EXIT DURING CRISIS: HOW OPENNESS, MIGRATION, AND ECONOMIC CRISIS AFFECT DEMOCRATIZATION

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ABSTRACT

Does economic crisis lead to authoritarian regime breakdown and democratization? In this paper, I argue that the availability of exit options for citizens conditions the relationship between economic crisis and democratization. Where citizens have more viable exit alternatives, economic crisis causes citizens to exit rather than protest, making democratization less likely. I measure exit options in three ways: a geographic instrument for bilateral trade; neighboring country GDP per capita; and past net migration. I use time series, cross-section data on up to 122 authoritarian regimes in 114 countries from 1946–2002 to test this argument and find evidence consistent with the hypothesis that more attractive exit options insulate dictators from the liberalizing effects of economic crisis.

RESUMEN

¿Las crisis económicas conducen al quiebre de los regímenes autoritarios y a la democratización? En este artículo sostengo que la disponibilidad de opciones de salida para los ciudadanos condiciona la relación entre las crisis económicas y la democratización.Donde los ciudadanos tienen más salidas alternativas viables, la crisis económica hace que los ciudadanos opten por salir en lugar de protestar, haciendo de este modo menos probable la democratización. Mido las opciones de salida de tres formas: con un instrumento geográfico del comercio bilateral, a través del Producto Bruto per cápita en los países vecinos y con la migración neta anterior. Para poner a prueba este argumento utilizo datos de serie temporal y cros-seccionales acerca de hasta 122 regímenes autoritarios en 114 países desde 1946 hasta 2002. Encuentro evidencia consistente con la hipótesis de que las opciones de salida más atractivas protegen a los dictadores de los efectos liberalizantes de las crisis económicas.

Many scholars have examined whether democracy affects economic growth (Sirowy & Inkeles 1990, Przeworski, Alvarez, Cheibub & Limongi 2000, Rodrik & Wacziarg 2005, Papaioannou & Siourounis 2005, Persson & Tabellini 2006). Yet we still know relatively little about the reverse question: Does economic growth sustain dictatorships? When does economic crisis lead to democratization? Earlier research contends that economic crisis is an important determinant of democratization, suggesting that poor economic performance can increase the likelihood of democratization (Richard 1986, Markoff & Barreta 1990, Gasiorowski 1995, Haggard & Kaufman 1995, Bratton & van de Walle 1997, Przeworski et al. 2000, Acemoglu & Robinson 2001). The basic causal story in this literature is that economic crises can affect democratization by creating a focal point for opposition mobilization; causing business elites and capitalists to defect from the authoritarian bargain; or creating division within the regime over economic policy in response to the crisis.[1]

However, there are prominent examples to the contrary. In Zimbabwe, Mugabe's regime has remained in power and in fact become more authoritarian during the protracted economic crisis that began in 1999.[2] In Togo, numerous, severe economic contractions failed to unseat Eyadema; his rule lasted thirty-eight years until his death in 2005.[3] In both cases, economic crisis both provided a focal point for regime opposition and created division within the ruling elite over economic policy.[4] Though rattled at various times, both Mugabe and Eyadema appear to have weathered the storm.[5] Further, there are numerous dominant single-party regimes that have survived periods of severe economic decline, including Gabon (late 1970s) and Malaysia (late 1990s). That some dictators appear to be impervious to economic crisis while others democratize when faced with a crisis, suggests that we need a clearer answer to the question of whether economic growth keeps dictators in power.

Much of the literature on democratization focuses on structural conditions that underpin a successful transition to democracy: level of development (Lipset 1959, Bollen & Jackson 1979, Burkhart & Lewis-Beck 1994, Przeworski et al. 2000); asset equality (Dahl 1971, Acemoglu & Robinson 2001, Acemoglu & Robinson 2006, Boix 2003); asset mobility (Boix 2003); and linkages with the West (Levitsky & Way 2005, Levitsky & Way 2006). In this literature it is argued, for example, that higher levels of development,

more capital mobility, higher equality, and better linkages offer more favorable conditions for democratization. Other scholars, however, have noted that contingent events, in particular economic crises, can disrupt authoritarian rule and set the stage for democratization (Linz & Stepan 1978, Haggard & Kaufman 1995, Gasiorowski 1995).

In this paper, I examine the interaction between particular structural conditions and economic crisis. I argue that the attractiveness of exit options available to citizens during times of crisis conditions the relationship between economic crisis and democratization. Citizens with more attractive exit options during an economic crisis are more likely to migrate instead of pressing the regime for democratic reforms. If the causal story that links economic crisis to democratization entails citizens defecting from the authoritarian bargain and mobilizing against the dictator, then the ability of citizens to move and migrate should sever the causal link between economic crisis and political liberalization. Here I turn arguments about openness, mobility, and linkages on their head and suggest that these conditions may not always make democratization more likely. So while I do not dispute the contention that, by themselves, certain structural characteristics, such as better linkages or fewer fixed assets, may provide favorable conditions for democracy, I argue that these conditions actually work against democratization during periods of economic crisis.

One way to conceptualize the "attractiveness of exit options" is to understand them as linkages between countries garnered through trade. Trade openness, or more precisely high trade levels, is often used as a measure of globalization (Berger 2000, Garrett 2000, Brune & Garrett 2005). Increasing globalization, in turn, has been used to explain democratization, through a myriad of causal pathways.[6] Some of these explanations suggest that globalization, implying higher trade levels, can reduce information costs, increase contacts with democracies, and make pro-democracy international nongovernmental organizations more effective precisely because they provide links between democracies and dictatorships (Diamond 1992, Schmitter 1996). Levitsky & Way (2006) argue that linkages to the West are the key component of international influence on democratization. More specifically, they define "linkages" as follows:

> Countries that are geographically proximate to—or located within the same
> region as—the US and the EU tend to have closer economic ties, more

extensive inter-governmental contact, and higher cross-border flows of people, information, and organizations. However, linkage may also be a product of colonial heritage, military occupation, or longstanding geo-political alliances, and it may be enhanced by ethnic, religious, and linguistic proximities that are unrelated to geography. Linkage is also a product of socioeconomic development, which tends to expand cross-border economic activity, communication, and travel.

Note that most of the factors comprising this definition of linkage are also found in determinants of trade flows: geography, colonial heritage, linguistic proximity, and socioeconomic development. Perhaps the only major determinant of trade flows not mentioned here, aside from policy, is the size of the country (smaller countries trade more). Here I suggest that trade openness is also a useful proxy for international linkages that reduce the cost of exit for citizens in a dictatorship enduring economic crisis. Later, I construct a trade instrument from a gravity model that uses geographic determinants of international trade as a proxy for the cost of exit during crisis.

I conceptualize exit mobility as the benefits (net of the costs) of migrating during a period of economic crisis. Trade is one of three variables I employ as proxies for exit mobility: a gravity model of bilateral trade; neighboring country GDP per capita; and net migration over the past ten years. While migration may be the most direct measure of exit mobility, a gravity model of bilateral trade contains many of the same determinants of migration, such as distance, remoteness, and common language. As Lucas (2005) argues, "in practice, it seems that trade and migration flows follow quite common paths. Trade and migration both generate contacts leading to channels of information and familiarity which facilitate flows of both goods and people, both concentrated over shorter distances" (2005, 9).

Neighboring country GDP per capita is used to capture the extent to which wages in nearby countries are higher (or lower) than wages in the home country, thereby making labor exit more (or less) attractive in times of crisis. The insight that relative wages, or differences in earnings, drive migration has been used to model migration (Sjaastad 1962, Todaro 1969, Stark & Taylor 1989) and is a key part of a recent proposal to use migration as a major tool of international development (Clemens, Montenegro & Pritchett 2009). Ideally, I would have consistent time series data on average wages in most or all

dictatorships. However, average wage data is relatively scarce in many authoritarian countries, particularly historically. I therefore rely on GDP per capita as a plausible proxy for average wages in neighboring countries, while controlling for home country GDP per capita.

Because I argue that exit mobility, in part measured as trade openness, conditions the relationship between economic crisis and democratization, this research also engages the literature examining globalization's effect on democracy. Recently, scholars have begun to investigate whether democratization causes changes in trade openness (Milner & Kubota 2005) and the conditions under which democratization might influence trade policy (Kono 2006, Kono 2008). But the empirical literature on whether trade openness causes democratization is relatively scant. Li & Reuveny (2003) test whether trade helps or hinders democracy and find that trade openness decreases democracy. This research, however, does not address important questions about the direction of causation, nor does it discuss how economic openness might condition the effect of growth on democracy. Rudra (2005) also examines how globalization affects democratization, arguing that globalization increases democracy when countries spend more on social welfare. Because this research studies welfare spending, however, the scope of the empirical tests are limited and weighted heavily towards democracies. In a recent paper, López-Córdova & Meissner (2008) use a gravity model of trade to examine how trade levels affect democracy in the long run, paying particular attention to capturing the exogenous effect of trade. They find that, in general, trade increases democracy, though the effect varies by time period. I build on this literature, as well as the economic literature on trade (Bergstrand 1985, Frankel & Romer 1999, Rose 2004), to construct a measure of trade that is exogenous to the process of democratization.

In the next section, I explore how economic openness insulates dictators from the liberalizing effect of economic crisis. This discussion highlights how the option of citizen exit and migration can be added to formal theories of democratization, altering those that focus on conflict between two classes. In the third section, I discuss the data and research design used to test the main hypothesis. I use three related measures of exit mobility: a gravity model of trade that employs geographic instruments for trade; neighboring country GDP per capita; and past migration patterns. In the fourth section, I present the

results of empirical tests on up to 122 authoritarian regimes in 101 countries from 1948–2002. The final section concludes with a discussion of the implications for future research.

Before proceeding, it is important to point out that I focus exclusively on the conditions under which economic crisis should affect the likelihood of democratization *in authoritarian regimes*. Thus, the theory and empirical tests rely on the experiences of dictatorships, and do not address what makes new democracies endure or consolidate. Because I employ trade openness as a measure of exit mobility, this project has implications for the literature on globalization and democracy. Previous empirical research that addresses how trade openness affects the level of democracy uses samples of both dictatorships and democracies (often including wealthy, well-established democracies), and thus necessarily mixes questions about democratization in authoritarian regimes with questions about democratic survival and consolidation (Rudra 2005, López-Córdova & Meissner 2008, Li & Reuveny 2003). The samples used in the empirical tests in the present study include only authoritarian regimes.

ECONOMIC CRISIS AND DEMOCRATIZATION

While one of the most studied questions in comparative politics is the relationship between economic development and the prospect of democratization (Acemoglu & Robinson 2006, Epstein, Bates, Goldstone, Kristensen & O'Halloran 2006, Boix & Stokes 2003, Przeworski et al. 2000, Lipset 1959), scholars have also long noted that economic crisis, or a short-term negative shock to economic output, can destabilize dictatorships and lead to democratization (Bratton & van de Walle 1997, Gasiorowski 1995, Haggard & Kaufman 1995, Callaghy 1990, Markoff & Barreta 1990, Richard 1986).[7] An economic crisis can disrupt the equilibrium of power in an authoritarian regime in a number of ways. Crisis can (1) provide a focal point for opposition protest (Bratton & van de Walle 1997); (2) create division within the regime itself over the appropriate response to the economic downturn (O'Donnell & Schmitter 1986); or (3) deplete the resources available to the regime to pay off nominal allies or repress potential opponents, leading to the defection of key supporters (Haggard & Kaufman 1995) and/or a decrease in collective action costs for potential protestors.

As Haggard & Kaufman (1995) argue, these causal mechanisms that link economic crisis to "authoritarian withdrawal" are likely interrelated. For example, dictators may garner support from business elites. When economic crisis hurts their profits, business elites may defect from the coalition of regime supporters and may even join the opposition. The authoritarian elite response to business elite defection may then cause division within the regime over how to respond. Further, economic crisis may compel the regime to withdraw subsidies (e.g., public employment, consumer subsidies) that have previously kept middle-class wageworkers in the support coalition, which in turn may lead to protest mobilization against the regime. Again, the defection and protest of the wageworkers may sow dissent within the regime itself.

These three causal mechanisms are certainly not exhaustive or mutually exclusive. One way to distinguish or categorize these (and other) causal paths is to think about them as the extent to which they entail substitution available to (1) the dictator and (2) citizens. Business elite defection from the authoritarian bargain, for example, is the result of a strategic decision that weighs support for the regime against alternative options such as fleeing the country or joining the opposition. Likewise, the extent to which a dictator cuts consumer subsidies or imposes inflationary taxes during an economic crisis is related to the availability of "unearned" sources of income such as oil rents or foreign aid. Finally, the likelihood of protest (and conflict) mobilization and its success is certainly a function of alternatives open to potential protestors.

Previous research has explored the effect of both types of substitution on democracy. For example, building on the literature that stresses democratization as a concession necessary to raise tax revenue (North & Weingast 1989, Levi 1988, Tilly 2004), scholars have argued that the availability of substitutes for taxation, such as oil rents or foreign aid, can deter democratization (Beblawi & Luciani 1987, Djankov, Montalvo & Reynal-Querol 2008, Morrison 2007, Morrison 2009).[8] An implication of these arguments would suggest that when a dictator does not rely on taxes from domestic economic production, an economic crisis is less likely to impinge on the dictator's ability to payoff his support coalition and retain power. These arguments focus on the availability of substitutes for tax proceeds for a *dictator*, implying that the availability of these non-tax resources may shelter a dictator from the liberalizing effect of economic

crisis. While the main analysis of this paper focuses on the availability of substitutes for citizens, it is important to note the possibility that substitutes available to dictators may also condition the relationship between economic crisis and democratization.[9]

Rogowski (1998) argues that the ability of citizens to exit a regime—the relatively low cost of migration—can lead to democratization,[10] while Boix (2003) shows that high asset mobility ensures that the median voter in a democracy cannot set high tax rates, making democracy safer for rich, asset-holding elites. These two arguments stress the viability of substitution for *citizens* in an autocracy. Rogowski's analysis is applicable to all citizens—rich and poor—while Boix's argument focuses on the ability of elite citizens to move their assets abroad. These theories link the availability of exit options directly to democracy. The contribution of the present study, however, is to explore how the availability of exit options influence the behavior of citizens *during economic crisis*, thereby specifying the conditions under which economic crisis should be more likely to cause democratization.

By definition, an economic crisis decreases some citizens' income. Further, when faced with declining tax revenue as result of economic crisis, the dictator may be tempted to increase the tax rate on some citizens to make up for lost revenue, which is necessary for the dictator to pay off and retain his support coalition. For example, a tax rate increase might include: inflationary spending that reduces citizens' real income (Bolivia); physically seizing citizens' land and capital assets (Zimbabwe); or reducing consumer subsidies on fuel (Burma, Iran). Both citizens' income loss from the crisis itself and potential income loss from a tax rate increase force citizens to weigh keeping their assets (labor and capital) in the domestic economy against moving their assets abroad. However, to understand how citizens respond to the crisis, we need to consider the options available to citizens in their interaction with the dictator. Here citizens have three options: voice, exit, or loyalty.[11] When faced with a crisis, citizens can keep their assets in the country and not pressure the regime for change (remain loyal, acquiesce, not revolt); they can keep their assets in the country and pressure the regime for change (voice, fight, revolt); or they can move their assets out of the country (exit). If citizens can exit at a relatively low cost (or with relatively high benefits) during an economic crisis, their departure may very well exacerbate the economic decline, but it will also reduce their incentive to

pressure the dictator to liberalize the polity. When exit is costly, however, citizens have a stronger incentive to pressure the government to liberalize when economic crisis hits if the exit option is more costly than remaining home (either acquiescing to or resisting the regime).

