ESSAYS ON EXCHANGE RATES CAPITAL FLOWS AND GROWTH

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Thesis Abstract

My thesis comprises of two parts. The first part consists of two chapters. The first chapter titled "A Model of Real Exchange Rates and Real Consumption Spending with Time Varying Discount Factors" generalizes a 'no-arbitrage' or 'real business cycle' equilibrium model by allowing for different time varying impatience parameters across countries and provides empirical evidence for this model vis-à-vis a restricted one, where impatience parameters are constant. My contribution is to show, based on a generalization of the equilibrium model of exchange rates, that the test equation linking exchange rates to fundamentals should allow for heterogeneity in time preferences across countries and across time, as well as noise-that is the model should not be tested as an exact relation.

The second chapter titled "Capital Flows and Exchange Rates" investigates the empirical relationship between capital flows and nominal exchange rates for five major countries. Recent international finance theory suggests that currencies are influenced by capital flows as much as by current account balances and log-term interest rates. Using Vector Auto Regressions (VAR's) I document the following: Incorporating net cross-border equity flows into linear exchange rate models can improve their in-sample performance. Using net cross-border bond flows, however, has no such effect; (ii) An equity-augmented linear model supports exchange rate predictability and outperforms a random walk in several cases.

The second part of my thesis consists also of two chapters (co-authored with Elias Papaioannou, LBS). In the third chapter titled "Democratization and Growth" we revisit the relationship between democracy and growth. Addressing several drawbacks of previous studies yields new evidence: (i) A permanent democratization results in a positive and significant increase of real per capita GDP growth of approximately one percent. (ii) A J-shaped dynamic pattern emerges, with output costs around the transition, but significantly positive growth gains after democracy's stabilization. The empirical evidence thus validates Hayek's (1960) insight that "the merits of democracy will only come in the long-run".

The fourth chapter titled "What Drives Democracy?" is directly linked to the previous one since it aims to explore further the endogenous formation of democratization. We examine countries that enter our sample as non-democratic to identify the systematic factors that led certain countries to abandon autocracy permanently. This approach stands in contrast to the limited empirical studies

that pool all countries to quantify the correlates of long-run democracy. We document that: (i) In contrast to recent studies that challenge the income-freedom link, a permanent democratization is more likely to occur in wealthy (but not oil-abundant) countries. ii) In line with the liberal hypothesis (Friedman, 1962), economic and political liberalizations appear to be re-enforcing. (iii) Democratic transitions are more likely to occur after an economic crisis. (iv) Beyond economic factors, religion and fractionalization are key determinants of political systems.

"Στέρησις αρχή κινήσεως και μεταβολής"

Αριστοτέλης

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Hard Work for a Short Life!

Gregorios *Dimitriou* Siourounis London, 20/05/2005

Thesis Introduction

The first two chapters of my thesis aim to provide fresh theoretical arguments and empirical evidence for a better understanding of the real and nominal exchange rate determination process.

In Chapter 1, I revisit a recent class of exchange rate models ("equilibrium" or "no arbitrage" models) that relate the real exchange rate to consumers and intertemporal marginal rate of substitution (see, for example, Prakash, Sercu and Uppal (2001)). A fair amount of the literature on exchange rate behavior is based on PPP, with PPP being on the basis of the consumption opportunity set. In contrast, the standard micro-economic equilibrium analysis views relative prices, and thus also exchange rates, as determined not just by consumption opportunity sets, but also by marginal utilities. I use non-linear regression analysis to test a sinusoidal real business cycle model (SRBC) for exchange rates assuming homothetic, state-independent power utility with a non-linearly time varying impatience parameter (time preference parameter); using levels of observed variables and efficient estimation criteria. I document that my model outperforms the generalized real business cycle model (GRBC); in that real spending and time dependent differences in impatience parameters across countries have a significant influence on the real exchange rate. The proposed way of relaxing the assumption of constant impatience parameter might be incorporated into a richer model to tackle problems in empirical asset pricing, like the equity premium puzzle.

In Chapter 2 I investigate the empirical relationship between capital flows and nominal exchange rates. The available empirical evidence on such a link is scarce. I employ a unified empirical model for net capital flows and nominal exchange rates, in which the same forces

that drive the latter also influence a countries' net cross border asset holdings. Through my findings I argue that capital markets can explain certain aspects of foreign exchange markets.

