems unlikely that an omitted variable could impact both neighboring institutions and home growth rates without also affecting neighboring growth rates. Thus, not only does the inclusion of this variable address the underlying causes of Easterly and Levine's growth spillovers, but it also alleviates the larger issue of omitted variable bias.

3.2. Empirical Strategy

I use two similar approaches to estimate the spillover effects of neighboring institutions. The first is first-order differencing model,

$$(1)\ \Delta growth_{home} = \alpha + \beta_1 \Delta saving_{home} + \beta_2 \Delta pop_{home} + \beta_3 \Delta educ_{home} + \beta_4 \Delta inst_{home} + \beta_5 \Delta inst_{neighbor} + \beta_6 \Delta growth_{neighbor} + \epsilon$$

in which $\Delta growth_{home}$ is the change in growth rates for the home country, $\Delta saving_{home}$ is change in home savings rates, Δpop_{home} is change in home population growth rates, $\Delta educ_{home}$ is change in home level of educational attainment, $\Delta inst_{home}$ is change in home institutions, $\Delta inst_{neighbor}$ is change in neighboring institutions, and $\Delta growth_{neighbor}$ is change in neighboring growth rates. For each variable, the average value between 1984 and 1988 is differenced from the average value between 2005 and 2009, smoothing any high-frequency fluctuations in the year-to-year measurement of institutions. Although these parameters exclude some countries from the sample (particularly the former Soviet Bloc nations for which data does

not exist before 1991), they also eliminate the risk that missing data for countries such as Poland are fundamentally driving changes in the neighboring institutions index.

One of the primary advantages of first-order differencing is that it takes into account the long-term persistence of institutions. Unlike policies, institutions cannot be reversed overnight, as evidenced by the wide body of literature emphasizing the colonial origins of present-day institutions.[17] Therefore, while it seems unlikely that a sudden change in institutions over a one year period would substantially affect growth rates, the persistence of this change over a longer period could potentially generate a greater impact. A similar empirical framework is employed by Busse and Hefeker (2007) to gauge the impact of institutional change on FDI flows and by Dollar and Kraay (2003), who use lagged changes in institutional quality and trade shares to predict changes in average decadal growth rates.

To complement this first-order differencing approach, I also employ a model incorporating country fixed effects.

$$(2)\ growth_{home.it} = \alpha + \beta_1 saving_{home,it} + \beta_2 pop_{home,it} + \beta_3 educ_{home,it} + \beta_4 inst_{home,it} + \beta_5 inst_{neighbor,it} + \beta_6 growth_{neighbor,it} + \delta_i + \epsilon$$

In this specification, the dependent variable is the annual growth rate ($lnGDP_{it} - lnGDP_{i,t-1}$) at time *t* differenced from the average annual growth rate over the 25 year period, and δ_i is a fixed effects dummy for country *i*. This approach is not without precedent, as Acemoglu et al. (2008) employ a similar model of country-fixed effects in examining the causal impact of income level on democracy.

[17] This body of literature includes Sokoloff and Engerman (2000), Acemoglu et al. (2001), Acemoglu et al. (2002), and Feyrer and Sacerdote (2009).

There are two main advantages to this method over first-order differencing. First, unlike first-order differencing which ignores all data between 1989 and 2004, the fixed effects model utilizes the entire 25-year sample period, significantly increasing the number of observations. Secondly, this specification exploits higher frequency year to year variation in both growth rates and institutions. While this additional noise might contribute to measurement error, there is also the possibility that institutional shocks influence growth rates over a much shorter period than 25 years. In this respect, the fixed-effects model is an important complement to the longer-run and lower frequency first-order differencing approach.

Despite these distinctions, first-order differencing and fixed effects are similar in that they wash out any country-specific or regional factors that do not change over time, such as geography. In doing so, these specifications eliminate a variety of plausible instruments for institutions including colonial disease environment, ethnolinguistic fractionalization, latitude, and colonial nation of origin. One alternate strategy for achieving a clean identification might be to use these instruments as proxies for neighboring institutions. However, the very nature of neighboring institutions limits the effectiveness of this strategy. For example, if I were to construct an index of neighboring colonial disease environment, it seems likely that this index would also be correlated with the disease environment of the home country, which in turn is correlated with home institutions. This would lead to omitted variable bias, as the instrument for neighboring institutions would pick up the effect of home institutions as well. Therefore, given the commonalities of geography and colonial experience on the regional level, it is nearly impossible to differentiate the effects of home and neighboring institutions using these instruments. The benefit of the first-order differencing/fixed effect model is that it eliminates these instruments as sources of omitted variable bias, as geography and past colonial experience

are time invariant. In doing so, I am able to at least partially differentiate the separate effects of home and neighboring institutions on home growth rates.

One issue with this methodology is that although country-fixed effects are held constant, there are still omitted variables that change over time and could be correlated with changes in the quality of neighboring institutions. However, as Ades and Chua (1997) argue, the use of neighboring rather than home variables of interest increases the plausibility of exogeneity, as it would seem less likely that a *regional* factor would change over time and influence both neighboring institutions and home growth rates. For example, let us consider a scenario in which a government policy decision increased the quality of home institutions while also independently influencing growth rates. While this would bias the coefficient estimate on home institutions, the neighboring institutions coefficient would remain unbiased, as it remains unlikely that all of the neighboring countries in a particular region would simultaneously adopt the same policy. Admittedly, this argument is inconsequential if home and neighboring institutions affect one another through spillovers. However, I will proceed from the null hypothesis that short and medium term changes in institutions over a 25-year period occur independently of one another, and then separately test for the possibility that these changes are actually linked.

In addition to these omitted variable concerns, there are reverse causality issues with both the fixed effects and first-order differencing specifications. Although the consensus of previous literature seems to be that institutions affect growth and not vice versa, this relationship has not been explored for neighboring institutions. Therefore, the possibility exists that changes in home growth rates are driving changes in neighboring institutions. While seemingly unlikely given the existing body of literature, this possible interpretation is an interesting extension warranting further investigation, and would actually support the existence of cross-border spillover effects.