This logic can be applied in a straightforward way to formal theories of democratization that posit conflict between two classes as the main explanatory device driving democracy (Wood 2000, Acemoglu & Robinson 2001, Acemoglu & Robinson 2006, Boix 2003, Morrison 2007). These models typically pit a lower class (the poor, citizens, or insurgents) against an elite class (the rich, the dictator/regime, or the government). In response to the income distribution set by the elite, the lower class typically has two primary actions: revolt (fight) or not revolt (acquiesce). The elite class or regime player then responds by deciding whether to democratize. Adding an exit option for citizens to these democratization games or adding a voice/protest move to a decision over whether or not to exit[12] forces the citizens to decide not just between options such as fight and acquiesce but between exit and fight. When the payoff to exit increases, citizens are less likely to pursue conflict, thereby reducing the likelihood of democratization in equilibrium.[13]

The starting point for one of the main contributions in this literature is the contention that there exist periods of time during which citizens have the capacity to overcome collective action costs and credibly threaten the dictator with revolution (Acemoglu & Robinson 2006). At other times, however, this threat is not credible. Further, during periods when the revolutionary threat is binding, the dictator cannot credibly promise to redistribute assets (or income) to quell the threat. The reason these promises are not credible, they argue, is that once the period of a binding revolutionary threat passes, the dictator can easily renege on any promises. Democratization, they suggest, is the only way to make promises of redistribution credible. Without a credible revolutionary threat spurred by sufficiently low collective action costs, democracy will never emerge in their story of democratization. While Acemoglu & Robinson (2006) do not dwell at length on circumscribing the universe of time periods when the revolutionary threat is binding, they do offer a suggestion in this regard: during periods of economic crisis. They posit that "some circumstances are uniquely propitious for solving the

collective-action problem—such as a harvest failure, a business-cycle depression, the end of a war, or some other economic, social or political crisis" (2006, 145). The possibility that economic crisis can be a fleeting moment when poor citizens overcome collective action barriers and credibly threaten revolution has been around since at least Brinton (1938), though this idea has certainly met its share of critics. An implication of this argument suggests that the link between economic crisis and democratization may run through collective action on the part of citizens to demand democracy. If economic crisis is the episodic factor that enables opposition mobilization to which the authoritarian elite can potentially respond by democratizing, then we should think carefully about when economic crisis is likely to spur opposition mobilization rather than simply result in citizen exit. Adding the possibility of citizen exit to these formal theories not only helps us think through when economic crisis may affect the likelihood of democratization, but it also focuses more precisely on the incentives of non-elite citizens in the opposition (and not simply the actions of regime elite) in a game of democratization (Wood 2000).

Various scholars have discussed the three alternatives for different groups of citizens: revolt/conflict; not revolt/acquiesce; and exit. Frieden (1991, 33) argues that the politicization of capitalists depends on their economic exit options and political alternatives.[14] For example, in Latin American countries such as Argentina and Brazil, he argues, import-substituting firms that were dependent on government protection policies and had low asset mobility had few exit options during economic crises in the 1970s and 1980s. The defection of these firms from the authoritarian bargain helped pave the way for transition from military rule to democracy (Haggard & Kaufman 1995). Wood (2000) shows how political mobilization of the poor helped inflict economic pain on the business elite in El Salvador and South Africa, thereby increasing the costs of elite support for the authoritarian regime. Rich elite interests whose incomes were dependent on the labor of the poor defected from the authoritarian bargain when the costs of continued conflict outweighed the costs of democratization. These examples illustrate the choice between voice and loyalty for both the poor (through political mobilization leading to conflict) and the rich (by defecting from the authoritarian bargain).

The large-scale migration of labor from Zimbabwe to neighboring South Africa during Zimbabwe's severe economic crisis, dating from 1999, illustrates the exit option of

wage earners.[15] Some observers estimate up to three million citizens (or nearly a quarter of the pre-crisis population) have fled as the economic return to their labor plummets relative to the wages earned in neighboring South Africa.[16] The loss of discontented citizens may sap the strength of the opposition, decreasing the chances they will be able to directly inflict harm on elites and raise the cost of support for Mugabe among party insiders.[17] The relatively high mobility of labor during Zimbabwe's current crisis stands in stark contrast to the restricted labor mobility of many black South Africans before the transition to democracy in 1994. Restrictive labor movement under the apartheid system may have contributed to the successful political mobilization of African labor—to a point where insurgents were capable of imposing sufficient economic costs on business elites to cause their defection from the authoritarian bargain, forcing democratization (Wood 2000). If this logic is correct, then under apartheid, exit costs were perhaps high enough to make voice and protest the more attractive option.

The main hypothesis I test argues that exit mobility should condition the effect of economic crisis on democratization. Specifically, economic crisis should be more likely to lead to democratization as structural openness that corresponds to higher exit mobility (lower exit costs) decreases. In regimes where citizens have low exit mobility, crisis should be associated with democratization. In regimes with high exit mobility, alternatively, economic crisis should have little effect on democratization.

DATA AND RESEARCH DESIGN

Exit Mobility

In an ideal world, we would be able to directly measure the costs and benefits of migration relative to not migrating in all dictatorships in every year. However, data in dictatorships is often very difficult to obtain and even when it exists, the regime often has an incentive to distort or misrepresent data (Rosendorff & Vreeland 2006). To capture the costs and benefits of exit, I measure variables that serve as plausible proxies for the net costs and benefits of migration using three separate measures: a geographic instrument for bilateral trade; neighboring country wealth; and net migration. These variables measure structural conditions of the economy (geographic determinants of trade); relative

geographic advantages (neighboring country wealth); and past observed behavior (net migration). However, none of these measures directly accounts for immigration policy—in either the home or foreign country.

Some data on immigration policy exists for relatively wealthy democracies, which are mostly the receiving countries of migrants from dictatorships in the post-war era. For example, Neumayer (2005) uses United Nations High Commissioner for Refugees (UNHCR) data on asylum seekers in Western European countries to calculate asylum recognition rates. Neumayer (2006) also uses data from the International Civil Aviation Association's Travel Information Manual (IATA 2004) to calculate bilateral visa restrictions. This data, however, is only for one calendar year (1999). If citizens in dictatorships only migrated to relatively wealthy democracies, this data might be used to infer the (variation in) net costs and benefits of migration that are due to differences in home country migration policy. However, this seems implausible, as much migration and certainly most refugee flows are between countries within the global south (UNHCR 2002). Data on emigration policy in dictatorships is even harder to obtain. Country studies of emigration policy in particular dictatorships during certain periods exist (Felshtinsky 1982, Finkelstein 1988, Fitzgerald 2005, Choate 2007), but to date there is no systematic cross-national data on emigration policy in dictatorships that is comparable across time.

Trade

The relationship between democracy and trade is likely to be endogenous because democracy can affect trade levels and trade openness may affect the level of democracy (Mansfield, Milner & Rosendorff 2000, Milner & Kubota 2005, Kono 2008, Li & Reuveny 2003, López-Córdova & Meissner 2008). One strategy to circumvent this simultaneity issue is to find variables that are correlated with trade but exogenous to democracy. In the literature on trade openness and economic growth, researchers frequently use gravity models of trade in which bilateral trade levels are regressed on geographic and demographic variables such as distance between nations, population, and whether the trading partners are island countries or landlocked (Frankel & Romer 1999). To capture the exogenous effect of trade on democracy, I use a similar identification strategy (López-Córdova & Meissner 2008). In addition to strictly geographic determinants of trade, gravity models of trade often include measures of the size of the

economy and colonial status. These non-geographic variables, however, are unlikely to be strictly exogenous to the process of democratization. For example, modernization theorists argue that economic development leads to democratization (Lipset 1959), and there is some evidence that wealthy countries with GDP per capita over $6000 are unlikely to revert from democracy to dictatorship (Przeworski & Limongi 1997). Further, colonial legacies may have a causal effect on both economic development (Acemoglu, Johnson & Robinson 2001, Banerjee & Iyer 2005) and democracy (Mamdani 1996, Engerman & Sokoloff 2005).

   Instead, I use an augmented gravity model that employs geographic variables as determinants of bilateral trade,[18] where trade is measured as the log of the sum of imports and exports divided by GDP:

$$ln(\frac{Imp+Exp}{GDP}) \; = \; X\lambda$$

$$= \; \lambda_0 + \lambda_1 Landlocked_{ij} + \lambda_2 Island_{ij} + \lambda_3 Border_{ij} + \lambda_4 ln(Area_i {}^* Area_j) +$$

$$\lambda_5 ln(Dist_{ij}) + \lambda_6 ComLang_{ij} + \lambda_7 \vartheta_i + \lambda_8 \vartheta_j + \varepsilon_{ij}$$

Landlocked and *Island* can take the values (0,1,2) depending on how many countries in the trading pair are landlocked or island countries; *Border* is a dummy variable for sharing a common border; and *ComLang* is a dummy variable for whether the two countries (*i* and *j*) share a common language. ϑ_i and ϑ_i are vectors of fixed effects for country *i* and country *j* respectively. The measure of trade used in the democracy equation is the sum of predicted bilateral trade across all trade partners:

$$TradeInstrument = T_i = \sum_i exp[X\hat{\lambda}] \tag{1}$$

This trade instrument captures the geographic determinants of trade unrelated to democracy. Using geography as an instrument for trade levels not only excludes factors such as level of development and colonial status used in many gravity models of trade, it also excludes policy determinants of trade. This is important because we know that one

way democracy can influence trade is through bilateral and multilateral trade agreements (Mansfield & Rosendorff 2000). Observed trade levels will pick up this reverse causation.

TABLE 1

GRAVITY MODEL OF TRADE

Landlocked	-0.989**
	(0.01)
Island	-0.356**
	(0.01)
Border	0.908**
	(0.04)
Common language	0.745**
	(0.01)
Area	0.361**
	(0.00)
Distance	-0.840**
	(0.01)
Constant	10.954**
	(0.07)
R-sq.	0.397
Observations	234,597

[*] $p<0.05$; [**] $p<0.01$. Dependent variable is the natural log of bilateral trade. OLS estimation.

To estimate the gravity model of trade, I use data from Rose's (2004) study of multilateral trade agreements such as the World Trade Organization (WTO) and its predecessor, the General Agreement on Tariffs and Trade (GATT). While this analysis has come under some criticism (Goldstein, Rivers & Tomz 2007), the concerns were leveled at the measurement of participation in multilateral agreements and not trade data itself. The data cover all bilateral trade for which there is available data, from 1948–1999. Table 1 reports the results of the gravity equation. To save space, I do not report the coefficient estimates for region dummies. All the variables are in the expected direction and similar sizes to estimates from previous studies (Anderson 1979, Rose 2004). The one exception is the coefficient for *Island*, which is typically positive.

To check the robustness of this specific gravity equation, I also estimated a second equation which adds the logged product of the populations of each country pair. In

unreported results, I use this second instrument in the democracy equations below, yielding very similar results. However, adding population to the gravity model in Table 1 yields predicted trade values with extreme outliers for China.[19] Following previous research (López-Córdova & Meissner 2008), I primarily use the unlogged predicted value of trade, though I do check the robustness of the main result using the logged value of predicted trade.

Using a gravity model of trade not only addresses concerns about reverse causality between democracy and trade, it also circumvents the possibility that economic growth causes trade outcomes. If trade, as a proxy for exit costs, is in part determined by economic crisis, then it is unclear what an interaction between trade and crisis would mean. To check whether observed trade levels in the current period are determined by lagged growth, I regressed trade as a share of GDP on the lagged two-year moving average of economic growth, with country fixed effects. The coefficient for *Growth* was positive and statistically significant (14.9, SE= 6.7), suggesting that lagged growth may affect trade levels. I repeated this exercise by regressing the trade instrument on lagged growth, again with country fixed effects. In this regression, the coefficient for *Growth* was small (less then one) and not statistically different from zero, suggesting that there is no relationship between the trade instrument and lagged economic growth. This finding is consistent with one application of gravity models of trade in the trade-growth literature (Frankel & Romer 1999).

Neighbor GDP

While trade is the primary measure of exit mobility used in the subsequent analysis, I also employ two other variables which may capture other facets of exit mobility. The theory suggests that exit should be less costly for labor when the potential wages earned in neighboring countries are higher. The case of Zimbabwe during the current economic crisis perhaps illustrates this best. Neighboring South Africa has much higher average wages than found in Zimbabwe: from 1998 to 2006, South Africa's GDP per capita rose from $2975 to $3562, while Zimbabwe's fell from $675 to $409.[20] As the economic crisis deepens in Zimbabwe, labor exits in search of higher wages. A recent survey of Zimbabwean citizens who had migrated to South Africa found that 92% of the respondents had migrated since the beginning of the economic crisis in 1999 (Makina

2007). The survey also found that 58% of these migrants had left for political reasons—a larger share than those who cited economic hardship (51%). This suggests that the majority of these migrants were opponents of the ZANU-PF. Thus, the loss of labor very well may have reduced the ability of the opposition to mobilize (or unify) against the ZANU-PF regime in Zimbabwe.

This logic suggests that migration may weaken political participation and prospects for opposition mobilization in sending countries. Recent evidence from Mexico is consistent with this pattern. Bravo (2007) finds that when migrants leave, this transforms the sending country's local politics by decreasing the political participation of those left behind—possibly because those left behind receive remittances from migrants and have less at stake in the productive economy of the sending country. The survey evidence from Zimbabwean migrants in South Africa indicates that 72% have three or more dependents living in Zimbabwe, while only 12% have three or more dependents living in South Africa. With mass migration from Zimbabwe to South Africa, it is possible that not only did some of the citizens most likely to protest against the regime leave, but those they left behind may in fact be less likely to mobilize against the regime precisely because they have close relatives in South Africa. The counterfactual scenario would be Zimbabwe facing the same economic crisis, but being located geographically next to Chad or the Democratic Republic of the Congo rather than South Africa. In this scenario, without the attractive exit option in the form of higher South African wages, labor would be more likely to mobilize against the regime and successfully press for democratization.

Distance matters for migration, in part due to higher transportation costs for longer distances or for reasons of psychological alienation (Lucas 2001). For example, Lucas notes that "the dominant sources of migration to the EU are the remaining portions of Europe, the Maghreb, and Turkey, all of which are neighboring regions. For the US the largest source of migrants is Mexico. The wealthier countries of East Asia have turned to other countries within the region to supply their unskilled labor needs" (2005, 47). This same point can be illustrated at the country-level as well. While South Africa is the largest regional magnet for migrants in sub-Saharan Africa, it is not the only relatively wealthy country in the region to attract migrants from poorer neighbors. Adepoju notes that "[a]

small and rich country, Gabon relies on contract labour and immigrants to supplement the domestic labour force. Most immigrants are from Mali, Equatorial Guinea, Nigeria, Senegal, Benin, Cameroon, and Togo" (2000, 391). With the exception of Senegal, which has a coastal capital city, all these countries are within 2000 kilometers of the capital and largest city in Gabon, Libreville, where nearly 45% of the country's population resides. With the exception of Equatorial Guinea, which has recently grown rapidly with the discovery of off-shore oil deposits, all the countries Adepoju notes with migrants in Gabon had a GDP per capita figures of less $1300 in the late 1990s. Gabon's GDP per capita in the late 1990s, by comparison, was nearly $5000.[21]

To capture the logic of higher exit *benefits*, I calculate the mean GDP per capita of neighboring countries (*NeighborGDPpc*), where neighbors are defined as all countries with capital cities within 2000 kilometers of the target country's capital city.[22] Previous research has used relative GDP per capita to measure relative wages (Hamilton & Whalley 1984, Moses & Letnes 2004), while others argue that immigration was the primary cause of income convergence in Europe in the pre–World War I era—suggesting that migration and per capita income differentials work in tandem (Taylor & Williamson 1997).[23] To ensure that the distance of 2000 kilometers is not simply an arbitrary cutoff, I also calculate the mean GDP per capita of neighboring countries within 3000 kilometers.

TABLE 2

NEIGHBOR GDP PER CAPITA
(Nondemocracies only)

Region	Neighbor GDP per capita	
	2000km	3000km
Latin America	$3384	$3915
Central/East Europe	$6810	$6892
West Europe	$6023	$5679
North Africa/Middle East	$6463	$5792
Sub-Saharan Africa	$1336	$1694
Central Asia	$3057	$3744
South/East Asia	$2840	$2830

Table 2 reports the mean level of GDP per capita across geographic regions for the sample used in the analysis below. The reported values are the mean for all

nondemocratic regimes for all their respective neighbors (democratic and nondemocratic) within the specified distance. While most of the mean values conform to received wisdom, two points may need some clarification. First, the means for Western Europe are those for Portugal and Spain (in the 1950s, 1960s, and 1970s), which are more proximate to relatively poor countries in North Africa than most of the sample from Eastern Europe. This helps explains why the mean neighbor-GDP per capita is lower for Western Europe than for Central and East Europe. Second, Middle Eastern and North African dictatorships have relatively rich neighbors due to their proximity to Western Europe and oil-rich monarchies in the Persian Gulf region. While Islam and oil dependence have been offered as explanations for the relative dearth of democracy in many Middle Eastern (and North African) countries, having relatively rich neighbors is another feature of authoritarian regimes in this region. As we will see below, this is another possible explanation for the persistence of nondemocratic regimes in this region: labor exit to (relatively) wealthy neighbors may depress the demand for democracy in the face of otherwise destabilizing economic crises.