More specifically I document that the in-sample performance of standard linear empirical exchange rate models improves when incorporating net cross-border *equity* flows. Net cross-border *bond* flows, however, are immaterial for exchange rate movements. Positive innovations to home equity returns (relative to foreign markets) are associated with short-run home currency appreciation and equity inflow. Positive shocks to home interest rates (relative to foreign countries) cause currency movements that are not consistent with the long-run interpretation of uncovered interest rate parity (UIP). Furthermore, in several cases, the empirical model outperforms a random walk for out-of-sample forecasting. This is extremely interesting in light of the seminal contribution by Meese and Rogoff (1983). My findings are consistent with the recent international finance literature.

Although exchange rates can be seen as a major independent field of study, it does, however, complement a broader picture: that of understanding the equilibrium institutional, sociopolitical and economic norms that explain the relative economic performance of countries. In this direction I have also explored a new wave of research, which sees institutions as the key driving force of economic development (Hall and Jones, 1999; Acemoglu, Johnson and Robinson, 2001, 2002, 2005; Rodrik, Subramanian and Trebbi, 2004). Within this literaturet many challenging questions on institutions and international growth remain open: For example, exactly which institutions and to what extent matter? What determines institutional structures? What are the major channels through which institutions influence development?

The last two essays of my dissertation shed light on each of these questions. The third essay (Chapter 3) employs a novel framework to reassess the effect of democracy on growth. The fourth essay (Chapter 4) identifies the determinants of recent democratizations and thus offers insights on the determinants of political institutions.

In Chapter 3, titled "*Democratization and Growth*" (which draws on joined work with Elias Papaioannou) we revisit the relationship between democracy and growth. In spite

of a massive theoretical and policy debate on the merits of representative institutions, the empirical growth literature has, so far, failed to detect a systematic and robust direct link. We discuss the conceptual and econometric limitations of previous studies and propose a novel approach that addresses these problems. First, to minimize measurement error, we identify permanent democratization episodes in the last forty years, relying not only on democracy indicators, but also on numerous historical resources and electoral archives. Second, to address the omitted-variables and unobserved country-heterogeneity problems that plague cross-country growth regressions, we employ a difference-in-difference specification. Instead of utilizing the cross-country data variation, we compare post democratization growth rates with growth during the non-democratic period. Third, we use annual data to quantify potentially non-monotonic transition dynamics. Addressing these drawbacks yields new evidence: (*i*) A permanent democratization results in a positive and significant increase of real per capita GDP growth of approximately half to one percent. (*ii*) A J-shaped dynamic pattern emerges, with sizable output costs around the transition, but significantly positive growth gains after democracy's consolidation. (*iii*) Interestingly, when we correct for the potential endogeneity of democratization (specifying a "treatment" first-stage model), the results imply that democratization is particularly beneficial, when the conditions required for its emergence and consolidation are favorable (i.e. when the estimated probabilities of democratization are high, growth accelerates). In line with this interpretation, the panel evidence reveal that countries with relatively high level of human capital gain the most from the political reforms.

In Chapter 4, titled "*What Drives Democratization?*" (which draws on joined work with *Elias Papaioannou*) we reverse the democracy-development link and explore the economic and social factors behind the recent Third Wave of Democratization. Specifically we employ various probabilistic panel models to explore further the endogenous formation of democratization. We examine countries that enter our sample as non-democratic to identify the systematic factors that led certain countries to abandon autocracy permanently. This approach stands in contrast to the (limited) empirical studies that pool all countries (always democratic, transition, and always autocratic) to quantify the correlates of long-run

democracy. This methodology thus better addresses issues arising from reverse causality (democracy causing higher levels of human capital and economic development) and sample selection (in the beginning of the sample the richest countries were already democratic). It also enables us to distinguish between factors that influence the transition versus the consolidation of democracy. We document that: (*i*) In contrast to recent studies that challenge the income-freedom nexus (Przeworski *et al.* 2000; Acemoglu, Johnson, Robinson and Yared, 2005a,b), a permanent democratization is more likely to occur in wealthy (but not oil-abundant) countries. (*ii*) In line with the liberal hypothesis (Friedman, 1962) and recent political economy models (Acemoglu and Robinson, 2005), economic and political liberalizations appear to be re-enforcing. (*iii*) Democratic transitions are more likely to occur after an economic (banking or currency) crisis or an armed conflict. (*iv*) Beyond economic factors, religion and fractionalization are key determinants of political systems. These results are verified and strengthened when we exploit the "quasi-natural experiment" of independence that occurred after the fall of communism, to test democratization theories in former Socialist Republics.

Appendix *A*, titled "*Data Filtering and Impulse Responses under Measurement Error in the Regressors*", provides a spectral density discussion on non-stationarity and data filtering and gives a new analytical toolbox I developed in order to asses the significance of measurement error in the regressors within a Vector Autoregressive framework, which is used in Chapter 2.