4. Results

4.1. Main Specification

My core series of regressions estimates the impact of neighboring institutions on home growth rates using both first-order differencing and fixed effects approaches. While taking the general form of (1) and (2) respectively, in addition to growth rates, I also include log GDP as an alternative dependent variable in both specifications. The composite indices for home and neighboring institutions are used as right-hand side variables, and both neighboring institutions and neighboring growth rates are weighted by shared border length with the home country. Results are displayed in Table 3. Notably, the home institutions coefficient is positive across all four specifications, and is significant at the 5% level in three of the four. The neighboring institutions coefficient is also positive and economically significant using both fixed effects and first-order differencing, but only when growth rates are the dependent variable. When these growth rates are replaced by levels, the neighboring institutions variable actually reverses signs. Finally, while neighboring growth rates are positive across all four specifications, they are only significant (at the 1% level) in the fixed effects model.

These initial results are suggestive on several fronts. First, changes in neighboring institutions seem to exhibit a more robust impact on growth rates than levels of GDP. This lends credence to the hypothesis that growth rates are relatively more affected by changes in institutional "flow" than income levels. As a result, I use growth rates as the dependent variable in ensuing specifications. Secondly, both fixed effects and first-order differencing (using growth rates) show neighboring institutions to be economically significant and comparable in magnitude to the home institutions index. For example, in the first-order differencing model, an increase of

.1 in the home institutions score generates a .78% increase in home growth rates over the 25 year period. Similarly, a .1 point change in neighboring institutions yields a .76% gain. Not only does this effect suggest the significance of institutional spillovers in absolute terms, but the parity between the home and neighboring coefficient estimates implies the relative importance of neighboring institutions in comparison to home institutions. Finally, the significance of neighboring growth rates in the fixed effects framework is evidence that short-term spillovers between countries persist independently of institutions. However, in the longer-term first-order differencing specification, the main source of cross-border spillovers is institutions rather than other neighboring factors such as macroeconomic policies, savings rates, or human capital investment.

I next test different measures of home and neighboring institutions, running separate regressions for the six individual sub-components of Bureaucracy, Corruption, Executive Constraint, Investment Profile, Law and Order, and Democracy. Results are displayed in Tables 4A and 4B. In the first-order differencing specification, bureaucracy, investment profile, law and order, and democracy all register positive home coefficients significant at the 5% level, while the neighboring coefficients remain insignificant. For this group of institutions, it appears that 25-year changes in the home country's institutional quality are important determinants of growth rates, while neighboring institutions have little or no impact. In contrast, for Executive Constraint and Corruption, the home coefficient is insignificant while neighboring coefficients are positive and almost significant at the 5% and 10%, levels, respectively. Therefore, changes in Executive Constraint or Corruption levels in the home country have only minor growth implications when compared to similar changes in neighboring countries. While Executive Constraint and Corruption remain important indicators of institutional quality, the results suggest

they differ from other institutional measures in functioning primarily through a set of cross-border spillover mechanisms.

Using fixed effects instead of first-order differencing generates somewhat contrasting results. While home executive constraint, investment profile, law and order, and democracy are all positive and significant, all neighboring institutions (with the exception of democracy) are insignificant. This indicates that while longer-term changes in neighboring institutions influence growth rates, short-term shocks remain relatively unimportant. Notably, democracy actually shows the opposite tendency, implying that higher frequency democratic spillovers can influence home growth rates, while longer-term and possibly more gradual shifts play a lesser role. Finally, although five out of the six measures of neighboring institutions in this specification are insignificant and three are negative, it is important to note that the composite neighboring institutions index is both positive and nearly significant at the 10% level. One plausible explanation for this contradiction is the power of regime change to simultaneously affect all institutional measures. For example, if a dictatorship is overthrown and replaced by a democracy, it would seem likely that all institutional scores would improve. This shock in aggregate institutional quality could affect growth rates in neighboring countries, even while short-term shocks to specific institutional sub-components show little impact.

While both first-order differencing and fixed effects approaches suggest an interesting relationship between neighboring institutions and home growth rates, this effect is not perfectly identified. One alternative interpretation of these results is that changes in home growth rates significantly drive changes in both home and neighboring institutions. While I consider this explanation unlikely given the extant body of literature, it remains a viable area for future exploration. Another possibility is that these results are being driven by omitted variable bias

(OVB). While any factor that is correlated with neighboring institutions and simultaneously exerts an independent effect on home growth rates is a potential source of OVB, it seems likely that such a source would also be picked up by the controls for home institutions or neighboring growth rates.

The most plausible grounds for omitted variable bias is a form of reverse causality, in which increased trade flows cause the development of better institutions instead of vice versa. To further illustrate this point, let us examine a hypothetical scenario in which increased world transportation costs cause home countries to trade more with their direct neighbors. If this increased trade volume incentivized neighboring and home countries alike to adopt better institutions, while simultaneously affecting home growth rates through increased technological diffusion, both home and neighboring coefficient estimates would be biased upward.

Rodrik et al. (2004) use instrumentation techniques to reject this explanation of reverse causality, noting that while institutions have a large direct effect on both trade and income levels, trade has an insignificant impact on income and institutions. This finding supports the chain of causality in which neighboring institutions → trade → growth, and it also reduces the possibility of trade-driven omitted variable bias. While my methodology makes replicating Rodrik's instrumental variable strategy infeasible, I further control for this source of omitted variable bias by including volume of trade as a right-hand side variable in supplementary regressions (See Table 9).

4.2. Neighboring Institutions→Home Institutions

Given the significant impact of neighboring institutions on home growth rates, I next test the specific mechanisms through which these spillover effects might occur. The first potential channel is through home institutions: if better neighboring institutions cause home institutions to

improve, it could be these home institutions which actually drive changes in home growth rates. To examine this possibility, I first run simple correlations between changes in home institutions and changes in neighboring institutions over a 25 year period (See Table 5).

The general lack of correlation between home and neighboring institutions suggests that changes occur independently across countries. Not only does this reduce the likelihood that institutional spillovers function through home institutions, but it also increases the probability that changes in neighboring institutions are exogenous with respect to home growth rates. If there exists an omitted variable which changes over time (and is therefore not washed out in the fixed effects specification), one would expect this variable to have a similar impact on both home and neighboring institutions. For example, if rainfall levels (and therefore crop yields) were to hypothetically influence the year-to-year quality of institutions, this impact would pervade regionally, affecting both home and neighboring countries alike. Therefore, the lack of synchronization in medium-term institutional changes implies that these changes occur independently of one another and in response to largely domestic, not regional, factors.