Net Migration

The third measure of exit mobility is net migration, using data from the United Nations (UN). (See Figure 1.) Net migration measures the number of people who immigrated to the target country minus the number of people who emigrated from the target country over a five-year period. In the raw data, a negative value for net migration indicates that the number of citizens who left the country exceeded the number of migrants entering the country. For example, from 2000–05, Mexico's raw net migration was -3,982,600, while raw net migration to the United States was +6,493,000.

FIGURE 1

NET MIGRATION BY REGION
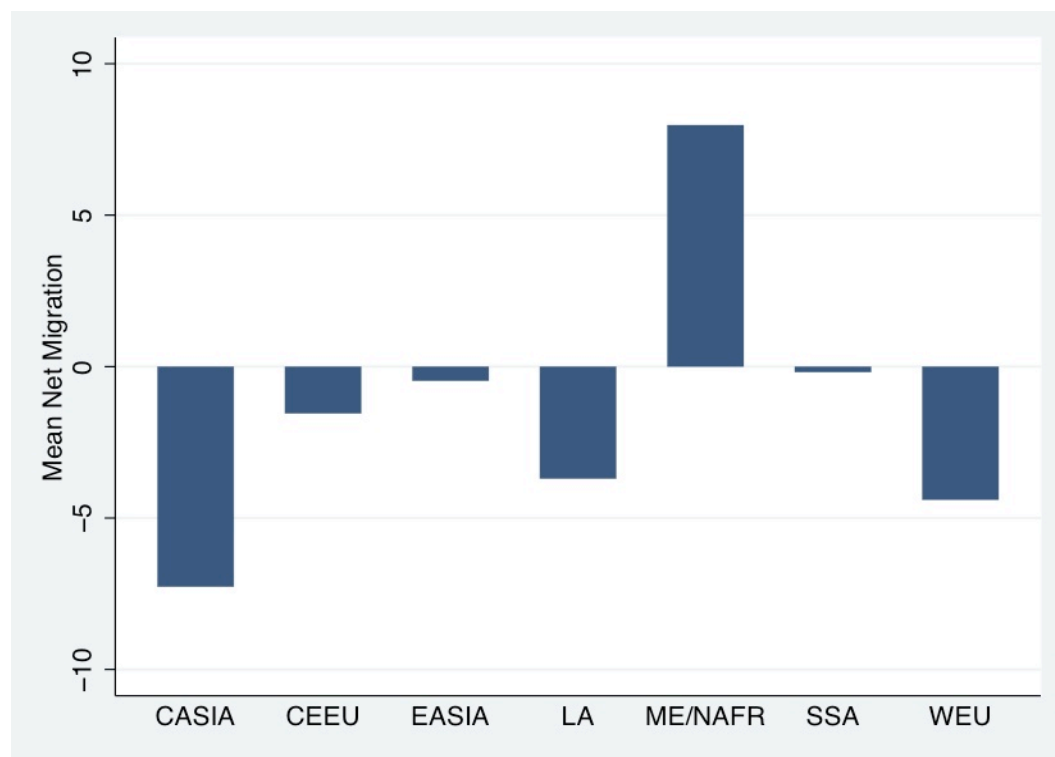
CASIA: Central Asia; CEEU: Central and Eastern Europe; EASIA: South and East Asia; LA: Latin America; ME/NAFR: Middle East and North Africa; SSA: Sub-Saharan Africa; WEU: West Europe.

I use migration data rather than refugee data for two reasons. First, the UN measure of net migration has more coverage for more years and countries than refugee data. Further, this measure attempts to capture all migration, not just the migration of persons classified as refugees. The refugee (and internally displaced persons) data from UNHCR that has been used in previous research only captures one specific type of migration (Davenport, Moore & Poe 2003, Moore & Shellman 2004). Similarly, data on asylum seekers looks at only one type of migrant and is based on paper applications instead of actual flows of people. The net migration data, alternatively, is based on calculations of total population, both citizens and noncitizens, regardless of refugee status.

A brief examination of data from Zimbabwe point to some of the differences between refugee data and net migration data. The UNHCR data on refugee flows from Zimbabwe to South Africa are missing until 2001 and then vary between 4 and nearly 500 between 2002 and 2008. The number of refugees migrating to Canada between 2002 and 2008 varies between 100 and nearly 3500 per year. Over the seven years between 2002 and 2008, refugees to Canada totaled over 14,000 and those to South Africa just under 1400—an order-of-magnitude difference. This snapshot suggests that refugee data is more likely to pick up migration to rich countries (e.g., Canada) than relatively poor ones (e.g., South Africa). Some observers estimate that by 2005, over one million Zimbaweans lived in South Africa—out of a total population of 13 million (Makina 2007). More recent news reports put this figure even higher—from one to three million.[24] The refugee data do not reflect this mass movement of people. In fact, the refugee data claim nearly 200,000 total refugees fleeing Zimbabwe in 1979, a year of white-African flight to neighboring countries in the wake of the Lancaster agreement that formalized an end to white minority rule in Rhodesia. During the period of land seizures and severe economic crisis from 1999–2008, however, the number of refugees fleeing Zimbabwe totaled less than 50,000. Thus the refugee data would suggest that exit in 1979 was nearly three times more than the entire decade beginning in 1999. Net migration data, however, report that Zimbabwe lost about 100,000 from 1975 to 1980 and over 225,000 migrants from 1995 to 2005. The migration data therefore indicate that more migrants fled Zimbabwe during the crisis under Mugabe than during the period of violent conflict and the subsequent white flight (1975–80) in the last years of Ian Smith's regime. This picture suggests that the migration data more accurately captures the mass movements of people from Zimbabwe than the refugee data.

Table 3 reports the mean level of net migration by region. The reported means are for nondemocracies from 1960–2000. In almost all regions except the Middle East, net migration is negative or near zero. This suggests that in nondemocracies, on average, more migrants leave than enter. The one exception is Middle Eastern dictatorships, which have many more people entering than exiting. This large figure is driven almost entirely by oil-rich Gulf states with small populations such as Saudi Arabia, Kuwait, and

especially the United Arab Emirates. Countries in the Gulf also have the largest variation in net migration.

To capture the possibility that past migration may condition the effect of crisis on democratization, I calculate the average net migration for each preceding 10-year period. I then standardize this measure by dividing it by population and multiplying by 100. I take the natural log of this measure to reduce to the influence of outliers. Finally, I multiply this figure by -1 so that positive values mean more people emigrated out of the country than into the country over the past decade. This means the migration variable can be interpreted in a similar manner as neighbor GDP or trade levels: higher values represent more openness or lower net exit costs.

Economic Crisis

The other key explanatory variable is economic crisis. To provide the widest possible coverage of this variable for authoritarian regimes, I use Maddison's (2004) data on GDP per capita. I construct the crisis variable in the following manner. First, I take the two-year lagged moving average of economic growth. The lag helps alleviate concerns that political change is driving the measure of crisis (reverse causation); the two-year moving average helps smooth the data and ensures that the crisis variable is not simply picking up regression to the mean dynamics (Gasiorowski 1995). I then multiply this lagged moving average by negative one and recode all observations of positive growth as zero. Truncating the distribution of *Crisis* at zero isolates the effect of negative growth—or economic crises—allowing all observations of positive growth to take the same value (zero). This ensures that the results are not driven by episodes of strong economic growth, which are likely to increase the longevity of authoritarian rule and thus decrease the likelihood of democratization. Finally, because the *Crisis* variable is (negative) growth multiplied by negative one, the expected direction of the estimated coefficient is positive: crisis (and more severe crisis) increases the likelihood of democratization.

To check the robustness of this measure, I also include one specification using growth data from the World Development Indicators instead of the Maddison data. This substitution decreases the sample size substantially, but ensures that the results are not simply due to one particular source for growth data. In addition, I create another variable

(*Boom*) in the same manner as *Crisis*, but measuring only positive growth instead of negative growth. I include this variable, interacted with *Trade*, both with and without *Crisis* and *Crisis*Trade*, to explore whether robust economic growth deters democratization, and if so, whether it is conditioned by exit costs.

Finally, I construct a variable (*CrisisLength*) that measures the length of economic crisis from the Maddison data: a count of how many previous, consecutive years the economy has contracted. In contrast to the *Crisis* variable, *CrisisLength* does not capture the depth of economic woes but only the duration of the crisis. Again, I report specifications that include this variable (interacted with *Trade*) both with and without *Crisis* and *Crisis*Trade*.[25] The distribution of crisis length years falls considerably after three years: 17% of observations have a 1-year crisis; 8% a two-year crisis; 4% a three-year crisis; 2% a four-year; 2% a five-year; and 2% a crisis of more than 5 years. The longest economic crisis in the data is Congo-Kinshasa from 1987–2002.

Democratization

The primary measure of the dependent variable, democratization, is derived from the Polity score—a scale that runs from -10 (most autocratic) to 10 (most democratic). This measure of democracy is more institutional than procedural, and has been used in numerous studies of democracy—both as a continuous variable (Ross 2001, Li & Reuveny 2003) and to create a binary indicator of democratization (Ward & Gleditsch 1998, Wright 2009). I code a binary variable which measures a four-point or more increase in the Polity score from the previous year for all four-point or more increases that result in a Polity score of zero or more in the observed year (Polity scaled -10 to 10). The four-point threshold for democratic change has been used in previous research and the results are equally robust to using a three-point threshold.[26] Restricting the concept of democratization to changes that result in an observed score greater than zero excludes cases of improvement in the level of "democraticness" that may not be considered full transitions to democracy. For example, in 1992 President Mobutu (Congo-Kinshasa) legalized parties and held a constituent assembly to help organize multiparty elections. This increase in "democraticness" is captured by an increase in the Polity score from -9 to 0. However, Mobutu's partial liberalization did not necessarily entail a full transition to

democracy. Mobutu did not hold free and fair multiparty elections (as promised) and remained in power another five years. Even then, the first multiparty elections were not held until 2005. Similarly, Polity measures the transfer of power that occurred during the 1979 Iranian Revolution when Mohammad Reza Shah Pahlavi was deposed by Ayatollah Khomenei as an increase in the Polity score from -10 to 0. The binary measure of democracy that I use excludes cases like these. Again, the main results do not depend on the choice of measuring democratization as increases in the Polity score that result in an observed Polity score greater than zero.

I check the robustness of the results using a second dichotomous measure of democratization.[27] This dichotomous variable captures one-off discrete transitions from dictatorship to democracy, similar to the measure used in the transitions literature (Przeworski et al. 2000, Boix & Stokes 2003, Epstein et al. 2006). Unlike the binary variable derived from the Polity scale, this discrete measure captures one observation of democratization during each democratization episode. To understand the difference between the two ways I measure democratization, consider the case of Hungary. The binary measure of democratization derived from the Polity scale scores both 1989 and 1990 as years of democratic transition because the Polity scale increased from -2 to +4 in 1989 and again to +10 in 1990. The discrete measure of democratization only codes 1990 as democratization. Thus the main measure I use throughout the analysis (derived from the Polity score) captures democratization episodes that occur over more than one year.

Empirical Model

The logic of using a geographic instrument for trade to measure the exogenous cost of exit suggests that an equation testing the effect of economic crisis and trade openness on democracy should exclude any potentially endogenous variables. In this spirit, I first test the model using only the main explanatory variables of interest, population, and the lagged level of the Polity score to control for ceiling effects. Excluding the control for ceiling effects generally makes the results stronger.

$Democratization = \beta_0 + \beta_1 Crisis + \beta_2 Crisis*Trade + \beta_3 Trade + \beta_4 Ln(pop) + \beta_5 Polity_{t-1} + \varepsilon$

I use a logit model with robust standard errors clustered on country. To explore the possibility that unit effects might bias the result, I also tested models with random and country-fixed effects. The results did not change (unreported).[28] Using country-fixed effects reduces the sample size because some countries do not experience variation in the dependent variable (i.e., no democratization) and are thus excluded.

Control Variables

Despite the nod towards a model with no potentially endogenous explanatory variables, in many of the specifications below I control for additional variables, sidestepping the question of whether all of these additional variables are strictly exogenous. In the base specification, I control for: *Log*(*GDP*), *NeighborPolity*, *Conflict*, and the *Post–Cold War* period. I control for level of development (*Log*(*GDP*)) because many scholars discuss the possibility that richer countries may be more likely to democratize than poorer ones (Lipset 1959, Przeworski et al. 2000, Boix & Stokes 2003), and richer countries also trade more. *NeighborPolity* controls for the possibility that democratic diffusion, which may be correlated with the geographic instrument for trade, may affect democratization. To construct this variable, I calculate the mean Polity score of all countries with capital cities within 2000 kilometers of the target country's capital.[29] *Conflict* is binary measure of civil conflict with more than 1,000 annual deaths, from (Gleditsch & Strand 2002). Modeling time (*Post-Cold War*) is important because international pressure to democratize shifts throughout the time period under consideration—particularly after the Cold War. In some specifications I include more comprehensive measures of calendar-time: decade dummies, year dummies, and calendar year time trends (*Time*, *Time*^{2}, and *Time*^{3}).

I also report models that include additional controls for authoritarian regime type. Geddes (1999) argues that military regimes are much more likely to democratize than other regime types, particularly single-party regimes, and some regime types are more likely to have open economies than others, making this an important control.[30] In these specifications, I also include controls for duration dependence, measuring how long the regime has been in power (*Lifetime*) and its square (*Lifetime*^{2}).[31] Including controls for duration dependence in other specifications does change the results and F-tests suggest they should only be included in specifications where regime type is used as a control

variable. Finally, in some specifications I also include region dummies, though these are highly correlated with *NeighborPolity*.

RESULTS

Table 3 reports the results of the model in equation (3). The main variable of interest is the interaction between *Crisis* and *Trade*. In model 1, the coefficient for this interaction is negative and statistically significant at conventional levels while the coefficient for *Crisis* is positive, suggesting that as openness increases, the positive effect of *Crisis* on the likelihood of democratization decreases.[32] As a shortcut to interpreting the interaction coefficient, in the bottom panel I report the linear combination of

$\beta_{Crisis} + X_{Trade} * \beta_{Crisis*Trade}$, where X_{Trade} takes the value of the 75th percentile of the with-in sample distribution (high openness) and the 25th percentile of the distribution (low openness). Examples of countries with relatively high openness are Algeria and Morocco; low openness countries are the Central African Republic, Guinea-Bissau, and Paraguay. The result in model 1 indicates that the effect of economic crisis on democratization in relatively closed regimes (25th percentile) is positive and statistically different from zero, while the effect of economic crisis in open regimes (75th percentile) is negative. This suggests that economic crisis only increases the likelihood of democratization in relatively closed economies.

TABLE 3

CRISIS, TRADE, AND DEMOCRATIZATION

	(1)	(2)	(3)
Crisis	11.570**	13.203**	68.610**
	(3.71)	(3.56)	(23.09)
Crisis*Trade	-1.332**	-1.849**	-4.340**
	(0.57)	(0.67)	(1.56)
Trade	0.027**	0.022**	0.238*
	(0.01)	(0.01)	(0.13)
Log(population)	0.196*	0.183*	0.167
	(0.11)	(0.10)	(0.11)
L.POLITY	0.104**	0.095**	0.107**
	(0.03)	(0.03)	(0.03)
Constant	-6.432**	-6.280**	-9.523**
	(0.95)	(0.94)	(1.78)
Log likelihood	-288.7	-251.5	-290.8
Observations	2799	2628	2799
Sample	Full	Exclude Central/ East Europe	Full
Trade instrument			log
Crisis @ 75th percentile	-1.7	-5.3	-1.3
of Trade instrument	(4.5)	(5.9)	(3.7)
Crisis @ 25th percentile	10.1***	11.2***	8.3***
of Trade instrument	(3.4)	(3.3)	(3.2)

* p<0.05; ** p<0.01. Dependent variable is a 4-point or more increase in the *Polity* score. Logit with standard errors clustered on country.