Appendix *B*, titled "*A New Democratization Dataset*", gives a detailed analysis of the methodology we followed with Elias in identifying and timing incidents of permanent democratizations that is used in Chapters 3 and 4. This Appendix is of particular importance to economists and political scientists since it provides an all new binary variable that characterizes the political regime of more than 100 countries. This variable can be used to test political science aspects of standard growth regressions and/or asset and trade gravity equations. The Appendix also reports graphs that show the evoluation of growth before and after a democratic transition. Finally Appendix *C* gives a detailed data description of the variables employed in each of the four chapters.

Chapter 1

A Model of Real Exchange Rates and Real Consumption Spending with Time Varying Discount Factors

1.1 Introduction

The study of the determinant of real exhange rates is one of the most researched areas in empirical international economics. Four decades, the main workhorse in the literature is the notion Purchasing Power Parity (PPP). The concesus from this literature is that real exchange rates tend to deviate from this relationship for very long time. This deviation is summarized with the half-life, which has been shown to range between 3-5 years.[1] This finding led Rogoff (1996) to argue that such persistent deviations are a puzzle. A range of studies support this view, including those that use panel techniques (Lothian (1997), for example) or long spans of data (see among others Taylor and Sarno (1998)). Studies that focus on non-linearities, however, note that half-lives might be much smaller (for example, Taylor, Peel and Sarno (2001)).

A recent class of exchange rate models ("equilibrium" or "no arbitrage" models) relates

[1] A notable exemption is the recent paper from Imbs, Mumtaz, Ravn and Rey (2005). This paper argues that aggregate real exchange rates are persistent because their components exhibit heterogeneous dynamics, that established time series and panel techniques fail to account for. They show that when these heterogeneities are taken into account half-lives fall to approximately one year. These estimates are consistent with plausible nominal rigidities.

the real exchange rate to consumption, real sector frictions and intertemporal marginal rates of substitution (see for example Head, Mattina and Smith (2002), MacDonald and Ricci (2002) and Apte, Sercu and Uppal (2002)). The main objective of this study is to extend such models by allowing for time varying discount factors modelled as functions of observed economic fundamentals. We obtain this by allowing the impatience parameter not only to be different across countries but also across time and we develop a new non-linear econometric technique to add fresh empirical evidence on the relation between real exchange rates and consumption growth. We document that the half-lifes of deviations of real exchange rates from the new model can be as low as one-to-two months.

Equilibrium models of the exchange rate show that, under specific assumptions, the real exchange rate is related to the ratio of marginal utilities of real spending. Lucas (1982), for example, considers an endowment economy with perfect markets and studies real exchange rates in an equilibrium framework. Dumas (1992) extends this framework in a one-good production economy where international trade of goods is costly and agents are characterized by identical risk aversion and time preferences parameters. Backus and Smith (1993) depart from trade costs and derive real exchange rates in a model with one perfectly tradable and one non-tradable good with agents characterized by constant elasticity of substitution preferences over these two goods. Similar in methodology are also monetary equilibrium models of the nominal exchange rate. Basak and Gallmeyer (1999), for example, examine a continues-time two country monetary equilibrium in which countries can have heterogeneous tastes and endowments with money in the utility function. They provide no-arbitrage pricing results for nominal exchange rates when agents-countries have general time-additive preferences and non-Markovian exogenous price processes.

These models, however, do not relate exchange rates to real observables. Moreover, solutions are those of a central planner since these models do not consider market failures. In our model the commodity market imperfections need not be of a particular type because their effect is already reflected in the consumption set. In our general formulation, we do not need to make restrictive assumptions about the utility functions or distributions. The main assumption is that financial markets are complete. Backus, Foresi and Telmer (2001),

generalize the Lucas (1982) model by linking nominal exchange rates to the marginal utility of nominal spending. This makes the formulae immune to the implicit assumption in all microfounded real exchange rate models with more than one good, that utility function is homothetic. This result relies on the notion of no-arbitrage in frictionless and complete financial markets. It implies that this class of models are robust to any real economy market failures as long as they do not affect the role of marginal utility as the asset pricing kernel. Within this framework, Apte, Sercu and Uppal (2001) (ASU hereafter) provide a general first-order decomposition of the difference of the nominal exchange rate for a wide range of (state-dependent) utility functions. They consider a variety of economic settings and analyze the link between the equilibrium model and the PPP hypothesis.