To further test the possibility that home institutions change in conjunction with neighboring institutions, I run a first-order differencing model with home institutions as the dependent variable, taking the form:

$$(3)\ \Delta inst_{home} = \alpha + \beta_1 \Delta saving_{home} + \beta_2 \Delta pop_{home} + \beta_3 \Delta educ_{home} + \beta_4 \Delta growth_{home} + \beta_5 \Delta inst_{neighbor} + \beta_6 \Delta growth_{neighbor} + \epsilon$$

For home and neighboring institutions, the composite border-weighted indices are used. The results displayed in Table 6 show that neighboring institutions are actually expected to have a *negative* effect on home institutions. However, upon further investigation, I conclude this result is non-robust, as additional specifications using different institutional sub-components and

weighting mechanisms register as statistically insignificant (see Appendix A). These OLS findings are in line with the correlational evidence presented in Table 5.

I also use an alternative fixed effect approach to estimate (3). In contrast to the first-order differencing specification, I find that neighboring institutions have a positive and statistically significant impact on home institutions, with a .1 point increase in the neighboring institutions index yielding a modest .015 point increase in the home institutions index. Therefore, while longer-run (25-year) trends in home and neighboring institutions occur independently of one another, higher frequency institutional shocks picked up by the fixed effects model seem to be related. Intuitively, this conclusion seems reasonable given recent events in the Middle East. Although long-run institutional trends might still be highly dependent on domestic factors and unconnected to one another, the past year has seen positive spillover shocks which have disrupted these long-run trends and led to an widespread improvement in institutions. This effect would be overwhelmed by the lower frequency and more gradual changes over a 25-year period, explaining the insignificance of neighboring institutions using the first-order differencing approximation.

4.3. Neighboring Institutions→Home Foreign Direct Investment

Another channel through which neighboring institutions might impact growth is foreign direct investment (FDI). To examine this possibility, I use home FDI flows as a percentage of total GDP as the dependent variable. The first-order differencing specification takes the following form:

$$(4)\ \Delta fdi_{home} = \alpha + \beta_1 \Delta saving_{home} + \beta_2 \Delta pop_{home} + \beta_3 \Delta educ_{home} + \beta_4 \Delta inst_{home} + \beta_5 \Delta inst_{neighbor} + \beta_6 \Delta growth_{neighbor} + \epsilon$$

Table 7 displays the results of this estimate as well as an alternative fixed effects approach. In both specifications home institutions are positive, but only in the fixed effects model are they significant at the 10% level. This supports the conclusions of Busse and Hefeker (2007), who find a positive relationship between FDI flows and institutions using a similar fixed effects approach. However, in both specifications, neighboring institutions remain insignificant. Therefore, I conclude that institutional spillovers have a minimal influence on home FDI flows, and that these flows do not explain the significant and positive impact of neighboring institutions on home economic growth.

4.4. Neighboring Institutions→Home Trade Volume

A third channel through which neighboring institutions could impact home growth rates is volume of trade. To test for this possibility, I estimate the following regression using the first order differencing approach,

(5) $$\Delta trade_{home} = \alpha + \beta_1 \Delta saving_{home} + \beta_2 \Delta pop_{home} + \beta_3 \Delta educ_{home} + \beta_4 \Delta inst_{home} + \beta_5 \Delta inst_{neighbor} + \beta_6 \Delta growth_{neighbor} + \epsilon$$

where $\Delta trade_{home}$ is the change in volume of trade in the home country, measured as *(Exports+Imports)/GDP*. Consistent with my previous methodology, I also augment this approach with a fixed effects specification. Results are expressed in Table 8.

In both specifications, neighboring institutions are positive and significant. Using the fixed effects model, a modest .1 point increase in neighboring institutions is expected to yield a 2.01% increase in trade share, while first-order differencing generates an even larger 4.59% gain. Furthermore, these coefficient estimates dwarf the impact of home institutions, implying that that openness to trade is highly influenced by the neighboring institutional environment. Reverse

causality represents an important caveat to this conclusion, as an increase in trade openness could potentially also incentivize the development of better neighboring institutions. However, given the previously discussed evidence in Rodrik et al. (2004), this interpretation seems the less likely of the two scenarios. Therefore, trade represents one plausible mechanism through which neighboring institutions might impact home growth rates.

4.5. Direct vs. Indirect Effects of Neighboring Institutions

To separately identify the direct and indirect effects of neighboring institutions, I add both FDI and Trade Volume to my base-line regressions (1) and (2). If openness and FDI are significant sources of economic growth within the home country, and are in turn influenced by neighboring institutions, we would expect the coefficient estimate on neighboring institutions to decline with the inclusion of these intermediaries. By including three possible channels through which neighboring institutions can function (home institutions, FDI, and trade volumes), I am able to isolate the direct effect of neighboring institutions on growth from any indirect effects functioning through these channels. Results are expressed in Table 9.

In both specifications, FDI is significant and positive at the 1% level, supporting past findings of a positive relationship between growth and FDI. However, somewhat surprisingly, Trade is insignificant, contradicting past literature including Frankel and Romer (1999) and Dollar and Kraay (2003). One explanation for these results might be the time period involved. For example Dollar and Kraay (2003) find a statistically significant impact of trade on growth, but only difference decade-long averages. Therefore, while institutions might be significant over the longer 25-year time period, perhaps changes in trade flows only impact growth rates in the short-run. While this would not explain the insignificance of trade using the fixed effects framework, it does perhaps shed light on the shortcomings of first-order differencing over a

relatively long timeframe. Another possible explanation is that the inclusion of home institutions is simply washing out any additional trade effect. This finding is in line with the conclusions of Rodrik et al. (2004).

More importantly, even with the inclusion of these controls, the neighboring institutions coefficient actually increases in magnitude and becomes more significant in the first-order differencing specification. Similarly, in the fixed effects model, although the standard error increases, the coefficient remains relatively constant compared to the baseline specification. Therefore, I am able to conclude that neighboring institutions influence home growth rates through primarily unobserved channels rather than the home institutions, trade, or FDI mechanisms.

4.6. Robustness

A fundamental robustness check is the use of alternative weighting mechanisms for neighboring institutions and neighboring growth rates. The significance of this weighting is seen in the example of Afghanistan and China. When weighted by their shared border length, China only contributes 1.4% of Afghanistan's neighboring institutions index, but when weighted by GDP, this percentage increases to 84.6%. Therefore, different weighting mechanisms have the potential to drastically change the composition of the neighboring institutions index. In addition to shared border length, I consider total surface area, GDP, and population size as alternative weighting mechanisms. Table 10 displays these alternatives for the baseline specification (1).