The second model in Table 3 excludes Central and Eastern European regimes from the sample. These regimes (e.g., Hungary and Poland under Communist rule) tend to have relatively high scores for openness (greater than the 75th percentile) because of their size and location in Europe. However, these regimes were relatively "closed" in the sense that exit was extremely difficult in practice because of the repressive nature of the regime and the active policy of forcibly restricting migration. Thus, if there is one region of the world where a geographic instrument for trade is unlikely to be a useful proxy for exit mobility, it is here. If a trade instrument is indeed a poor proxy for exit in these regimes, the results should improve if we exclude them from the empirical analysis. This is the case: the result of model 2 shows that once we exclude these regimes, the effect of economic crisis in

relatively closed countries is even stronger while crisis in open regimes proves even more stabilizing.

The distribution of the trade instrument is left-skewed, meaning there are many more regimes with low scores of openness than high scores. So to ensure that the main result is not due to some extreme outlier for the trade instrument, model 3 uses the natural log of the trade instrument. The main result holds, but is slightly weaker, as shown in the coefficient for *Crisis* at the 25th percentile of logged *Trade*.

FIGURE 2

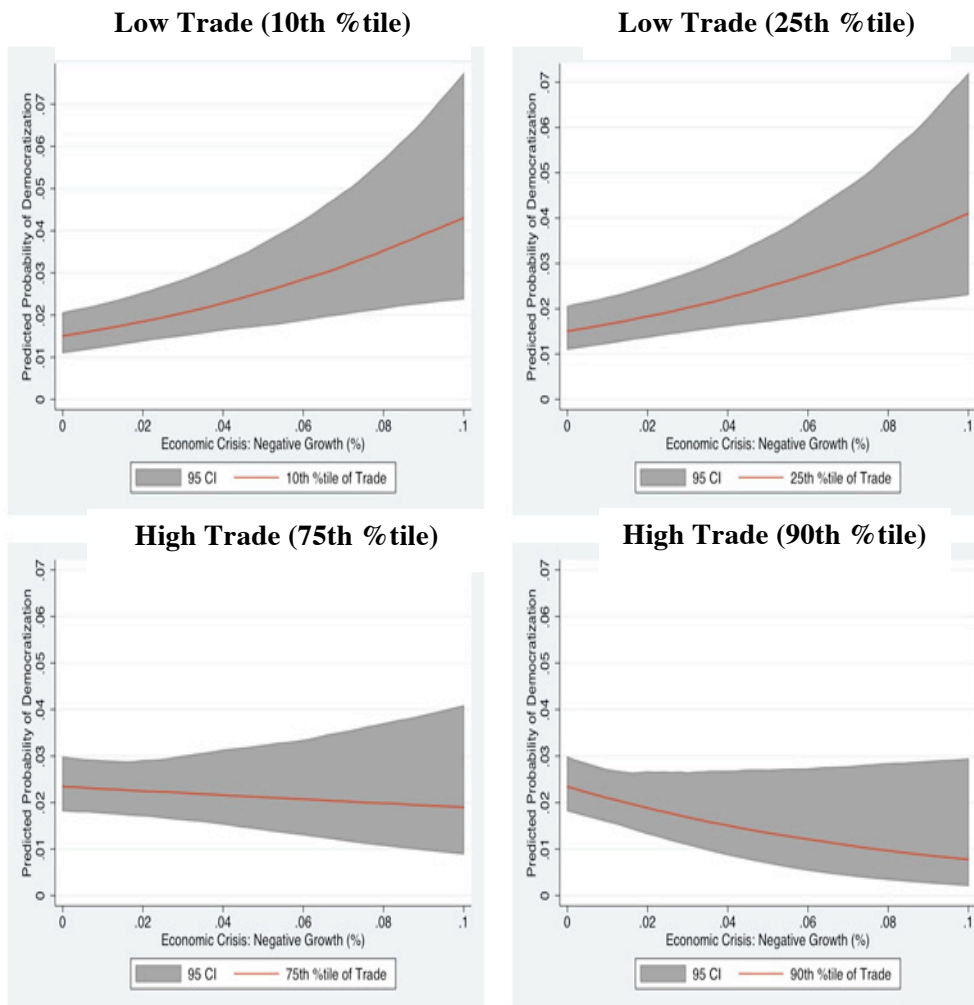**ECONOMIC CRISIS AND DEMOCRATIZATION, BY TRADE LEVEL**

Figure 2 illustrates the substantive significance of the main finding from model 1 in Table 3. The vertical axis plots the predicted probability of democratization based on simulations of the effect of economic crisis at various trade (instrument) levels.[33] The horizontal axis plots the depth of economic crisis: 0 when there is no crisis and up to a 10% contraction in GDP (averaged over the previous two years). In the upper left corner, for example, the graph depicts the relationship between crisis and democratization in closed economies (10th percentile of the trade distribution). The predicted probability of democratization when there is no economic crisis (0 along the horizontal axis) is 1.5%, rising to 2.5% when growth decreases by 5%, and again to over 9% when crisis reaches 10% negative growth. Thus, an extremely severe economic crisis can more than quadruple the predicted probability of democratization at low trade levels. Even at the 25th percentile of the trade distribution, as depicted in the upper right panel, we see a strong positive correlation between crisis and the likelihood of democratization. At high trade levels, however, this relationship disappears. At the 75th percentile of trade the slope of the line depicting the relationship between crisis and democratization is negative, but not statistically different from zero. A similar trend emerges at very high trade levels (90th percentile). These simulations suggest that economic crisis, even at 5% negative growth, can significantly increase the likelihood of democratization—but only at low trade levels.

TABLE 4

CRISIS, TRADE, AND DEMOCRATIZATION
(Control Variables)

	(1)	(2)	(3)	(4)	(5)
Crisis	11.580**	7.699	10.447**	11.138**	10.628**
	(4.26)	(4.81)	(4.03)	(4.14)	(3.88)
Crisis*Trade	-1.448**	-0.973	-1.450**	-1.415**	-1.323*
	(0.66)	(0.68)	(0.66)	(0.65)	(0.77)
Trade	0.015	0.015	0.015	0.015	-0.002
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Log(population)	0.183	0.068	0.271	0.211	0.274
	(0.25)	(0.26)	(0.25)	(0.25)	(0.23)
Log(GDP)	0.004	0.063	-0.094	-0.029	-0.104
	(0.23)	(0.23)	(0.24)	(0.24)	(0.24)
Neighbor democracy	1.163**	1.083**	1.397**	1.242**	0.157
	(0.43)	(0.45)	(0.43)	(0.43)	(0.60)
Conflict	-1.091	-1.424	-1.234*	-1.146	-0.915
	(0.77)	(0.98)	(0.74)	(0.77)	(0.75)
Post Cold War	0.722**	1.001**		1.381*	1.576**
	(0.36)	(0.41)		(0.81)	(0.44)
Monarchy		-2.177*			
		(1.14)			
Military		1.481**			
		(0.50)			
Single party		-1.452**			
		(0.52)			
Lifetime		0.060*			
		(0.03)			
Lifetime2		-0.001			
		(0.00)			
Polity\s\do5(t-1)	0.082**	0.082**	0.091**	0.082**	0.056*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	-6.191**	-6.872**	-5.314**	-6.545**	-3.597
	(2.12)	(2.03)	(2.42)	(2.14)	(2.47)
Log likelihood	-276.1	-249.5	-274.7	-275.1	-260.6
Observations	2799	2799	2799	2799	2799
Controls not reported			Time polynomials	Decade dummies	Region dummies
Crisis @ 75th percentile of Trade instrument	-2.9	-2.0	-4.0	-3.0	-2.6
	(5.2)	(5.8)	(5.4)	(5.2)	(6.5)
Crisis @ 25th percentile of Trade instrument	10.0***	6.4	8.9**	9.6**	9.2***
	(3.9)	(4.4)	(3.7)	(3.8)	(3.5)

*p<0.05; **p<0.01. Dependent variable is a 4-point or more increase in the *Polity* score. Logit with standard errors clustered on country.

Table 4 reports the results from models that include various control variables. Model 1 controls for *Log(GDP)*, *NeighborPolity*, *Conflict*, and the *Post–Cold War* period. Consistent with much earlier research, having more democratic neighbors and the post–Cold War period increase the likelihood of democratization. With these controls, the main result remains. In the second model, I add controls for authoritarian regime type and duration dependence. The results for the regime type variables are highly significant and in the expected direction: military regimes are more likely to democratize than personalist regimes (the omitted category), while monarchies and dominant-party regimes are less likely. Adding these controls puts the theory to a very difficult test because military regimes are the most susceptible to economic crises (Geddes 1999), whereas single-party regimes tend to be more resilient—even in the face of economic decline (Haggard & Kaufman 1995). The estimated coefficient on the interaction between *Crisis* and the *Trade* is smaller (in absolute size) and no longer statistically significant. However, the same general pattern remains: crisis has a destabilizing effect in closed economies but a stabilizing effect in open regimes. The coefficient for the linear combination of $\beta_{Crisis} + X_{Trade} * \beta_{Crisis*Trade}$ at the 10th percentile (unreported) is still large, positive, and statistically significant. In the final three models of Table 4, I add controls for calendar time in the form of time polynomials and decade dummies, and dummies for regions. Adding these controls does not alter the main finding.

In unreported results, I test the specification in model of Table 3 on split samples. First, I split the sample at the median value for *Trade* to ensure that the main results is not simply due to high collinearity between the main interaction term and the constituent variables. The previous pattern remains: the effect of economic crisis on democratization is large and statistically different from zero in relatively closed economies, but negative in open economies. I then split the sample into the Cold War and post–Cold War periods. The main results hold in both periods even though the sample size in the post–Cold War period is considerably smaller. I also exclude all observations from the 1980s and the main pattern still holds. That the results hold in different time periods is an important finding because it ensures that the pattern is not simply due to aid-induced multiparty elections in the early 1990s in Africa or to the economic crises associated with the "lost decade" of the 1980s.

TABLE 5

CRISIS, TRADE, AND DEMOCRATIZATION
(Transition to Democracy)

	(1)	(2)	(3)
Crisis	15.305**	17.153**	14.590**
	(4.27)	(5.27)	(6.34)
Crisis*Trade	-1.194**	-1.621**	-1.009
	(0.55)	(0.76)	(0.86)
Trade	0.023*	0.007	0.012
	(0.01)	(0.02)	(0.02)
Log(population)	0.184	-0.234	-0.430*
	(0.11)	(0.21)	(0.23)
Log(GDPpc)		0.492**	0.496**
		(0.24)	(0.23)
Neighbor democracy		0.953**	0.550
		(0.44)	(0.50)
Conflict		0.011	0.033
		(0.54)	(0.63)
Post-Cold War		0.953**	1.307**
		(0.30)	(0.33)
Monarchy			-2.953**
			(1.25)
Military			1.828**
			(0.57)
Single party			-1.494**
			(0.47)
Lifetime	0.023	0.020	0.120**
	(0.03)	(0.03)	(0.04)
Lifetime-sq.	-0.000	-0.001	-0.001**
	(0.00)	(0.00)	(0.00)
Constant	-6.245**	-10.486**	-10.403**
	(1.06)	(2.51)	(2.13)
Log likelihood	-265.5	-251.6	-223.1
Observations	2879	2879	2879
Crisis @ 75th percentile	3.4	1.0	4.6
of Trade instrument	(4.1)	(5.1)	(6.0)
Crisis @ 25th percentile	14.0***	15.4***	13.5**
of Trade instrument	(3.9)	(4.7)	(5.7)

*p<0.05; **p<0.01. Dependent variable is a one-off full transition to democracy. Logit with standard errors clustered on country.

TABLE 6

CRISIS, TRADE, AND DEMOCRATIZATION
(Alternative measures of *Crisis*)

Growth Source	WDI	Maddison	Maddison	Maddison	Maddison
	(1)	(2)	(3)	(4)	(5)
Crisis	13.276**		9.180**		8.707**
	(4.43)		(4.29)		(4.04)
Crisis*Trade	-1.724**		-1.811**		-1.218**
	(0.65)		(0.74)		(0.51)
Boom		-18.4**	-16.9**		
		(7.90)	(8.07)		
Boom*Trade		0.268	0.052		
		(0.26)	(0.26)		
Crisis length				0.174**	0.116
				(0.08)	(0.09)
Crisis length*Trade				-0.017	-0.008
				(0.01)	(0.01)
Trade	0.024*	0.006	0.016	0.017	0.016
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Log(population)	0.184	0.190	0.099	0.183	0.142
	(0.25)	(0.24)	(0.25)	(0.25)	(0.26)
Log(GDP)	-0.028	0.018	0.105	-0.000	0.046
	(0.24)	(0.23)	(0.24)	(0.24)	(0.25)
Neighbor democracy	1.224**	1.154**	1.130**	1.132**	1.134**
	(0.43)	(0.42)	(0.43)	(0.42)	(0.42)
Conflict	-1.095	-1.238	-1.198	-1.099	-1.096
	(0.80)	(0.78)	(0.77)	(0.77)	(0.77)
Post-Cold War	0.600*	0.652*	0.699*	0.705*	0.696*
	(0.36)	(0.35)	(0.36)	(0.37)	(0.37)
Polity(t-1)	0.084**	0.087**	0.083**	0.083**	0.082**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	-5.595**	-6.039**	-6.724**	-6.149**	-6.565**
	(2.23)	(2.10)	(2.18)	(2.20)	(2.24)
Log likelihood	-236.423	-274.767	-272.159	-276.515	-275.429
Observations	2188	2799	2799	2799	2799
Crisis Length @ 75th percentile				0.01	0.03
of Trade instrument				(0.07)	(0.07)
Crisis Length @ 25th percentile				0.16**	0.11
of Trade instrument				(0.07)	(0.08)

*p<0.05; ** p<0.01. Dependent variable is a 4-point or more increase in the *Polity* score. Logit with standard errors clustered on country.

In Table 5 I employ an alternative measure of democratization: a binary coding of full transition from dictatorship to democracy, similar to that used in previous literature

on democratic transitions (Boix 2003, Epstein et al. 2006). This data can be directly estimated as a duration model with controls for time dependence (Beck & Tuck 1998, Carter & Signorino 2008), so all three models include controls for duration dependence (*Lifetime*, *Lifetime*^{2}). The first model includes only population as a control variable; the second controls for *Log*(*GDP*), *NeighborPolity*, *Conflict*, and the *Post–Cold War* period; and the third adds controls for authoritarian regime type. In all three models, the main result is considerably stronger than when using the dependent variable derived from Polity scores. For example, the effect of crisis on democratization in the first model is more than four times as strong in closed as open regimes. In the second, this effect over 15 times as strong. Controlling for regime type in column 3 again weakens the result, but the main pattern still remains: the effect of crisis is three times as strong in closed as in open countries.

Finally, Table 6 examines alternative measures of economic crisis.[34] In the first column, I use a measure of *Crisis* derived from the World Development Indicators. The results are similar to those using the Maddison (2004) data. The second and third columns include *Boom*—which is the converse of *Crisis*—and its interaction with *Trade*. The last two models include *Crisis Length* and the interaction term. First, note that in all three models with *Crisis*, the main result holds. That is, even controlling for periods of rapid economic growth and the length of an economic crisis, the effect of the depth of crisis itself is still strongly conditional on the predicted level of trade. That the main finding still holds once we control for *Crisis Length* rules out the possibility that open economies simply recover faster from economic crisis, thereby diminishing the liberalizing effect of crisis.[35] Second, the effect of *Boom* is strongly negative, suggesting that periods of rapid economic growth reduce the likelihood of democratization. However, this effect does not appear to be conditional on trade, even controlling for economic crisis. Both at high and low trade levels, economic booms deter democratization. The results in the last two columns suggest that longer crises increase the likelihood of democratization, but this effect diminishes as trade increases. The coefficient for the interaction between *CrisisLength* and *Trade* is negative in both models, though smaller when *Crisis* is included as a control. Simulations of model 4 indicate that at high trade levels (75th percentile), the likelihood of democratization is 1.5% when there is no crisis, which stays

the same after one year of crisis, and increases to 1.8% after 5 years of crisis. At low trade levels (25th percentile), the predicted likelihood of democratization increases from 1.3% to 1.6% after one year of crisis, again to 3.0% after 5 years of crisis. Thus at high levels of trade an enduring economic crisis barely affects the likelihood of democratization, but in low trade regimes an enduring crisis more than doubles the likelihood. These simulations suggest that not only is the effect of the depth of economic crisis conditional on trade levels, but the impact of the length of economic crisis on democratization is also conditional on the "natural openness" of the regime.