All these models, however, are based on the standard discounted utility model paradigm as founded by Samuelson (1937) and further elaborated by Koopmans (1960) who showed that such utility specifications can arise from a plausible set of axioms. The main feature of these models is the notion of constant time discounting of future utility.[2] Extensive empirical literature for the last twenty years has shown that constant discount utility is not plausible in many respects. Frederick, Loenestein and O'Donoghue (2002) offer excellent critical reviews to all the anomalies that violate the constant discounting paradigm and solutions pproposed in the literature.[3] Many models have been proposed to overcome such anomalies in the consumption literature. Hyperbolic discount factors a la Laibson (see Laibson 1994, 1997, 1998) have been used extensively in Economics in a variety of models to explain standard stylized facts such us the comovement of income and consumption, the existence of asset specific propensities to consume or reductions in consumption during retirement (see, among others, Angeletos et al (2001) and Diamond and Koszegi (2003)). In the literature connecting exchange rates and consumption spending we are not aware of any

[2]I will use time discounting and time preferences inerchangeably to denote time preferences. Note, however, that these two need not be the same and most of the times are not the same. Time discounting includes any reason why an agents might care less for any future consequence, whereas time preference is more specific and refers to the prefer for immediate relative to later consumption.

[3]Some of them are: the 'sign effect' (gains are discounted more than loses); the 'magnitude effect' (small outcomes are discounted more than large ones); hyperbolic dicounting; the 'delay-sppedup' assymetry; preference for improving sequences and other. Please see Frederick, Loenestein and O'Donoghue (2002) for a full list of references.

study that considers such forms of discount factors to explain real exchange rate fluctuations. Time varying preferences offer a plausible direction to extend the current framework since they confront with agent's intra-temporal heterogeneity regarding choices in different times in the future and can potentially solve the disconnect between slow moving consumption spending patterns and volatile real exchange rates. I choose to model discount factors as non-linear functions.[4] This approach has potentially important implications since the non-linear time-varying discount factor implies some intra-temporal fluctuations for consumption-investment choices, which can make consumption paths more volatile than those emerging from the standard paradigm.

The time preference need not be completely exogenous to observables. Many authors have shown that observed consumption patterns can help us estimate discount functions from observed consumption levels (see for example Koopmans, Diamond and Williamson (1964). Recent empirical literature uses the notion of implied utility from consumption-based choices to derive estimates for discount functions (see for example Laibson, Repetto and Tobacman (2004)). This method of estimating discount factors is not unique. A variety of methodologies have been proposed in the literature. The most commonly used is the one of confounding factors where the researcher observes an intertemporal choice and she then computes the discount rate that this preference implies. Others include consumption reallocation constrains, intertemporal arbitrage and habit formation (for a full list of such techniques and reference see Frederick, Loenestein and O'Donoghue (2002)). Most of these techniques are appplied in the micro-level in some controlled experimental environment and cannot be extended easily in the country-level. I choose to model discount functions as sinusoidal, depending on the observed country-specific consumption levels. I then insert these functions in the equation for the real exchange rate and estimate the factor loadings associated with consumption expenditures and prices. I show that his model outperforms the previous equilibrium model specifications and offers new insights for the explanation of real exchange rate fluctuations.

The empirical literature in PPP is abundant. Test of its validity use a variety of cur-

[4]Rubinstein (2000) offers a persuasive critic on hyperbolic discounting.

rencies, frequencies and data sets. Abuaf and Jorion (1990) use Dickey and Fuller tests to establish the presence of mean reversion in real exchange rates, a phenomenon not evident from autocorrelation techniques, in an attempt to find long-run relations between exchange rates and relative price levels. Frankel and Rose (1996), Froot, Kim and Rogoff (1995), Lothian and Taylor (1996), and Wei and Parsley (1995) have used expanded data sets and a variety of different techniques to offer mixed results for the validity of PPP. A review of the empirical literature can be found in Froot and Rogoff (1995).

Empirical tests of the equilibrium model are scarce and with colficting results. Backus and Smith (1993) test, and reject, the hypothesis that the real exchange rate is an exact loglinear function of the real-consumption ratio, by comparing the means, standard deviations, and autocorrelations of changes in the two time series. Koedijk, Nissen, Schotman and Wolff (1996), in which they cross sectionally, relate changes in exchange rates and find no significant link. Koedijk et al. (1996) find evidence consistent with PPP when they study paired data at the integrated level, but again, no traces of any consumption effect. Sercu and Uppal (200) use also levels of the data and find support for the existence of a long-term relation between nominal exchange rates and real consumption. These authors, however, do not show if the estimated cointegration vector (long-run attractor) is consistent with an equilibrium model of real exchnage rates. ASU estimate tha magnitude and the sign of the coefficients to show that as a long-run relation, the equilibrium model outperforms PPP marginally. More importantly yhey show that deviations of the real exchange rate from teh equilibrium model have half-lives of only four to five quarters. In our empirical tests we show that the extendent equilibrium model outperforms clearly the PPP and the simple equilibrium model and that half-lives are reduced even further to thre to four quarters.