Although there is some variation in standard error and coefficient estimates, changing the weighting of neighboring institutions and growth rates does not significantly influence the results. With the exception of GDP, the standard errors and magnitudes of the neighboring coefficient estimates are similar across all specifications. However, I continue to use shared

border length as my primary weighting mechanism due to the endogeneity concerns surrounding the other weights. In the case of GDP, it seems possible that a country with a higher GDP will also have better institutions, and by extension be less likely to experience large fluctuations in institutional quality. Consequently, weighting by GDP would overvalue countries with negligible institutional changes over the specified time frame, putting downward pressure on the neighboring institutions index and biasing the coefficient upwards. Given the high degree of correlation between total population size, surface area, and GDP, these same endogeneity problems are present in the population and surface area mechanisms as well. Therefore, shared border length remains my principal weighting mechanism throughout all specifications.

To test whether a specific group of countries is driving the results, I generate dummy variables for advanced and non-advanced economies[18], and then interact these terms with the neighboring institutions index (See Table 11). The coefficient of each interaction term measures the impact of neighboring institutions on home growth rates given the home country's level of economic development. While the advanced economy interaction term has a negative coefficient and is insignificant, the non-advanced economy interaction term is both positive and significant. These findings suggest that institutional spillovers are most relevant for developing economies, as growth rates in advanced countries seem to be driven by largely non-institutional trends.

One potential issue with the first-order differencing approach of (1) is the lack of a "catch-up" term for the dependent variable. In other words, if a country is growing at 10% in 1984, even if it improves its institutions, it will probably not be able to improve growth rates very much over the 25 year period. Therefore, it might be necessary to control for initial growth, defined as the average annual growth rate from 1984-1988. Table 12 displays the results of this specification.

[18] Dummy variables are generated based on the Barro and Lee (2000) definitions.

The addition of this control greatly reduces the magnitude and significance level neighboring institutions, while also halving the coefficient estimate of home institutions. Additionally, the R^2 value increases from 0.32 to 0.73, indicating that the "catch-up" term explains a sizeable percentage of the variation in growth rates over the specified time frame. One explanation for this occurrence could be if initial growth rates are correlated with initial levels of home and neighboring institutions. If this were the case, these proxies for institutional "stock" could wash out a large portion of the institutional change over a 25 year period. More broadly, this result suggests the difficulty in reducing omitted variable bias by holding time-invariant fixed effects constant through first-order differencing. If initial levels of growth rates or institutions are actually main drivers of change over the specified period, then it becomes much more difficult to eliminate omitted variable bias in the form of other related factors, such as geography and disease environment. Therefore, while this inclusion of a "catch-up" term does not preclude the existence of institutional spillovers, it does highlight some of the endogeneity concerns limiting the current specifications.

5. Conclusion

I draw four main conclusions from my investigation of institutional spillover effects. First, neighboring institutions play a statistically significant and similar role to home institutions in determining home growth rates. Although not every measure of neighboring institutions registers as significant, a composite index of neighboring institutions is robust across different weighting mechanisms. This composite index shows that interactions among various proxies for neighboring institutions are important in determining the magnitude and significance of the spillover effect. Furthermore, the coefficient estimate is similar in size to a composite index of

home institutions, indicating that perhaps *regional* rather than strictly domestic changes in institutions are fundamental in determining growth rates.

Secondly, the evidence suggests that there are significant distinctions between the first-order differencing and fixed effects approaches. While both estimate changes in growth rates over time, the fixed effects specification measures higher frequency and presumably less gradual shifts than first-order differencing. In line with past research emphasizing the long-term persistence of institutions, I find that institutional spillovers mainly impact home growth rates in the long-run, while annual shifts in neighboring institutions have a much lesser effect.

A third conclusion of this paper is that shifts in institutions among neighboring countries occur independently of one another over the 25 year period. Although there is a great deal of evidence for institutional convergence among neighboring countries in the long-run due to shared colonial origins, geographic features, and so on, it appears that when held constant by first-order differencing, these exogenous instruments play little role in determining present day institutional changes. Somewhat contradicting this assertion is the significance of a "catch-up" term, evidence that these instruments might still exert some influence over medium and short-term institutional shifts. Nonetheless, if this were the case, I would have expected to see much higher correlations between home and neighboring changes in institutions over the specified time frame. One noteworthy exception to this conclusion is the implication that neighboring institutions can actually affect home institutions in the form of short-term shocks. By using fixed-effects instead of first-order differencing, I am able to show that higher frequency annual shifts between neighboring institutions are actually correlated. This seems to support a theory of institutional spillovers in the Middle East.

A final implication of this paper is that the mechanisms through which institutional spillovers occur remain largely unobserved. Even when controlling for home institutions and trade (both of which displayed some evidence of correlation with neighboring institutions), the neighboring institutions component remained constant, exerting an independent impact on home growth rates. The insignificance of trade in these specifications was somewhat surprising, but do support the findings of Rodrik et al. (2004). However, given the past body of literature supporting the significance of trade share on growth rates, I can not entirely reject the plausibility of a trade-based component to institutional spillovers.

These findings do not preclude other possible channels through which institutional externalities might function. One such mechanism could be the development of regional cooperation and integration, from power and transportation infrastructure to the establishment of regional security organizations. In this sense, positive institutions among neighboring countries could facilitate regional geo-political stability, effecting endogenous regional growth through channels unrelated to trade or FDI. Another possible channel which has not been fully explored is that of cultural spillovers. Within this framework, institutional spillovers occur not as a result of geographic proximity, but rather through cultural similarity. In this manner, a democratic revolution in Tunisia could illicit a response throughout the Arab world, even if the nations are not directly bordering one another.

Finally, the identification of institutional externalities has important implications for policy-makers. First and foremost, regionally oriented development policy seems to be essential given the equal importance of home and neighboring institutions in effecting growth. This implication is widely supported by anecdotal evidence; large growth accelerations such as those in East Asia are oftentimes regionally oriented. However, it has been largely assumed that this

phenomenon is caused by an independent convergence in "good" policies. My results suggest that this is not necessarily the case, as regional externalities developed by simultaneous institutional shifts among neighboring countries can actually generate far greater growth prospects than autonomous or unilateral change. Therefore, pressuring the poorest countries in a region to adopt better institutions is a flawed development strategy. Alternatively, aid and incentives should be directed comprehensively and spread out over entire regions instead of concentrated in the worst offenders.