CRISIS, NEIGHBOR GDP, AND DEMOCRACY

A second way to measure exit costs is to look at the average *wage* levels of neighboring countries. One proxy for average wage levels in countries neighboring dictatorships is to calculate the mean GDP per capita, with higher mean GDP per capita implying a higher average wage. Therefore, if the exit argument is correct and applies to labor, then we should expect that economic crisis is less likely to lead to democratization as the neighboring countries' GDP per capita rises. In this respect, having wealthier neighbors is akin to higher trade levels: more of each should diminish the liberalizing effect of economic crisis.

Because I calculated *Neighbor GDPpc* using the Maddison (2004) data, the coverage for this variable extends from 1946 to 2002, yielding a substantially larger sample than used previously in the trade models. Models 1 and 2 in Table 7, for example, cover over 120 regimes in 114 countries. In the third model, I add controls for *Islam*, *Monarchy*, and *Oil Rents*. *Islam* is the share of the population that is Islamic and is time invariant. The oil variable is measured as the logged per capita value of oil rents, lagged one year. Oil rents are calculated by multiplying annual oil production by the world oil price.[36] The latter three models use a strictly dichotomous measure of democracy, with slightly more coverage. In five of the six models, the interaction between *Neighbor GDPpc* and *Crisis* is negative and statistically different from zero, suggesting that economic crisis has less of a destabilizing effect in regimes with rich neighbors. Even in model 5, where the interaction coefficient is not statistically significant, the substantive

results indicate that crisis in a regime with poor neighbors (25th percentile of neighbor GDP per capita) is twice as destabilizing as crisis in regimes with relatively rich neighbors (75th percentile). In unreported results, I also test whether including region dummies as control variable changes the results. I find that main results hold, though the coefficient for neighboring country democracy is considerably smaller and no longer statistically significant. This suggests that region dummies may be picking up much of the variation in neighboring country democracy, but the same is not necessarily true for neighboring country GDP per capita.

Figure 3 depicts the marginal effect of economic crisis on the likelihood of democratization across a range of values for *Neighbor GDPpc*. The predicted values are taken from simulations of model 1 in Table 7. The top panel shows the marginal effect of an economic crisis of -5%. The vertical axis on the left displays the marginal effect while the horizontal axis is the wealth of neighboring countries. With relatively poor neighbors (GDP per capita less than $2000), crisis increases the likelihood of democratization between 1% and 2%. With rich neighbors (GDP per capita greater than $6000), however, crisis decreases the likelihood of democratization. The graph also shows the distribution of *Neighbor GDPpc* in the sample, with the density depicted along the vertical axis on the right side. The bulk of dictatorships have neighbors with GDP per capita less than $3000, suggesting that economic crises increase the likelihood of democratization in most regimes. However, this analysis suggests that economic crisis has little liberalizing effect in regimes with rich neighbors. The bottom panel of Figure 3 repeats this analysis, but with a -10% economic crisis. The pattern remains the same, though the scale on the left axis is now larger. Deep economic crisis in regimes with relatively poor neighbors increase the likelihood of democratization by between 2% and 4%, while crises in countries with rich neighbors again appear to decrease the chances of democracy.
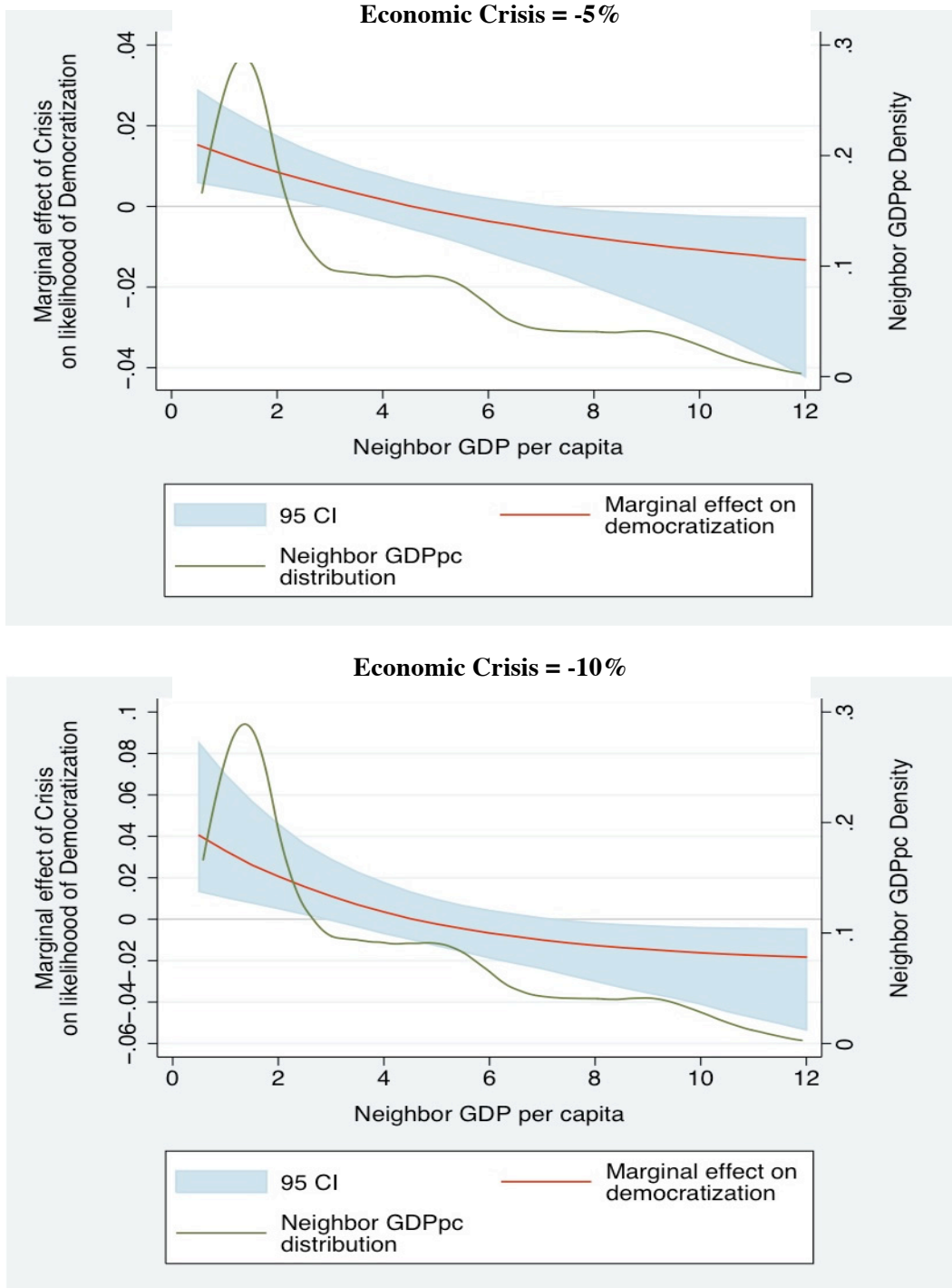
TABLE 7

GROWTH, NEIGHBOR GDP, AND DEMOCRACY

	4-point D in Polity			Full Democratic Transition		
	(1)	(2)	(3)	(4)	(6)	(6)
Crisis	12.220**	10.120**	9.637**	14.100**	13.585**	13.017**
	(3.90)	(4.79)	(3.95)	(4.16)	(5.08)	(4.50)
Crisis*Neighbor GDPpc	-2.657**	-1.858**	-2.479**	-2.250**	-1.536	-2.133**
	(0.85)	(0.86)	(0.87)	(0.86)	(0.98)	(0.84)
Neighbor GDPpc	0.001	0.025	0.039	-0.123*	-0.097	0.035
	(0.07)	(0.07)	(0.09)	(0.07)	(0.08)	(0.08)
Log(population)	0.094	0.033	0.152	-0.312	-0.470**	-0.265
	(0.22)	(0.24)	(0.26)	(0.20)	(0.20)	(0.26)
Log(GDP)	0.087	0.081	0.007	0.523**	0.550**	0.412
	(0.20)	(0.20)	(0.21)	(0.22)	(0.19)	(0.27)
Neighbor democracy	0.980**	0.976**	1.023*	1.235**	0.986*	0.708
	(0.33)	(0.43)	(0.53)	(0.38)	(0.52)	(0.56)
Conflict	-1.182	-1.671*	-1.042	0.370	0.119	0.269
	(0.78)	(0.99)	(0.77)	(0.49)	(0.58)	(0.50)
Post-Cold War	0.677**	0.774**	0.993**	0.862**	1.046**	1.319**
	(0.34)	(0.37)	(0.43)	(0.26)	(0.29)	(0.32)
Military		1.719**			1.786**	
		(0.47)			(0.44)	
Single party		-1.359**			-1.567**	
		(0.46)			(0.41)	
Monarchy		-2.452**	-1.131		-3.140**	-1.137
		(1.13)	(0.98)		(1.25)	(1.08)
Islam			-0.008			-0.007
			(0.01)			(0.01)
Oil rents			-0.153			-0.342
			(0.23)			(0.25)
Polity(t-1)	0.078**	0.080**	0.065*			
	(0.03)	(0.03)	(0.04)			
Lifetime		0.082**		0.024	0.114**	0.038
		(0.03)		(0.02)	(0.03)	(0.03)
Lifetime-sq.		-0.001**		-0.000	-0.001**	-0.001
		(0.00)		(0.00)	(0.00)	(0.00)
Constant	-6.844**	-7.239**	-5.743**	-10.177**	-10.523**	-9.059**
	(1.71)	(1.75)	(1.81)	(2.01)	(1.68)	(2.50)
Log likelihood	-328.4	-295.1	-277.4	-308.7	-274.7	-248.2
Observations	3685	3685	3036	3911	3911	3118
Crisis @ 75th percentile	-1.9	0.3	-3.5	2.2	5.4*	1.7
of Trade instrument	(3.2)	(3.5)	(3.5)	(2.7)	(3.0)	(2.8)
Crisis @ 25th percentile	8.8***	7.7*	6.4**	11.2***	11.6***	10.2***
of Trade instrument	(3.2)	(4.0)	(3.3)	(3.3)	(4.1)	(3.7)

*p<0.05; **p<0.01. Dependent variable is a 4-point or more increase in the *Polity* score (1–3) and a one-off full transition to democracy (4–6). Logit with standard errors clustered on country.

FIGURE 3

ECONOMIC CRISIS AND DEMOCRATIZATION, BY NEIGHBOR GDP PER CAPITA

Economic Crisis = -5%

Economic Crisis = -10%

It is worth referring back to Table 2, which shows the mean level of neighbor GDP per capita for different regions of the world, and noting that Middle Eastern and North African authoritarian regimes on average have richer neighbors than dictatorships in any other region outside of Europe. This is due in part to their proximity to Europe itself, but also to the presence of rich oil-exporting Persian Gulf regimes in the region. Many scholars have fingered Islam and the natural resource curse as explanations for the relative dearth of democracy in this region despite cycles of economic boom and bust that have felled dictatorships in many other regions of the world. The analysis in this section points to another possibility: that having rich neighbors blunts the liberalizing effect of economic crisis by offering an opportunity for those most likely to revolt and press for democracy in the midst of economic hard times a chance to migrate to a nearby and *wealthy* neighbor.

CRISIS, MIGRATION, AND DEMOCRACY

The third measure of exit costs is migration. Recall that migration measures the net migration to and from a country over the past decade. Higher values for migration represent more migrants leaving the country than entering the country so the coefficient for *Migration* can be interpreted in the same manner as trade levels or neighboring country GDP per capita: higher values represent more exit mobility.

To test the effect of crisis on democratization, conditional on migration, I control for *log(GDPpc)* rather than *log(GDP)* and *log(population)* because the measure of migration has population in the denominator. As suggested by the migration patterns in oil-rich Gulf nations in Figure 1, oil rents may be correlated with migration patterns. Positive oil price shocks increase net (in)migration, while negative shocks are typically associated with an outflow of migrant workers. Further, we know that oil rents can affect democracy (Ross 2001, Jensen & Wantchekon 2004). Thus all models include a variable measuring (the natural log of) lagged oil production multiplied by the world oil price (*Oil Rents*), from Humphreys (2005). Finally, labor migration often involves males leaving the home country in larger numbers than females, so these models also include the share of the population that is female, leading to the following baseline specification:

$$Dem.= \beta_0+\beta_1 Crisis+\beta_2 Crisis*Migration+\beta_3 Migration+\beta_4 Ln(GDPpc)+$$
$$\beta_5 Ln(Oil)+\beta_6 Female\%+\beta_8 Polity_{t-1}+\varepsilon \qquad (2)$$

Equation (2) assumes that migration is exogenous to the process of democracy. To address concerns of endogeneity, I also test a two-stage model that uses the predicted trade values from Table 1 in the previous section as an instrument for migration. The first stage regresses migration on all the other explanatory variables in (4) plus the trade instrument and population growth. I then calculate the predicted migration level and the first stage residual. I then estimate the following equation:

$$Dem.= \beta_0+\beta_1 Crisis+\beta_2 Migr\hat{a}tion+\beta_3 Ln(GDPpc)+$$
$$\beta_4 Ln(Oil)+\beta_5 Female\%+\beta6 PostColdWar+\beta_7 Polity_{t-1}+\varepsilon \qquad (3)$$

Note that equation (3) does not include the interaction between *Crisis* and *Migration* and substitutes *Migr$\hat{a}$tion* for *Migration*. I estimate (3) to test whether the errors in the first and second stage are correlated.[37] Finally, I interact *Migr$\hat{a}$tion* with *Crisis* and include this in the second stage:

$$Dem.= \beta_0+\beta_1 Crisis+\beta_2 Migr\hat{a}tion+\beta_3 Migr\hat{a}tion*Crisis+\beta_4 Ln(GDPpc)+$$
$$\beta_5 Ln(Oil)+\beta_6 Female\%+\beta7 PostColdWar+\beta_8 Polity_{t-1}+\varepsilon \qquad (4)$$

The last model reported in Table 8 is equation (4). The Wald test for exogeneity in (3) indicates that the errors from the two stages are not correlated.

Table 8 reports the results for migration. The first column reports the base model specified in equation (2): the negative coefficient on the interaction between *Crisis* and *Migration* suggests that as migration increases (more people leave), the effect of *Crisis* on the likelihood of democratization decreases. The second model adds further controls. The main result is slightly stronger. The third model excludes observations from oil-rich Persian Gulf countries such as Saudi Arabia and Kuwait[38] to ensure the results are not driven by these somewhat peculiar regimes that disproportionately absorb many migrants. The main result is again slightly stronger. The next two models test alternate measures of the dependent variable. Model 4 uses a dependent variable derived from the Polity scale with a 3-point or more increase as the threshold for democratization instead of the 4-point threshold. Model 5 using the strictly dichotomous measure of a one-off transition to

democracy. The results in both of these specifications strongly support the main finding. The final model tests a two-stage model as described above. Here the coefficient for the interaction between *Crisis* and *Migration* is still negative and statistically different from zero, though smaller in absolute size.

TABLE 8

CRISIS, MIGRATION, AND DEMOCRACY

						IV
Dependent variable	dPol4	dPol4	dPol4	dPol3	Democ.	dPol4
	(1)	(2)	(3)	(4)	(5)	(6)
Crisis	3.979	3.309	4.143*	4.672*	7.859**	2.488*
	(2.52)	(2.52)	(2.51)	(2.58)	(2.93)	(1.33)
Crisis*Migration	-2.049*	-2.861**	-3.032**	-3.609**	-4.047**	-1.862**
	(1.19)	(1.10)	(1.36)	(1.16)	(1.47)	(0.91)
Migration	0.256*	0.319**	0.352**	0.337**	0.266	0.216
	(0.16)	(0.16)	(0.17)	(0.16)	(0.20)	(0.16)
Log(GDPpc)	0.476**	0.388*	0.376	0.364*	0.663**	0.165
	(0.23)	(0.22)	(0.23)	(0.21)	(0.22)	(0.10)
Female %	-0.048	-0.240*	-0.407**	-0.267**	-0.109	-0.103*
	(0.10)	(0.12)	(0.19)	(0.12)	(0.10)	(0.06)
Oil rents	-0.386**	-0.375	-0.276	-0.428*	-0.545**	-0.167
	(0.19)	(0.25)	(0.23)	(0.26)	(0.27)	(0.11)
Conflict		-0.981	-1.018	-1.068	0.372	-0.438
		(0.74)	(0.75)	(0.74)	(0.43)	(0.28)
Post-Cold War		1.28**	1.30**	1.27**	1.46**	0.52**
		(0.37)	(0.37)	(0.34)	(0.32)	(0.16)
Monarchy		-2.178	-1.268	-2.342	-1.762	-0.790
		(1.79)	(1.05)	(1.90)	(1.22)	(0.56)
Islam		-0.010	-0.011	-0.009	-0.008	-0.004
		(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Polity(t-1)	0.088**	0.055	0.059*	0.066**		0.030*
	(0.03)	(0.03)	(0.03)	(0.03)		(0.02)
Lifetime					0.038	
					(0.03)	
Lifetime-sq.					-0.001	
					(0.00)	
Constant	-5.381	5.064	13.453	6.550	-4.135	1.798
	(6.05)	(6.77)	(10.36)	(6.73)	(5.09)	(3.18)
Log likelihood	-287.199	-271.760	-269.670	-283.464	-239.330	-257.878
Observations	2889	2889	2740	2889	2971	2543

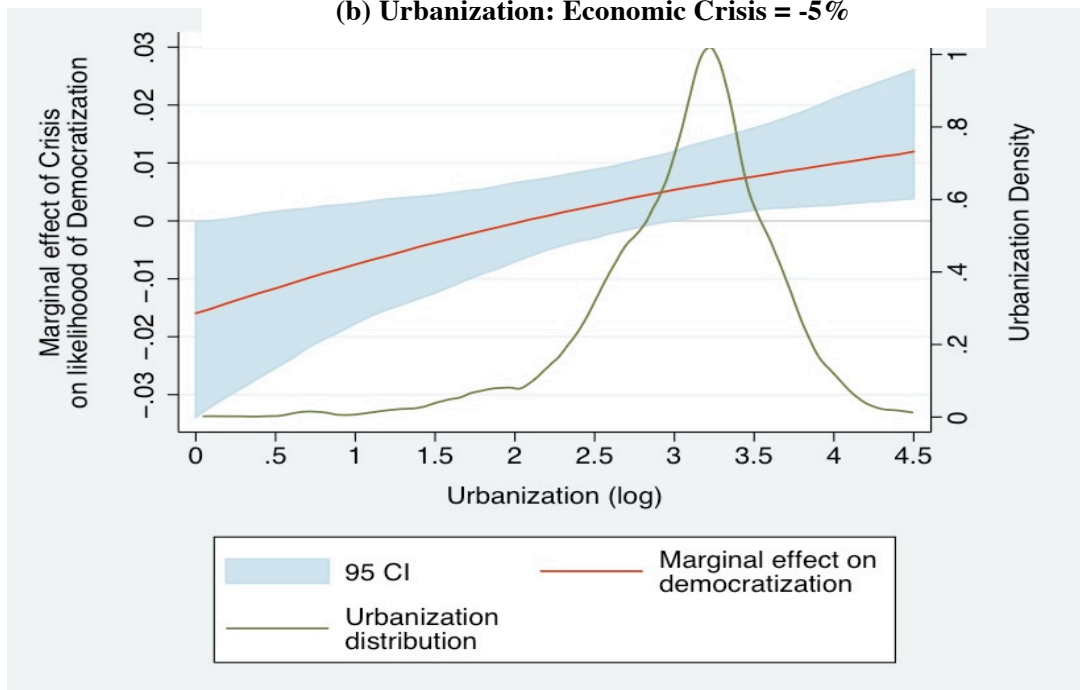$p<0.05$; ** $p<0.01$. Logit with standard errors clustered on country.

FIGURE 4

ECONOMIC CRISIS AND DEMOCRATIZATION, BY MIGRATION AND URBANIZATION

(a) Out Migration: Economic Crisis = -5%
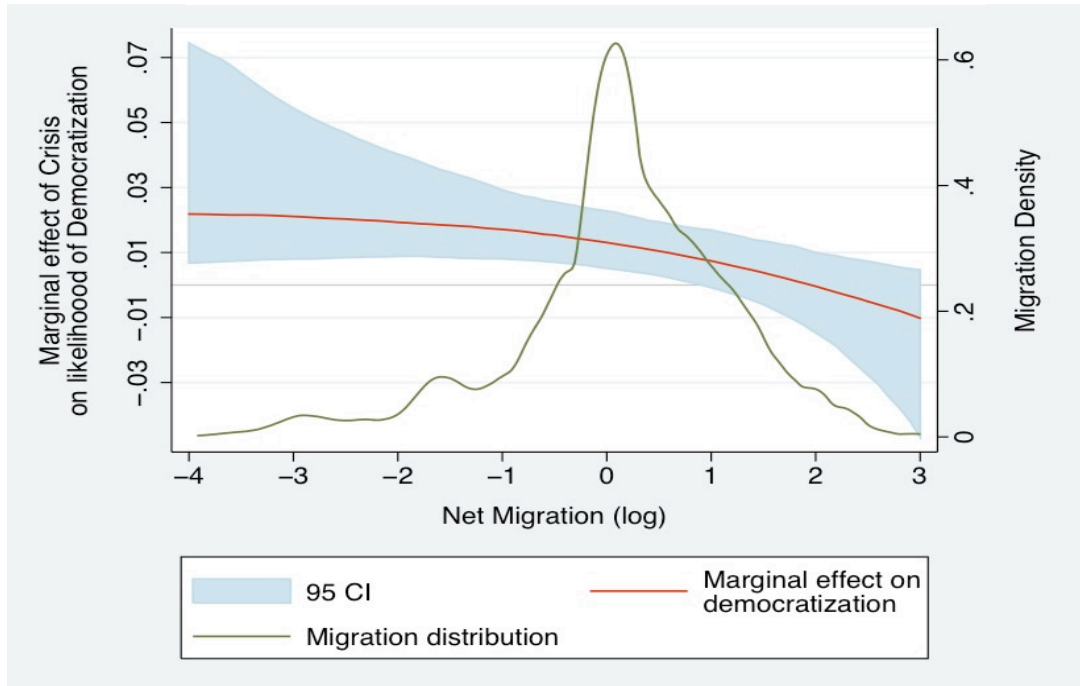
(b) Urbanization: Economic Crisis = -5%

The top panel (a) of Figure 4 uses the model reported in column 5 to simulate the marginal effect of economic crisis on the likelihood of democratization at various levels of migration. The left vertical axis displays this marginal effect and the horizontal axis measures migration. The graph also shows the distribution of migration. The top panel shows the marginal effect of a -5% economic crisis. For all regimes with negative migration (more migrants entering than leaving), economic crisis increases the likelihood of democratization by over 1%. For the regimes that experience positive migration (more leaving than entering), the effect of crisis is not statistically different from zero, even turning negative at high levels of out-migration. A graph of the same simulations (not reported), but with a -10% economic crisis instead of a -5% economic crisis, shows a similar pattern: increasing the likelihood of democracy between 2.5% and 7% in regimes with negative migration.

Recall that the measure of migration attempts to capture long-term migration trends over the previous ten years. Countries with positive migration have seen more people leave than enter. This finding suggests that economic crisis has little effect on the likelihood of democratization in regimes in which historically have more out-migration. An implication of this finding suggests that when migrants leave, the citizens left behind are less likely to demand democracy in the face of economic crisis.

FURTHER IMPLICATIONS OF MIGRATION

That past migration may condition the democratizing effect of economic crisis suggests that exit mobility, proxied by past migration, affects this process by allowing some citizens to easily leave. If the citizens who leave are those who are most likely (all else equal) to overcome collective action costs and mobilize against the regime, then those who do not migrate would be less likely, accordingly, to resist the regime and press for democracy. This suggests that if the causal path from crisis to democracy leads through protest mobilization, then the crisis effect may be conditional on the propensity of the certain types of citizens to protest. A large literature posits that one group of citizens may have a higher likelihood of initiating violence that can potentially unseat authoritarian rulers: a rapidly urbanizing population.

Some of the migration literature focuses on the relative trade-offs of internal and external migration (Stark & Taylor 1991, Daveri & Faini 1999). For example, if potential migrants emigrate in response to changes in relative incomes (Stark & Taylor 1989), migration decisions will be affected by comparison of incomes in home regions of the home country to both: (1) other regions in the home country (internal migration); and (2) incomes in foreign countries (external migration). While it is well beyond the scope of this study to anticipate why different types of migration occur, that a change in economic status can cause both internal and external migration suggests it is possible that economic crisis—or a change in relative deprivation—leads to urbanization (internal migration) instead of external migration in some cases. Again, if the causal link between crisis and democratization runs through protest mobilization and the threat of violence against the regime, internal migration in the form of rapid urbanization might *increase* the likelihood that economic crisis leads to protest (Walter & Ragin 1990) and thus democratization.

The crisis effect may thus be stronger in rapidly urbanizing regimes—one reason observers suggest that China's stability is predicated on avoiding economic crisis, particularly amidst rapid urbanization. This implication complements the migration finding in another important way. Herbst (1990) argued that in post-colonial Africa, independence for many smaller countries that formerly comprised larger colonial areas meant an increase in the number of African borders and the loss of regional (colonial) transport networks and economic systems. The days of external migration, he argued, were numbered. Instead, the new migration in much of Africa would be internal, particularly rural to urban migration.[39] Realizing the destabilizing effect of rapid urbanization, African leaders pursued a perverse policy of subsidizing urban citizens at the expense of rural areas to quell potential political instability resulting from increased urban migration (Bates 1981). Secession and retreat into the informal economy,[40] Herbst posits, may not be sufficient exit options to diffuse discontent. Hence some emphasize protest in the literature on political liberalization in the early 1990s in sub-Saharan Africa (Bratton & van de Walle 1997).

This may not be a story that rings true only in Africa. There is evidence from other regions of the world that economic crisis causes internal migration, especially in the context of other globalizing forces such as increased foreign capital flows. For example,

Perz (2000) finds that economic crisis, combined with rapid globalization in rural Brazil in the 1980s, led to large-scale internal migration away from rural areas to small urban and metropolitan areas.

TABLE 9

CRISIS, URBANIZATION, AND DEMOCRATIZATION

	(1)	(2)	(3)	(4)
Crisis	-19.044**	-27.587**	-10.598*	-14.048*
	(8.05)	(10.99)	(6.06)	(8.08)
Crisis*Urbanization	7.246**	10.833**	5.145**	7.836**
	(2.63)	(3.50)	(2.05)	(2.68)
Urbanization	-0.171*	-0.131	-0.233**	-0.164*
	(0.10)	(0.11)	(0.09)	(0.10)
Urban % (ceiling effect)	0.004	0.012	0.006	0.002
	(0.01)	(0.01)	(0.01)	(0.01)
Log(GDPpc)	-0.114	-0.007	-0.160	0.419
	(0.24)	(0.29)	(0.28)	(0.40)
Conflict	-1.079	-0.930	0.265	0.318
	(0.77)	(0.75)	(0.48)	(0.45)
Neighbor democracy	1.061**	0.964**	0.919**	0.597
	(0.33)	(0.43)	(0.31)	(0.42)
Post-Cold War	0.499	0.980**	0.562*	1.205**
	(0.38)	(0.39)	(0.29)	(0.31)
Islam		-0.007		-0.007
		(0.01)		(0.01)
Oil Rents		-0.276		-0.482*
		(0.22)		(0.26)
Monarchy		-1.268		-1.409
		(1.11)		(1.12)
Polity$_{(t-1)}$	0.073**	0.047		
	(0.03)	(0.04)		
Lifetime			0.010	0.034
			(0.03)	(0.03)
Lifetime-sq.			0.000	-0.000
			(0.00)	(0.00)
Constant	-3.100*	-3.981*	-2.770	-7.151**
	(1.67)	(2.09)	(1.76)	(2.60)
Log likelihood	-273.931	-244.559	-274.241	-229.008
Observations	2886	2631	3048	2686

* p<0.05; ** p<0.01. Dependent variable is a 4-point or more increase in the *Polity* score. Logit with standard errors clustered on country.

To test these implications of the urbanization argument, I construct a variable that measures the logged value of urbanization over the previous five years.[41] I then interact this variable with economic crisis. Unlike external migration, we should expect urbanization to *increase* the crisis effect. That is, economic crisis should have a stronger effect on the likelihood of democratization in authoritarian regimes with high levels of urbanization.

Table 9 reports the results. The first two models employ the dependent variable derived from the Polity scale while the latter two use the one-off measure of full transition to democracy. The second and fourth models add controls for *Islam*, *Oil Rents*, and *Monarchy*. Consistent with expectations, the coefficient for the interaction between *Crisis* and *Urbanization* in all four models is positive and statistically significant at conventional levels. This suggests that the liberalizing effect of crisis gets stronger as urbanization rates increase. The lower panel (b) of Figure 4 illustrates the substantive effect of a -5% economic contraction on the likelihood of democratization across a range of values for (logged) urbanization.[42] At the median level of urbanization, economic crisis increases the likelihood of democratization by about 0.7%. At low levels of urbanization, this effect becomes negative (though not statistically different from zero until urbanization rates approach zero). At high urbanization rates (75th percentile), crisis is associated with a 1% increase in the likelihood of democratization. A deeper economic contraction of -10% produces larger increases in the likelihood of democratization: an increase of 1.4% at the median level of urbanization and a 2.6% increase at the 75th percentile.

DISCUSSION

The findings in this paper provide evidence consistent with the hypothesis that higher exit mobility helps insulate dictators from the liberalizing effect of economic crisis. To measure exit costs, I employ three different variables, each of which not only serve as plausible proxies for exit mobility, but which also points towards its own set of implications: a geographic instrument for bilateral trade; neighboring country GDP per capita; and past net migration.

Trade and Globalization

The literature on the relationship between trade and democracy has mostly examined democracy's effect on trade (Mansfield & Rosendorff 2000, Milner & Kubota 2005, Kono 2008). However, as attention has turned to the reverse relationship, scholars have focused on establishing the exogenous effect of trade on democracy by constructing an instrument using a gravity model of bilateral trade (López-Córdova & Meissner 2008). Employing geographic variables in a gravity model of trade pushes us closer to establishing exogeneity than simply regressing democracy on trade. I argue that geographic instruments also provide an intuitive proxy for the costs of exit for citizens living under authoritarian rule. While distance no doubt increases some of the fixed costs of transport (such as fuel), distant locations, such as islands, may nonetheless have relatively low (net) exit costs because the pathway is so well trod. Sufficient demand can enable suppliers to achieve scale economies to reduce costs. This is one reason the dollar cost of transporting goods from Abidjan to Addis Ababa is more than three times as much as the cost of transporting goods from Abidjan to Japan (Moyo 2009, 124). This insight is captured in the gravity model because islands trade more and landlocked countries trade less. Bordering countries which conduct a considerable amount of trade may also offer relatively low exit costs because trade and labor migration have some of the same geographic, linguistic, and regional determinants. Again, a gravity model of trade accounts for this possibility.

  The results in this study suggest that economic crisis increases the likelihood of democratization in countries with low trade levels. In highly open dictatorships, however, crisis has little effect on democratization. Thus the theory proposed in this paper has implications for the debate over the political consequences of globalization by helping us think about how openness affects the relationship between economic crisis and democratization. Students of globalization have long thought about how increased economic openness affects political outcomes in democracies. Some scholars focus on how the distributional effects of increased trade or financial flows, for example, are translated into political support for social policies of "compensation" (Rodrik 1998, Garrett 2001, Rudra 2002, Rudra & Haggard 2005, Rudra 2005, Brune & Garrett 2005).

Others examine how globalization and economic performance affect electoral outcomes in democracies (Samuels & Hellwig 2007). All of these studies of democracies examine electoral accountability and the political institutions that translate preferences into power because these are the mechanisms which mediate power in democracies.