Another important implication of these findings is the differentiation between institutions and policy. As Dani Rodrik notes, development incentives have often focused on "good governance" and policies, while largely ignoring underlying institutional weaknesses. (Rodrik 2007) This often leads to situations in which poor institutions are masked by ineffective policy shifts, an occurrence which can similarly superimposed onto the framework of institutional spillovers. For example, Murdoch and Sandler (2002) argue that civil wars in neighboring countries generate negative cross-border externalities. Therefore, a logical policy remedy might be to negotiate a cease-fire agreement or deploy international peacekeepers. However, if civil war is simply a product of bad institutions, these policy remedies will not address the underlying problem, and the negative spillover effects of poor institutions will persist throughout the region. From a development perspective, it remains much more difficult to implement good institutions than to coach sound policy. Nonetheless, it is this institutional approach which will generate the greatest regional growth externalities and minimize negative spillover effects among neighboring countries.

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Tables and Figures

Table 1: Summary Data 2005

	All Countries	Advanced Economies	Non-Advanced Economies
Sample Size	148	19	129
GDP per capita			
Mean	6489	28505	3143
Std. Dev.	11237	15619	4964
Min	89	4680	89
Max	74482	74482	29878
Growth Rate			
Mean	0.039	0.022	0.042
Std. Dev.	0.039	0.015	0.040
Min	-0.039	-0.001	-0.039
Max	0.251	0.070	0.251
Savings Rate			
Mean	13.582	8.783	14.321
Std. Dev.	17.245	6.642	18.247
Min	-12.639	-3.374	-12.639
Max	155.522	24.547	155.522
Population Growth			
Mean	1.495	0.698	1.612
Std. Dev.	1.370	0.446	1.421
Min	-1.590	-0.057	-1.590
Max	10.518	1.641	10.518
Education			
Mean	7.393	10.082	6.923
Std. Dev.	2.693	1.485	2.581
Min	1.239	6.474	1.239
Max	12.749	12.339	12.749
Home Institutions			
Mean	0.595	0.890	0.541
Std. Dev.	0.178	0.097	0.130
Min	0.235	0.667	0.235
Max	1.000	1.000	0.858
Neighboring Institutions			
Mean	0.574	0.842	0.536
Std. Dev.	0.148	0.111	0.108
Min	0.292	0.508	0.292
Max	0.970	0.970	0.856

Notes: All data is from 2005. Advanced and non-advanced economies replicate Barro and Lee (2000) definitions. GDP per capita is measured in 2000 US dollars. Home and neighboring institutions are composite scores, with neighboring institutions weighted by shared border length.

Table 2: Variation in Home Institutions 1984-2009

Sample	Observations	Average Std. Dev. Institutions
All	118	0.0683472
Advanced	17	0.038386
Non-Advanced	101	0.0733901
South Asia	3	0.0972717
Europe/Central Asia	20	0.0430154
Middle East/North Africa	18	0.068695
Sub-Saharan Africa	31	0.0728021
Latin America	20	0.1032771
East Asia/Pacific	9	0.0779293

Notes: All data is from 1984-2009. For each country, standard deviation of home institutions (composite index) is calculated over the specified time period. These standard deviations are then averaged by region. Regional dummies replicate Barro and Lee (2000) definitions.

Table 3: Core Specification

	Fixed Effects		First Order Differencing	
Dependent Variable	**log GDP**	**Annual Growth**	**Δ log GDP 1984-2009**	**Δ Annual Growth 1984-2009**
Saving	0.003***	0.001***	-0.008**	0.001***
	(7.635)	(7.594)	(-2.514)	(3.188)
Population	0.024***	-0.004**	-0.012	-0.011***
	(4.212)	(-2.181)	(-0.227)	(-2.647)
Education	0.119***	-0.001	0.034	-0.005
	(20.513)	(-0.550)	(0.586)	(-1.010)
Home Institutions	0.332***	0.035***	0.282	0.078***
	(8.400)	(2.837)	(0.865)	(3.048)
Neighboring Institutions	-0.153***	0.027	-0.428	0.076**
	(-2.833)	(1.573)	(-0.933)	(2.113)
Neighboring Growth	0.346***	0.260***	0.128	0.018
	(3.978)	(9.556)	(0.066)	(0.119)
Country Fixed Effects	Yes	Yes	No	No
Number of Observations	1752	1750	74	74
R^2	0.99	0.29	0.11	0.32

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. log GDP is per capita and in 2000 US$. Annual Growth is $(lnGDP_t - lnGDP_{t-1})$. Δ log GDP 1984-2009 and Δ Annual Growth 1984-2009 are calculated by differencing average values between 1984-1988 from average values between 2005-2009. Independent variables in first-order differencing specifications are similarly constructed. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Table 4A: Measures of Institutions—First-Order Differencing

Institutional Measure:	Composite Index	Bureaucracy	Corruption	Executive Constraint	Investment Profile	Law and Order	Democracy
First-Order Differencing							
Dependent Variable: Δ Annual Growth 1984-2009							
Savings	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(3.188)	(2.941)	(2.924)	(3.643)	(3.770)	(2.751)	(3.005)
Population	-0.011***	-0.011**	-0.011**	-0.012***	-0.015***	-0.010**	-0.013***
	-(2.647)	-(2.441)	-(2.457)	-(2.956)	-(3.208)	-(2.144)	-(3.025)
Education	-0.005	-0.004	-0.005	-0.004	-0.006	-0.006	-0.007*
	-(1.010)	-(0.956)	-(0.966)	-(0.872)	-(1.242)	-(1.234)	-(1.749)
Home Institutions	0.078***	0.008**	0.003	0.002	0.006***	0.007**	0.006**
	(3.048)	(2.242)	(0.886)	(1.037)	(2.676)	(2.329)	(2.359)
Neighboring Institutions	0.076**	0.002	0.009	0.005**	0.001	-0.004	0.001
	(2.113)	(0.295)	(1.617)	(1.981)	(0.348)	-(0.777)	(0.246)
Neighboring Growth	0.018	0.160	0.138	0.037	0.109	0.194	0.127
	(0.119)	(0.989)	(0.879)	(0.246)	(0.719)	(1.201)	(0.850)
Country Fixed Effects	No	No	No	No	No	No	No
Number of Observations	74	74	75	82	75	75	77
R^2	0.32	0.26	0.24	0.26	0.28	0.27	0.27

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Δ Annual Growth 1984-2009 is calculated by differencing average values 1984-1988 from average values 2005-2009. Independent variables are similarly constructed. The composite indexes for home and neighboring institutions are calculated as equally weighted averages of bureaucracy, corruption, executive constraint, investment profile, and law and order. Neighboring institutions and neighboring growth rates are weighted by shared border length. Constants are not reported.

Table 4B: Measures of Institutions—Fixed Effects

Institutional Measure:	Composite Index	Bureaucracy	Corruption	Executive Constraint	Investment Profile	Law and Order	Democracy
Fixed Effects							
Dependent Variable: Annual Growth							
Savings	0.001***	0.001***	0.001***	0.000	0.001***	0.001***	0.001***
	(7.594)	(7.691)	(7.664)	(1.208)	(7.348)	(7.490)	(4.774)
Population	-0.004**	-0.003*	-0.003*	0.001	-0.004**	-0.003	-0.004**
	-(2.181)	-(1.732)	-(1.705)	(0.644)	-(2.010)	-(1.456)	-(2.044)
Education	-0.001	0.002	0.001	0.004**	-0.002	0.001	-0.001
	-(0.550)	(1.241)	(0.452)	(2.183)	-(1.160)	(0.802)	-(0.307)
Home Institutions	0.035***	0.000	-0.000	0.002*	0.004***	0.003***	0.005***
	(2.837)	(0.047)	-(0.138)	(1.860)	(5.610)	(2.587)	(4.529)
Neighboring Institutions	0.027	0.000	-0.003	-0.001	-0.001	0.001	0.005***
	(1.573)	(0.124)	-(1.425)	-(0.552)	-(1.451)	(0.434)	(2.910)
Neighboring Growth	0.260***	0.241***	0.234***	0.470***	0.230***	0.236***	0.378***
	(9.556)	(8.590)	(8.397)	(17.168)	(8.245)	(8.514)	(14.202)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1750	1784	1806	2012	1809	1809	1882
R^2	.0.29	0.28	0.28	0.27	0.30	0.28	0.30

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Annual Growth is $(lnGDP_t - lnGDP_{t-1})$. The composite indexes for home and neighboring institutions are calculated as equally weighted averages of bureaucracy, corruption, executive constraint, investment profile, and law and order. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Table 5: Correlations between $\Delta inst_{home}$ and $\Delta inst_{neighbor}$

Institutional Measure	Home/Neighbor Correlation
Composite Index	-0.162
Bureaucracy	0.011
Corruption	-0.120
Executive Constraint	0.004
Investment Profile	-0.005
Law and Order	0.103
Democracy	0.112

Notes: Simple correlations are between changes in home and neighboring institutions from 1984-2009. The composite index is an equally weighted average of bureaucracy, corruption, executive constraint, investment profile, and law and order.

Table 6: Interactions between Home and Neighboring Institutions

Dependent Variable: Home Institutions	Fixed Effects	First-Order Differencing
Home Growth	0.139*** (2.837)	1.561*** (3.048)
Saving	0.001*** (3.046)	-0.000 -(0.220)
Population	0.003 (0.773)	0.029 (1.470)
Education	0.035*** (9.994)	0.008 (0.378)
Neighboring Institutions	0.147*** (4.396)	-0.372** -(2.321)
Neighboring Growth	-0.053 -(0.960)	0.885 (1.315)
Country Fixed Effects	Yes	No
Number of Observations	1750	74
R^2	0.86	0.20

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables in first-order differencing specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Table 7: Foreign Direct Investment

Dependent Variable: FDI/GDP	**Fixed Effects**	**First-Order Differencing**
Saving	0.006	0.049
	(0.437)	(1.458)
Population	-0.013	0.564
	-(0.162)	(0.984)
Education	0.953***	-0.737
	(5.776)	-(1.244)
Home Institutions	2.149*	3.923
	(1.830)	(1.150)
Neighboring Institutions	2.495	-6.653
	(1.534)	-(1.355)
Neighboring Growth	3.357	17.567
	(1.321)	(0.874)
Country Fixed Effects	Yes	No
Number of Observations	1731	72
R^2	0.29	0.17

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. FDI/GDP is foreign direct investment as a percentage of total GDP. Dependent and independent variables in first-order differencing specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Table 8: Trade

Dependent Variable: Trade/GDP	**Fixed Effects**	**First-Order Differencing**
Saving	0.137***	0.157
	(3.066)	(0.858)
Population	-1.483***	-0.656
	-(5.120)	-(0.206)
Education	6.913***	0.516
	(11.754)	(0.155)
Home Institutions	14.891***	8.766
	(3.600)	(0.453)
Neighboring Institutions	20.068***	45.857*
	(3.541)	(1.680)
Neighboring Growth	11.890	-25.345
	(1.314)	-(0.221)
Country Fixed Effects	Yes	No
Number of Observations	1759	75
R^2	0.89	0.05

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Trade/GDP is volume of trade *(imports+exports)* as a percentage of total GDP. Dependent and independent variables in first-order differencing specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Table 9: Direct vs. Indirect Effect of Neighboring Institutions

Dependent Variable: Annual Growth Rates	**Fixed Effects**	**First-Order Differencing**
Savings	0.001*** 6.918	0.001*** 2.718
Population	-0.004** -2.003	-0.012*** -2.853
Education	-0.003 -1.390	-0.002 -0.413
Home Institutions	0.032*** 2.623	0.066*** 2.691
Neighboring Institutions	0.021 1.240	0.109*** 3.021
Neighboring Growth	0.251*** 9.506	-0.040 -0.274
Home FDI/GDP	0.001*** 3.588	0.003*** 3.237
Home Trade/GDP	0.000 0.959	-0.000 -1.456
Country Fixed Effects	Yes	No
Number of Observations	1722	71
R^2	0.31	0.35

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. FDI/GDP is foreign direct investment as a percentage of total GDP. Trade/GDP is volume of trade *(imports+exports)* as a percentage of total GDP. Dependent and independent variables in first-order differencing specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Table 10: Weighting Mechanisms