Thinking about how globalization and economic performance affect political outcomes in dictatorships may also requires us to look at the mechanisms that mediate power in these regimes, though these mechanisms may not be elections and fine distinctions among political institutions that translate votes into power. The study of how institutions mediate power in dictatorships is still in its infancy.[43] That said, I have looked briefly at how the existence of legislative institutions affects the relationship between economic crisis and democratization, finding that legislatures in military and dominant party regimes blunt the destabilizing effect of economic crisis and democratization. This result is consistent with earlier research suggesting that single party regimes, almost all of which have legislatures, are more resilient to economic crisis than other types of dictatorships (Haggard & Kaufman 1995). That legislative institutions in some regimes may condition the effect of economic crisis on democratization suggests that dictators have alternatives as well. Similarly, payoffs from an ample supply of oil rents to an authoritarian government may diffuse discontent stemming from an economic crisis. While I have focused exclusively on the exit options for citizens in this paper, it is also possible that dictators have alternatives—such as institutions in which they can share power or natural resource revenue. But, as the results in this paper suggest, understanding globalization's effect on democracy should account for the exit options available to all players in the game of democratization—in particular the options available to citizens whose only assets may be their own labor. Future research needs to think critically about how alternatives available to both citizens and regime elites affect their strategies during periods of economic crisis.

Living in a Rich Neighborhood

Second, the findings in this paper indicate that economic crisis is less likely to be associated with democratization in dictatorships with relatively rich neighbors. This suggests that that when labor can find higher wages in neighboring countries, economic

crisis is less destabilizing. These findings are consistent with the hypothesis that relatively attractive exit options for citizens, particularly labor, can mitigate the liberalizing effect of economic crisis on democratization.

However, having rich neighbors may not always undermine this liberalizing effect. Some have argued that migration from rural Mexico to the United States (and northern Mexico) during the 1990s helped erode support for the long-ruling PRI (Institutional Revolutionary Party) and paved the way for democratization (Magaloni 2006). Relatively poor rural residents supported the PRI, according to this logic, in exchange for income support and government subsidies. Migration weakened this exchange by disrupting the supply of government-provided (local public) goods as workers moved and by increasing the availability of income, in the form of remittances, untethered from the PRI and their policies. In this case, migration disrupted the clientalism that kept the PRI in power (Pfutze 2009). While this pattern may fit the overall Mexican case, Magaloni (2006, 96) points out that in southern Mexico, where the poorest supporters of the PRI lived, migration may have contributed to "delaying democracy" in manner similar to that posited by Hirshman (1981). This suggests that we might explore regional variation in exit mobility within particular countries to better understand the exit and protest outcomes in response to economic crisis.

If this interpretation of the Mexican case is correct, it provides a useful counter-example for thinking about new ways forward in this research. The conditional effect of neighboring country wealth on the crisis-democratization relationship may only hold for dictatorships of the rich. Thus this research might narrow the scope, but improve its fit by focusing only on authoritarian regimes where the rulers are a coalition of wealthy elite. Moving the research in this direction explicitly entertains the question of who comprises the dictator's coalition. The theoretical framework offered by Acemoglu and Robinson (2001, 2006) and others who follow this line of research abstracts away from this question by focusing exclusively on social class models where the authoritarian ruler represents the upper class. However, regimes supported by different types of coalitions will react differently to increased globalization and economic crisis (Pepinsky 2008). For the present project this would entail thinking carefully about whether Communist regimes in the Cold War era (and even today) were in fact left-wing regimes where a coalition other

than the rich elite were in power. If these, or other regimes (e.g., Malaysia's United Malays National Organisation, or UMNO), were supported by coalitions of the poor rather than the rich, they should be excluded from the scope of this analysis.

The Mexican case, though, illustrates the complexity of thinking about how exit mobility will affect democratization in regimes that are supported in part through clientalistic exchange of monetary goods to poor citizens for (electoral) regime support.[44] On the one hand, exit (and remittances) can decrease the incentive to engage the regime politically, as the evidence from Bravo (2007) and Goodman & Hiskey (2008) suggests. Less political engagement would lead us to think that exit decreases the likelihood of a "threat from below" (Wood 2000), hurting the chances of democratization. Alternatively, exit mobility may sever the clientalistic ties between the regime and its support base (poor citizens), thereby increasing the likelihood of regime breakdown and democratization. That both implications of exit mobility were happening simultaneously in Mexico should lead us to think about why the "breakdown of the clientalistic ties" effect dominated the "political disengagement" effect. For the PAN (National Action Party) to win, they had to collectively mobilize (electorally, in this case) against the regime, and thus may not have been as prone to the "political disengagement" effect of exit as the PRD (Party of the Democratic Revolution) supporters. These are obviously conjectures, but ones that should help us think about the context in which these arguments are likely to ring true.

The rich neighborhood finding has implications for understanding the relative dearth of democracy in the Middle East. As noted earlier, Middle Eastern and North African dictatorships on average have wealthy neighbors—at least relative to authoritarian regimes in other parts of the world outside Europe. The possibility that living in a wealthy neighborhood blunts the liberalizing effect of economic crisis may thus be one factor that contributes to the dearth of democracy in this region. I controlled for some of these competing explanations, such Islam, oil rents, and the presence of monarchies. All of these variables were in the expected ranges, though including them did not alter the main finding. The type of statistical test used in this project, however, can only help explain why this region has less democracy than other regions. But this analysis does a poor job of explaining change within this region over time because the variables used to measure democratization (or movement toward democracy) are necessarily blunt

and vary little over time within in this region. This suggests there is still much work to be done, especially in establishing whether exit mobility conditions the effect of economic crisis across time within this region of the world.

Migration

The third finding in this paper suggests that past migration also conditions the effect of crisis on democratization. Economic crisis is less likely to lead to democratization in authoritarian regimes that have experienced more migrants leaving than entering over the previous decade. The measure of migration used in this analysis attempts to capture past migration patterns, which may be proxying for the types of people who have left and who remain—in particular the types of citizens who can credibly threaten violence. Past (out) migration may therefore imply a relative dearth of citizens most likely and capable of rebelling in the face of economic crisis. I tested an implication of this logic by examining how urbanization rates affect the relationship between economic crisis and democratization. Consistent with the contention that a rapidly urbanizing population may be more prone to collectively organizing violent resistance to regime leaders, I find that higher urbanization rates make crisis more destabilizing.

A more precise measure might look specifically at young male migration.[45] Some of the earlier literature on rebellion argued that demographics matter, in particular the prevalence of young men (Gurr 1970, Goldstone 1991, Elbadawi & Sambanis 2000, Urdal 2006).[46] This does not necessarily imply that women do not protest or demand democracy (see Baldez (2002) for example).[47] However, it may not simply be protesting and demanding democracy, but the threat of violence that is key to the argument developed in class-based theories of conflict and democratization (Boix 2003, Acemoglu & Robinson 2006). Violence may still largely be the province of young men. In a preliminary test of this implication, I examined whether the female share of the population conditioned the relationship between economic crisis and democratization, finding that crisis was more likely to be associated with democratization in countries with more men. This finding was not very robust, though. If past migration is the explanation for the relative absence of men, this finding coincides with the findings for migration.

Both the relatively low cost of exit in the current period and the absence of rebellion-prone citizens due to past migration point to the same general causal mechanism in the theory provided above. However, they do represent distinct processes that may be difficult to distinguish with cross-national data. A case study of a particular country that experienced high (low) past migration but retains relatively costly (cheap) exit options in the current period would provide purchase on disentangling these factors.

There is still much work to be done. The present paper does not address how globalization redistributes income (by factors or sectors) in dictatorships or how this redistribution affects political survival (either alone or conditional on economic crisis). Further, I have not distinguished between types of economic crises, but instead focused on sharp declines in economic output because these are most likely to affect the well-being of non-elite citizens. However, other types of economic crises, such as banking and financial crises, certainly affect the likelihood of democratization—though this effect may vary by who comprises the dictator's coalition (Pepinsky 2008). To understand whether economic crisis would lead to democratization in a particular country (e.g., China), we probably need a more nuanced explanation than the one provided in this paper. However, by proposing a model that focuses on the alternatives available to citizens, this project provides a framework for thinking about these questions.

APPENDIX: A STATIC MODEL OF DEMOCRATIZATION WITH EXIT

Consider a game[48] where a dictator (the rich, r) chooses over democracy (D) or autocracy (A). In democracy, the tax rate is set by the median voter ($\tau^p > 0$), who is poor; in a dictatorship, the tax rate is set to zero (no redistribution) or some rate τ^r, where $\tau^p > \tau^r > 0$. Citizens (the poor) decide whether to revolt (conflict) or not (no conflict). If the citizens do not revolt, the dictator keeps his promise and retains the initial positive tax rate $\tau^r > 0$ (some redistribution) with a probability q, and resets the tax rate to $\tau^r = 0$ (no redistribution) with some probability $1-q$.

$U^r(D)$ is the payoff for the dictator (r) under democracy (D); $U^r(R)$ is the payoff for the dictator under revolution (R); $U^r(A)$ is the payoff for the dictator when there is no revolution and the dictator keeps his promise to redistribute — setting the tax rate to $\tau^r > 0$; $U^r(A^{'})$ is the payoff for the dictator when there is no revolution and the dictator reneges and resets the tax rate to $\tau^r = 0$. Similarly, $U^p(\cdot)$ is the notation for the utility of the citizens (p) under the four different outcomes: *(D,R,A,A)*. The payoffs and sequence of play are shown in Figure A.
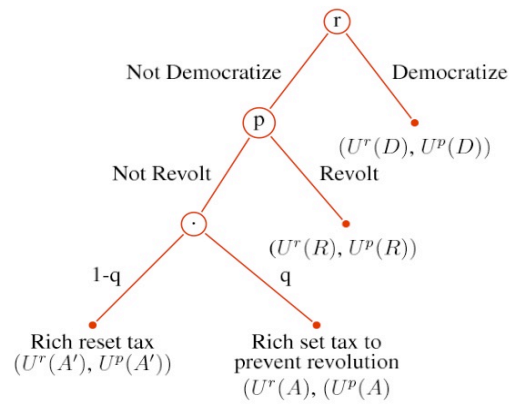
Analyzing the subgame perfect equilibria of this static game of democracy and redistribution,[49] democratization occurs when two conditions are met: (1) citizens prefer to revolt: $U^p(R) > qU^p(A) + (1-q)U^p(A^{'})$; and (2) the dictator prefers democracy to revolution: $U^r(D) > U^r(R)$. The threat of revolution, if costly enough to the dictator, forces him to democratize. This threat of revolution[50] is the essential element of many game-theoretic accounts of democratization that posit a conflict between two (or more) social classes over redistribution is central to the story of democratization (Acemoglu & Robinson 2001, Acemoglu & Robinson 2006, Boix 2003, Wood 2000). Revolution occurs when the rich prefer the revolutionary outcome (or some sustained conflict) to democracy ($U^r(R) > U^r(D)$) and the poor prefer revolution to autocracy ($U^p(R) > qU^p(A) + (1-q)U^p(A^{'})$). Finally, autocracy occurs when the poor prefer autocracy to revolution: $qU^p(A) + (1-q)U^p(A^{'}) > U^p(R)$). Briefly, the poor are more likely to prefer autocracy, all else equal, as: (1) the utility of revolution decreases (or the cost of revolution increases); (2) the probability that the dictator keeps his promise to redistribute (in the face of a

revolutionary threat) increases; or (3) the amount that the dictator promises to redistribute increases.

FIGURE A

DEMOCRATIZATION WITH REVOLT

A1: Democratization with Revolt

Not Democratize   Democratize

$(U^r(D), U^p(D))$

Not Revolt   Revolt

$(U^r(R), U^p(R))$

1-q   q

Rich reset tax
$(U^r(A'), U^p(A'))$

Rich set tax to
prevent revolution
$(U^r(A), (U^p(A)$

$r \equiv$ Dictator (rich)
$p \equiv$ Citizen (poor)

Democratization if:
(1) $U^p(R) > q(U^p(A)) + (1 - q)(U^p(A'))$
(2) $U^r(D) > U^r(R)$

EXIT AND INEQUALITY

We begin with a two-class model (rich and poor) where all individuals within each class have the same income. Income of the rich is denoted y^r and income of the poor is denoted y^p. Average income is $\bar{y}$. A fraction $\delta<1/2$ of citizens are rich and $1-\delta>1/2$ are poor. Let θ represent the share of total income accruing to the rich, and $1-\theta$ be the poor's share of total income. An increase in θ represents a larger share of income for the rich and hence more inequality.

$$y^p = \frac{(1-\theta)\bar{y}}{1-\delta} \qquad\qquad (A1)$$

$$y^r = \frac{\theta\bar{y}}{\delta} \qquad\qquad (A2)$$

Let N be the absolute size of the population, so δN are the number of rich and $(1-\delta)N$ are the number of poor. For a given number of citizens in a given period, the total income of poor and the rich are given by the following:

$$
\begin{aligned}
Y^p &= y^p(1-\delta)N \\
&= (1-\theta)\bar{y}N \\
Y^r &= y^r\delta N \\
&= \theta\bar{y}N
\end{aligned}
$$

Proposition: If the poor exit in larger proportion to the rich, then inequality increases.

Let e be the share of the population that are poor and that exit. By setting $e>0$, we assume that the poor exit is greater proportion than the rich. The absolute number of the poor is now $(1-\delta-e)N$, while the absolute number of rich remains the same at δN. In the next period, the total income of the rich and the poor are the following:

$$
\begin{aligned}
Y_1^p &= y^p(1-\delta-e)N \\
&= \frac{(1-\theta)\bar{y}N(1-\delta-e)}{1-\delta} \qquad\qquad (A3)
\end{aligned}
$$

$$Y_1^r = y^r \delta N$$
$$= \theta \bar{y} N \tag{A4}$$

Let θ^e be the level of inequality after exit:

$$\theta^e = \frac{Y_1^r}{Y_1^r + Y_1^p} \tag{A5}$$

$$= \frac{\theta}{\frac{1-\delta-e+\theta e}{1-\delta}}$$

Let δ^e be the share of the population that is rich after exit:

$$\delta^e = \frac{\delta}{1-e} \tag{A6}$$

It follows that $\theta^e > \theta$ because $\theta < 1$:

$$\theta^e > \theta$$

$$\frac{\theta}{\frac{1-\delta-e+\theta e}{1-\delta}} > \theta$$

$$1 > \frac{1-\delta-e+\theta e}{1-\delta}$$

$$1-\delta > 1-\delta-e+\theta e$$

$$e > \theta e$$

$$1 > \theta$$

Thus, exit can influence the payoffs endogenously by changing the level of inequality (θ) and the share of the population that is rich (δ). To see how this works, we take the derivative of θ^e w.r.t e:

$$\frac{d\theta^e}{de} > 0 \tag{A7}$$

To see why this is the case, note that $\theta-1<0$ and e is in the denominator of (3). $\delta^e = \dfrac{\delta}{1-e}$, so $\dfrac{d\delta^e}{de}>0$. Thus citizen exit, if done in higher proportion than rich exit, increases inequality and the share of the population that is rich.

We next want to explore how this endogenous impact of exit on inequality affects collective action. To this, though, we first specify the revolution constraint and show how exit-induced changes to inequality and the share of the population that are rich affect the revolution constraint.

EXIT, INEQUALITY, AND THE REVOLUTION CONSTRAINT

To understand how exit affects the likelihood of democratization through inequality and the share of the population that is rich, we specify the payoffs to the citizens for revolution $U^p(R)$ and for autocracy with no revolution $U^p(A)$. Following Acemoglu and Robinson (2006, Chapter 5), we assume that during revolution, some fixed amount of resources are destroyed (μ), and all other resources in the economy are distributed among the citizens:

$$U^p(R,\mu) \quad = \quad \frac{(1-\mu)\bar{y}}{1-\delta} \tag{A8}$$

The payoff to the citizens under autocracy with no redistribution is:

$$U^p(A|\tau=0) \quad = \quad y^p \tag{A9}$$

Setting these two payoffs equal to each other yields the revolution constraint. When this condition is met, citizens prefer revolution to autocracy and can credibly threaten the dictator with revolution:

$$\frac{(1-\mu)\bar{y}}{1-\delta} \quad > \quad y^p \tag{A10}$$

Recalling (A1) and substituting for y^p in (A11):

$$\theta > \mu \tag{A11}$$

We incorporate exit into the revolutionary constraint by substituting (A6) into (A11), yielding the following revolution constraint:

$$\frac{\theta}{\frac{1-\delta-e+\theta e}{1-\delta}} > \mu$$

$$\theta(1-\delta)-\mu(1+\delta)+\mu(1-\theta)e > 0 \tag{A12}$$

Because $\mu(1-\theta)>0$, an increase in exit (assuming the poor exit in higher proportion than the rich) makes the revolution constraint more likely to bind. Intuitively, as the poor leave, inequality increases making revolution more attractive for citizens. This suggests that endogenous exit may make democracy more likely as it increases the chances that the revolutionary threat will be binding. Thus at first glance, the endogenous effect of exit on inequality would appear to work in favor of democratization.