Weighting Mechanism: Dependent Variable: Δ Annual Growth 1984-2009	**Area**	**Border**	**GDP**	**Population**
Saving	0.001***	0.001***	0.001***	0.001***
	(3.163)	(3.188)	(2.990)	(3.228)
Population	-0.011***	-0.011***	-0.015***	-0.012***
	-(2.685)	-(2.647)	-(3.222)	-(2.766)
Education	-0.005	-0.005	-0.005	-0.004
	-(1.023)	-(1.010)	-(0.963)	-(0.906)
Home Institutions	0.069***	0.078***	0.057**	0.068***
	(2.732)	(3.048)	(2.144)	(2.656)
Neighboring Institutions	0.051*	0.076**	0.039	0.051
	(1.650)	(2.113)	(1.023)	(1.616)
Neighboring Growth	0.064	0.018	-0.006	-0.012
	(0.426)	(0.119)	-(0.038)	-(0.069)
Country Fixed Effects	No	No	No	No
Number of Observations	74	74	71	73
R^2	0.31	0.32	0.30	0.30

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by total surface area, shared border length, GDP, and population. Country fixed effects dummies and constants are not reported.

Table 11: Advanced vs. Non-Advanced Economies

Dependent Variable:	**Δ Annual Growth 1984-2009**
Saving	0.001*** (2.988)
Population	-0.011** -(2.480)
Education	-0.004 -(0.932)
Home Institutions	0.081*** (3.125)
Neighboring Institutions (Adv. Econ)	-0.011 -(0.110)
Neighboring Institutions (Non-Adv. Econ)	0.086** (2.278)
Neighboring Growth	0.006 (0.041)
Country Fixed Effects	No
Number of Observations	74
R^2	0.33

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Table 12: First-Order Differencing Controlling for Initial Growth

Dependent Variable:	**Δ Annual Growth 1984-2009**
Savings	0.000* (1.668)
Population	-0.004 -(1.363)
Education	-0.005* -(1.820)
Home Institutions	0.043** (2.560)
Neighboring Institutions	0.022 (0.951)
Neighboring Growth	0.132 (1.349)
Home Initial Growth	-0.841*** -(9.934)
Country Fixed Effects	No
Number of Observations	74
R^2	0.73

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. Home initial growth is average annual growth rate from 1984-1988. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Country fixed effects dummies and constants are not reported.

Figure 1: Home Institutions 1984-2009

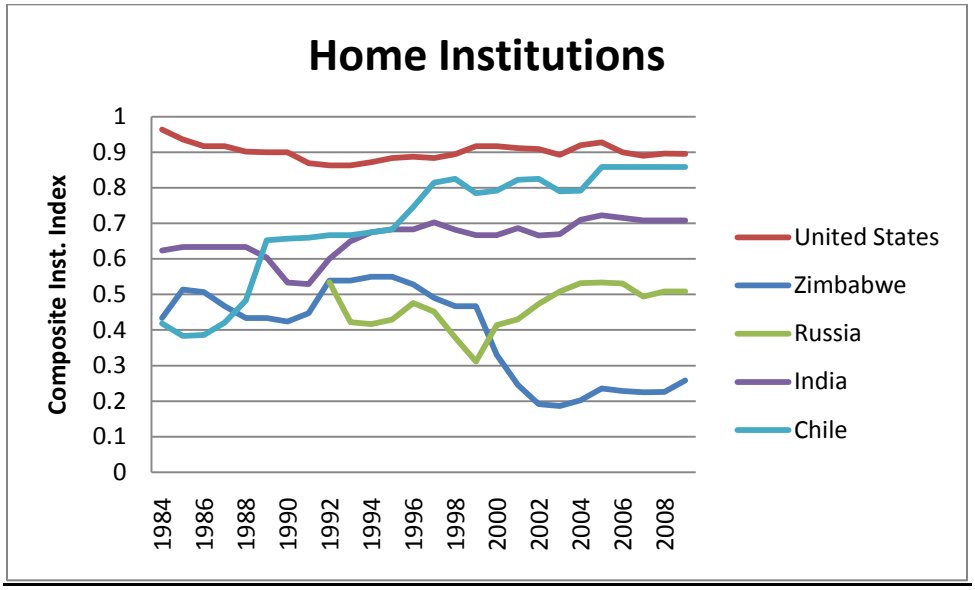
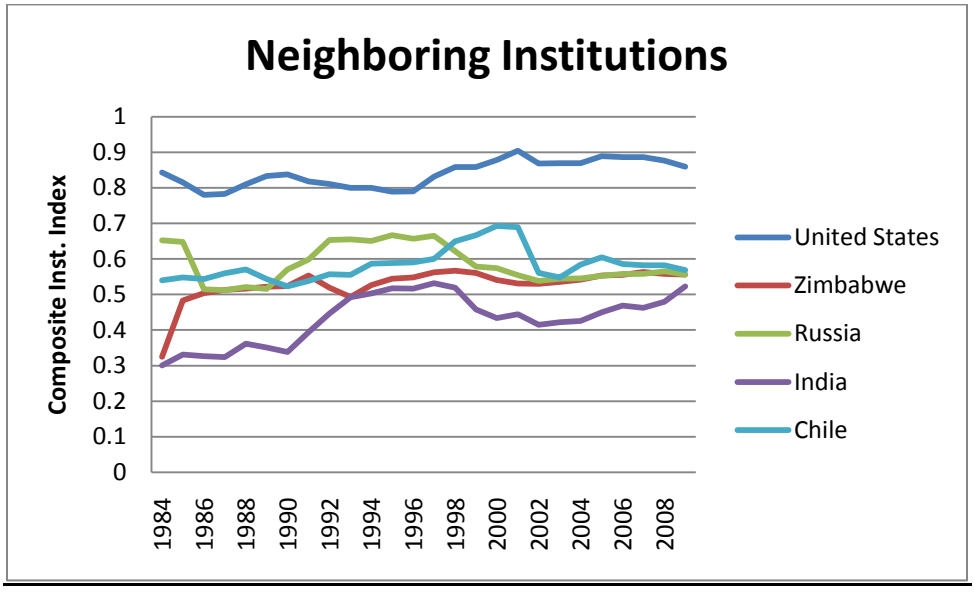


Figure 2: Neighboring Institutions 1984-2009



Appendix A

Table A: Interactions between Home and Neighboring Institutions-Area

	Bureaucracy	Corruption	Executive Constraint	Composite Index	Investment Profile	Law and Order	Democracy
Dependent Variable: Home Institutions Weighting: Area							
Home Growth	9.148** (2.226)	3.994 (0.905)	6.255 (0.797)	1.457*** (2.732)	16.222*** (2.692)	10.704** (2.367)	11.875** (2.334)
Saving	0.001 (0.089)	0.009 (0.967)	0.003 (0.169)	0.000 (0.157)	-0.038*** -(2.682)	0.009 (0.862)	0.003 (0.246)
Education	0.055 (0.336)	0.127 (0.746)	-0.172 -(0.535)	0.007 (0.342)	0.256 (1.066)	0.222 (1.225)	0.454*** (2.586)
Population	0.115 (0.744)	0.352** (2.181)	0.207 (0.691)	0.026 (1.309)	0.836*** (3.662)	-0.099 -(0.570)	0.486*** (2.604)
Neighboring Institutions	**0.107 (0.640)**	**-0.164 -(1.016)**	**-0.026 -(0.146)**	**-0.105 -(0.722)**	**-0.063 -(0.363)**	**0.137 (0.881)**	**-0.151 -(1.118)**
Neighboring Growth	-7.211 -(1.317)	5.699 (1.032)	13.780 (1.383)	0.324 (0.470)	-0.667 -(0.085)	-3.244 -(0.526)	-1.486 -(0.227)

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by surface area. Constants are not reported.

Table B: Interactions between Home and Neighboring Institutions-Border

	Bureaucracy	**Corruption**	**Executive Constraint**	**Composite Index**	**Investment Profile**	**Law and Order**	**Democracy**
Dependent Variable: Home Institutions Weighting: Border							
Home Growth	9.222** (2.242)	3.761 (0.886)	7.972 (1.037)	1.561*** (3.048)	16.017*** (2.676)	10.626** (2.329)	12.002** (2.359)
Saving	0.002 (0.187)	0.009 (0.946)	-0.003 -(0.154)	-0.000 -(0.220)	-0.040*** -(2.827)	0.008 (0.787)	0.002 (0.129)
Education	0.033 (0.202)	0.135 (0.810)	-0.163 -(0.527)	0.008 (0.378)	0.265 (1.105)	0.227 (1.251)	0.454*** (2.577)
Population	0.101 (0.638)	0.372** (2.316)	0.315 (1.064)	0.029 (1.470)	0.881*** (3.820)	-0.118 -(0.651)	0.500*** (2.640)
Neighboring Institutions	**0.138 (0.662)**	**-0.300 -(1.618)**	**-0.245 -(1.363)**	**-0.372** -(2.321)**	**-0.095 -(0.547)**	**0.045 (0.249)**	**-0.166 -(1.040)**
Neighboring Growth	-6.106 -(1.081)	7.479 (1.360)	21.887** (2.233)	0.885 (1.315)	4.559 (0.580)	-2.430 -(0.381)	1.912 (0.287)

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by shared border length. Constants are not reported.

Table C: Interactions between Home and Neighboring Institutions-GDP

	Bureaucracy	**Corruption**	**Executive Constraint**	**Composite Index**	**Investment Profile**	**Law and Order**	**Democracy**
Dependent Variable: Home Institutions Weighting: GDP							
Home Growth	8.433** (2.032)	3.255 (0.737)	7.933 (1.014)	1.186** (2.144)	15.295** (2.483)	10.012** (2.259)	12.213** (2.464)
Saving	0.004 (0.365)	0.010 (1.036)	-0.001 -(0.063)	-0.000 -(0.011)	-0.042*** -(2.898)	0.011 (1.080)	0.001 (0.102)
Education	0.046 (0.272)	0.162 (0.934)	-0.091 -(0.286)	0.016 (0.728)	0.263 (1.087)	0.230 (1.281)	0.447** (2.566)
Population	0.095 (0.585)	0.338** (1.982)	0.343 (1.093)	0.017 (0.733)	0.908*** (3.804)	-0.108 -(0.605)	0.480** (2.499)
Neighboring Institutions	**0.188 (1.117)**	**-0.032 -(0.181)**	**-0.159 -(0.813)**	**-0.159 -(0.917)**	**-0.099 -(0.590)**	**0.220 (1.504)**	**-0.237* -(1.833)**
Neighboring Growth	-6.499 -(1.152)	3.337 (0.591)	18.165* (1.823)	0.464 (0.646)	7.831 (0.973)	-5.749 -(0.972)	-2.386 -(0.367)

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by GDP. Constants are not reported.

Table D: Interactions between Home and Neighboring Institutions-Population

	Bureaucracy	Corruption	Executive Constraint	Composite Index	Investment Profile	Law and Order	Democracy
Dependent Variable: Home Institutions Weighting: Population							
Home Growth	8.730** (2.106)	3.597 (0.803)	6.725 (0.853)	1.421*** (2.656)	15.932*** (2.626)	10.365** (2.316)	11.955** (2.407)
Saving	0.002 (0.176)	0.011 (1.096)	0.002 (0.125)	0.000 (0.246)	-0.039*** -(2.731)	0.010 (0.958)	0.001 (0.055)
Education	0.049 (0.293)	0.154 (0.890)	-0.102 -(0.314)	0.010 (0.474)	0.246 (1.012)	0.240 (1.329)	0.474*** (2.729)
Population	0.112 (0.700)	0.328** (1.965)	0.242 (0.786)	0.026 (1.256)	0.854*** (3.660)	-0.103 -(0.587)	0.509*** (2.695)
Neighboring Institutions	**0.123 (0.745)**	**-0.068 -(0.396)**	**-0.018 -(0.099)**	**-0.038 -(0.255)**	**-0.087 -(0.509)**	**0.168 (1.079)**	**-0.231* -(1.780)**
Neighboring Growth	-6.513 -(1.076)	2.695 (0.441)	15.144 (1.390)	0.131 (0.170)	1.851 (0.215)	-3.973 -(0.589)	-1.363 -(0.194)

Notes: Data is from 1984-2009. T-scores are reported below the coefficient estimates in parentheses. *** p<.01, ** p<.05, * p<.10. Dependent and independent variables specification are calculated by subtracting average values between 1984-1988 from average values between 2005-2009. The home and neighboring institutions variables are composite indexes. Neighboring institutions and neighboring growth rates are weighted by population. Constants are not reported.