However, the effect of exit on the revolution constraint is more complex if we introduce collective action into the game. Thus far, we have assumed that the cost of revolution for the citizens is an exogenous fixed value of μ. In the next section, we allow exit to affect the prospects for collective action against the dictator. We expand the model introduced in Acemoglu and Robinson (2006) to allow for exit to increase the share of the population that is rich,[51] which in turn affects the prospects of successful collective action. Thus the effect of exit on the revolution constraint works through two countervailing mechanisms: while exit increases the level of inequality and makes the revolution constraint more likely to bind, exit also increases the share of the population that is rich and thus raises the effective cost of collective action and makes the revolution constraint less likely to be binding.

Before we examine the prospects for collective action, we note that if exit increases inequality, as this section suggests, exit also makes democracy less attractive to the rich because higher inequality means a higher tax rate under democracy. In this section and the next, we analyze the effect of exit on the revolution constraint (via

inequality and collective action) by concentrating on the relative payoffs of autocracy and revolution for the poor citizens. However, exit's effect on inequality also factors into the dictator's decision over whether to use repression or permit democracy. If exit increases inequality to the point where it pushes the value of democracy (for a dictator) lower than the threshold where the rich prefer repression to democracy, then increased inequality will lower likelihood of democratization.

EXIT, COLLECTIVE ACTION, AND THE REVOLUTION CONSTRAINT

In this section, I examine how exit affects the prospects of democratization when exit alters the collective action calculus of the citizens. Exit can affect the level of inequality and the relative share of the population that is rich, which in turn determines the incentives of the citizens to overcome collective action problems and pursue revolution. Assuming that citizens (poor) exit in larger proportion than the rich, exit increases the collective action threshold necessary to successfully pursue revolution. Briefly, if the collective action costs—or the technology required to surmount a successful revolution— are fixed relative to the size of the rich, then a decrease in the number of the poor means that a higher share of the poor are required to participate in the revolt for it be successful. Put another way, if the absolute number of citizens required to participate in successful collective action against the regime is fixed, not as a proportion of the poor, but relative to the size and strength of the rich, then a exit by the poor will raise collective action costs and decrease the threat of revolution. Formally, exit by the poor increases the share of the population that are rich, as in equation (6), and this in turn raises the effective threshold for collective action.

Assume a cost $\varepsilon \bar{y}$ for participating in collective action in revolt against a dictator. Next allow those who participate in collective action (if successful) to confiscate the income of the rich to distribute among only those who act collectively (Olson's exclusion). Let $\xi^p < 1 - \delta$ be the number of poor necessary for successful revolution. The revolution constraint is now:

$$y^p + \frac{(1-\mu)y^r}{\xi^p} - \varepsilon\bar{y} \quad > \quad y^p$$

$$\frac{(1-\mu)y^r}{\xi^p} \quad > \quad \varepsilon\bar{y} \tag{A13}$$

Recalling (2) and substituting for y^r in (10) yields:

$$\frac{(1-\mu)\theta\bar{y}}{\xi^p\delta} \quad > \quad \varepsilon\bar{y}$$

$$\theta \quad > \quad \frac{\varepsilon\xi^p\delta}{1-\mu} \tag{A14}$$

Equation (A14) is similar to (A11) except the right side is now $\frac{\varepsilon\xi^p\delta}{1-\mu}$ instead of μ. In the previous section, we modified the left side of this equation by substituting θ^e for θ—that is, allowing exit to affect inequality. We now modify the right side of (A14) as well by allowing exit to affect the share of the population that is rich, by substituting δ^e for δ. The new revolution constrain that models the effect of exit on both inequality and collective action (via the share of the population that is rich) is:

$$\frac{\theta(1-\delta)}{1-\delta-e(1-\theta)} \quad > \quad \frac{\varepsilon\xi^p\delta}{(1-\mu)(1-e)} \tag{A15}$$

$$\theta(1-\delta)(1-\mu)(1-e) \quad > \quad \varepsilon\xi^p\delta[1-\delta-e(1-\theta)]$$

$$\theta(1-\delta)(1-\mu)(1-e)-\varepsilon\xi^p\delta(1-\delta-e+\theta e) > \quad 0$$

Rearranging and dropping terms that do not include e, yields the following:

$$e[\varepsilon\xi^p\delta(1-\theta)-\theta(1-\delta)(1-\mu)] \quad > \quad 0$$

The derivative of e with respect to the revolutionary constraint is therefore:

$$\varepsilon\xi^p\delta(1-\theta)-\theta(1-\delta)(1-\mu)$$

This expression is less than zero, implying that exit decreases the chances that the revolution constraint is binding, if:

$$\theta(1-\delta)(1-\mu) \quad > \quad \varepsilon\xi^{p}\delta(1-\theta) \qquad\qquad (A16)$$

To see whether this inequality holds, note that $\theta > 1-\theta$ and by definition $1-\delta > \xi^{p}$. Thus for (A19) to be false, it would have to be true that $1-\mu < \delta\varepsilon$, which (recall $\delta < 1$) would imply that $\varepsilon+\mu > 1$. If this latter expression is true, it would imply that the share of income destroyed in revolution plus the share of income born in the cost of collective action would be greater than total income. It is implausible that the poor would pursue revolution when the costs of collective action necessary to credibly threaten revolt plus the cost revolution itself are greater than the total income in the economy. Thus in any plausible scenario, exit decreases the likelihood that the revolutionary constraint is binding. The net effect of exit—allowing for exit to affect both inequality and the costs of collective action (via the share of the population that is rich)—is to decrease the credibility of the poor's threat of revolution.

THE COMPOUNDING EFFECT OF EXIT ON COLLECTIVE ACTION

To this point, we have taken the *share* of the poor necessary to achieve successful collective action as an exogenous constant, ξ^p. We then examined how an exogenous increase in exit by the poor (e) affects the revolutionary constraint in one period. Now we allow the collective action threshold to vary over time as a proportion of the population by fixing the absolute threshold.

The absolute threshold for collective action is fixed (before any exit) at $\xi_0^p * N_0$. After exit, the new collective action threshold expressed as a *share* of the population is the absolute threshold divided by the new (absolute) number of citizens (poor and rich). Continuing to assume that the poor exit in larger proportion than the rich ($e>0$), the absolute number of rich remains the same as when there is no exit, but the number of poor is now smaller because some have exited:

$$\xi_e^p \;=\; \frac{\xi_0^p N_0}{\delta_0 N_0 + [(1-\delta_{t-1})(1-e)N_{t-1}]} \tag{A17}$$

where δ_{t-1} and N_{t-1} are the share of the population that is poor and the absolute size of the population in the previous period, respectively. For example, after an initial period of exit, the absolute number of poor is simply $(1-\delta_0)(1-e)N_0$. As can be seen in (12), an increase in exit (e) increases the threshold for collective action as a share of the population (ξ_e^p).

That is, $\dfrac{d\xi_e^p}{de}>0$.

The comparative statics in the last subsection showed that an increase in exit (not accounting for the compounding effect of exit on the collective action threshold) decreases the likelihood that the revolution constraint will be binding. A brief inspection of (A15) shows that, assuming there is still inequality ($\theta>1/2$) and the poor are still a majority ($\delta<1/2$), increasing the collective action threshold reduces the likelihood the revolution constraint binds. Therefore, if exit increases collective action threshold in (A17), then introducing the exit effect on collective action into the revolution constraint in (A15) further decreases the prospects that the revolutionary constraint will bind.

ENDNOTES

[1] Numerous scholars argue that policy responses to economic crisis trigger popular protest and regime instability (Bienen & Gersovitz 1986, Nelson 1990, Walton & Seddon 1994).

[2] Even though he was forced into a power-sharing agreement with the opposition in 2009, Mugabe still maintains control over the military, internal security, and the judicial system. To win back control of the Parliament, Zimbabwe African National Union–Patriotic Trust (ZANU/PF), has begun prosecuting sitting opposition MPs in an effort to disqualifying them from Parliament.

[3] From 1981–1983 and again from 1990–1994, per capita GDP *decreased* by over 7% annually; in the three years from 1998 to 2001, annual growth averaged less than -4%.

[4] In Zimbabwe, for example, former cabinet minister Simba Makoni a member of the ruling ZANU/PF, defected to the opposition and ran against Mugabe in the 2008 election.

[5] Eyadema died in office in February 2005; Mugabe remains in power in March 2010.

[6] See Li & Reuveny (2003) for a list arguments that link globalization to democratization.

[7] Scholars have also examined how economic crisis (Remmer 1990) and economic openness (Samuels & Hellwig 2007) affect political outcomes in developing country democracies. These studies focus on accountability and the electoral consequences for incumbent politicians.

[8] On testing the implications of the rentier thesis, see also (Ross 2001, Smith 2004, Morrison 2009).

[9] In related research, I examine whether the presence of oil revenue and foreign aid conditions the relationship between economic crisis and democratization, finding that the availability of oil revenue diminishes the liberalizing effect of economic crisis, while foreign aid actually increases that effect. This finding for oil revenue is consistent with the rentier-state thesis. The finding for foreign aid, however, suggests not only that foreign aid and oil affect democratization differently, but that at least since 1960, foreign aid conditionality may be one reason why economic crisis leads to democratization.

[10] See also Bates & Lien (1985).

[11] See Hirschman (1970), Gelbach (2006), Clark, Golder & Golder (2007) on games of exit, voice, and loyalty. See Wood (2000), Acemoglu & Robinson (2001), Acemoglu & Robinson (2006), Boix (2003), Morrison (2007) on games of democratization that model conflict between two classes.

[12] Rogowski's (1998) model allows for a binary decision between exit and not exit.

[13] In the Appendix, I illustrate the effect of exit on the decision of the lower class by endogenizing exit in a well-known model of democratization (Acemoglu & Robinson 2006). If

the poor exit in larger proportion than the rich, exit increases inequality but makes collective action against the regime more difficult and the threat of revolution less likely by raising the threshold for successful collective action. If the collective action costs—or the technology required to surmount a successful revolution—are fixed relative to the size of the rich, then a decrease in the number of the poor via exit means that a higher share of the poor are required to participate in the revolt for it be successful. Higher collective action costs that result from exit lower the chances that the threat of revolution by the poor is credible, thereby decreasing the likelihood of democratization.

[14]   See also Milner & Keohane (1996).

[15]   See also Hirschman (1993).

[16]   "Rumblings within." *Economist*. June 21, 2007. The World Bank estimates the total population of Zimbabwe in 2005 at 13 million. The World Food Programme estimated that over 4 million Zimbabweans would need food assistance in late 2007 or would face starvation. Presumably, the return on labor for these 4 million is close to zero.

[17]   The Movement for Democratic Change, the main opposition party, pushed for the ruling ZANU-PF to allow the refugees to vote in the 2008 elections, suggesting that these refugees support the opposition.

[18]   The one exception is common language, though including this variable does not change the results. The correlation between the trade instrument used in this analysis and a trade instrument using the same gravity equation but without the common language variable is 0.99.

[19]   Adding population to the gravity equation in Table 1 also changes the sign on the estimated coefficient for *Island* from negative to positive—the expected direction.

[20]   All GDP figures in constant $US (2000). While GDP per capita only captures averages, the UN estimates that the distribution of income as measured by a GINI coefficient is similar in South Africa (0.59 in 1995; 0.58 in 2000) and Zimbabwe (0.57 in 1995), though the GINI in Zimbabwe has likely changed since 1995.

[21]   Data from Maddison (2004).

[22]   I thank Xun Cao for sharing data on distance.

[23]   Clemens et al. (2009), however, use individual-level survey data on wages and occupation in 43 countries in one time period to assess the bilateral wage differentials caused by migration restrictions.

[24] IRIN 2009. "South Africa: The ebb and flow of Zimbabwean migrants." July 28, 2009.
available at:
http://lite.alertnet.org/thenews/newsdesk/IRIN/0947a4fc2b7a968f9e4693f395460d5c.htm

[25] Logging the *CrisisLength* variable or including *CrisisLength*^{2} does not appreciably change the results.

[26] (Smith (2004) and Morrison (2009) use a three-point threshold, but combine both increases and decreases in the Polity scale into one binary measure.

[27] I also checked the robustness of the main result in Table 3 in two other ways. First, I use a binary indicator of democratization that marks an increase in the Polity score above the threshold of 7 (on a -10 to 10 scale). Second, I estimated an error-correction model with the continuous Polity scale as the dependent variable. Both tests yielded positive results for the main hypothesis.

[28] A likelihood ratio test suggests that the total error variance due to the inclusion of random effects is statistically different from zero. However, the coefficients on the main variables of interest are not statistically different from the same coefficients in models without random effects. Results are available from the author.

[29] Using 3000 kilometers instead of 2000 kilometers does not change any of the results.

[30] Gasiorowski (1995) also finds that military regimes are much more likely to democratize, in part because of their vulnerability to economic crisis. The updated data on authoritarian regimes (Wright 2008) includes updated coding on authoritarian regime type (military, single party, monarch, and personal—the omitted category).

[31] Carter & Signorino (2008) suggest including three duration polynomials: *Lifetime*, *Lifetime*^{2}, and *Lifetime*^{3}. Including this last polynomial does not change the result. F-tests indicate that it does not belong in the equation.

[32] Ai and Norton (2003) show that directly interpreting the sign and statistical significance of an interaction term in a non-linear model can lead to erroneous conclusions because the substantive effect of the interaction term varies by the value of other covariates. Using their software (Norton, Wang, and Ai 2004), I computed the value of the interaction term across all predicted values and found that the distribution of corrected interaction terms was generally more negative than the uncorrected interaction terms reported in Table 3.

[33] Simulations set all explanatory variables at their mean, and varies crisis, trade, and the interaction between the two.

[34] All the results reported in this table get stronger when the control variables are dropped from the analysis and when using the binary indicator of a full transition to democracy as the dependent variable.

[35] I thank Laia Balcells for pointing out this possibility.

[36] The data are from Humphreys (2005).

[37] The Wald test of exogeneity using STATA's *ivprobit* command. If the correlation coefficient (r) is not statistically different from zero, the test suggests that the exclusion restriction is met.

[38] Out-migration after the 1991 Iraqi invasion of Kuwait makes the migration figure for Kuwait an extreme outlier.

[39] Examining the average level of urbanization in authoritarian regimes in different regions from 1960 to 2002 reveals that once I control for ceiling effects, sub-Saharan Africa has the highest urbanization rates of any region save the Middle East and North Africa. Controlling for oil production and ceiling effects, sub-Saharan Africa authoritarian regimes have the highest urbanization rates.

[40] On retreat to the informal economy, see Tripp (1997).

[41] The same results hold if I create a variable measuring urbanization rates over the past 10 years. Using the raw values and not logged values strengthens the results.

[42] Simulations conducted using model 3, Table 9, with all variables set at their mean values except *Post-Cold War*, which is set to one. Of all the models presented in Table 9, I base the simulation on the model with the weakest support for the hypothesis. Simulations of other models would produce stronger substantive results.

[43] See Geddes (1999), Magaloni (2006), Gandhi & Przeworski (2006), Gandhi (2008), Brownlee (2007), Smith (2005), Greene (forthcoming) for important contributions.

[44] This is not just applicable to Mexico. Blaydes (2009), for example, suggests that regime stability in Egypt has a similarly clientalistic basis.

[45] I am grateful to Naunihal Singh for pointing this out to me.

[46] See Wilson & Daly (1985) and Olweus & Low (1988) on the disproportionate incidence of violence among young males.

[47] During the protests following the June 2009 Iranian presidential election, the image of a slain young woman protester became a potent visual symbol of the larger protest movement in Iran.

[48] See Acemoglu & Robinson (2006, Chapter 6).

[49] See also Boix (2003).

[50] Note that when the democratization outcome occurs, revolution is not observed.

[51] As in the previous section, we assume that the poor exit in higher proportion than the rich. See equation (A7).

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