More specificly, the present paper shows, based on a generalization of the equilibrium model of exchange rates, that (i) the relationship between exchange rates and fundamentals should allow for heterogeneity in time preferences across countries and across time; (ii) empirical work should use levels of variables rather than first differences; (iii) tests should be used for the sign of the estimated coefficients; (iv) the specification of the regression should offer demonstrated advantages over alternatives, and the significance tests should not rely on

asymptotic results; and (v) the tests should not use countries where severe capital restrictions or exchange rate controls have imposed, thus violating the integrated-market assumption of the model. We show that when accounting for non-linear time varying impatience parameters across countries the 'Sinusoidal Real Business Cycle' (SRBC hereafter) model outperforms the GRBC model offering new evidence on the relation between real exchange rates and real consumption spending.

In section 1.2 we develop the model to be used for the empirical exercise. Section 1.3 presents the econometric specification. Section 1.4 presents the data. Section 1.5 contains the empirical analysis and reports the results. Finally, section 1.6 concludes.

1.2 The Economy

We consider a world economy that consists of $M \geqslant 2$ countries. We focus on two arbitrary countries that are referred to as the home country (subscript $k = 1$) and the foreign country (subscript $k = 2$). Each country has a representative consumer with a standard, strictly quasi-concave utility function defined over $N \geqslant 2$ goods. Across countries these representative agents may differ in terms of risk aversion, consumption preferences, and initial wealth. Uppal and Sercu (2000) extend the Backus et al. (1996) model to establish that the nominal exchange rate equals the ratio of the marginal indirect utility of total nominal spending. That is, if country k's marginal utility is denoted by $\Lambda_k(t)$, $k = \{1, 2\}$ then:

$$S(t) = \frac{\Lambda_2(t)}{\Lambda_1(t)} \tag{1.1}$$

In general, the marginal utility of nominal spending in country k, at date t, is an indirect one, derived from the (static) problem of a consumer who faces a vector of prices for the N goods, $p_k(t)$, and who allocates a budget of $C_k(t)$ over the consumption of these goods, , in order to maximize, $U_k(c_k(t)), X_k(t), t)$, where $X_k(t)$ is a vector of (possibly country-specific) state variables that affect utility.[5] This problem can be written as:

[5]The optimal level of Ck(t), itself, can be obtained by solving the intertemporal problem of the consumer.

$$V(C_k(t), p_k(t), X_k(t), t) \equiv$$

$$\equiv \max \left\{ U_k(c_k(t)), X_k(t), t) - \Lambda_k(t) \left[\sum_{j=1}^{N} c_{kj}(t) p_{kj}(t) - M_k(t) \right] \right\} \tag{1.2}$$

where $V(C_k(t),\ p_k(t),\ X_k(t), t)$ refers to the period-t indirect utility function of total spending given prices; $U_k(c_k(t),\ X_k(t), t)$ denotes the utility function of the representative investor in country k and implicitly includes the discounting for time $M_k(t)$ is the nominal consumption spending in country k. Thus, the marginal indirect utility of nominal spending in country k is the multiplier in the above optimization problem given by:

$$\Lambda_k(t) = \frac{\partial V(M_k(t), p_k(t), t)}{\partial M_k(t)} \tag{1.3}$$

Let $\Pi_k(t)$ denote the consumption price level in country k at time t, θ_k the central planers welfare weight, η_k the measure of relative risk aversion of the representative investor, and δ_κ the constant impatience parameter. In addition we assume perfect, arbitrage-free capital markets, and a representative consumer with constant relative risk aversion (CRRA) utility functions and homothetic preferences of the form:

$$\Phi_k(u_k(t)) = \begin{cases} \frac{u_k(t)^{1-\eta_k}}{1-\eta_k}, & \text{for } \eta_k \neq 1 \\ \ln\left[u_k(t)\right], & \text{for } \eta_k = 1 \end{cases} \tag{1.4}$$

Then combining equations (1.1), (1.3) and (1.4), and rearranging we obtain that the real exchange rate is given by:

$$s(t)\frac{\Pi_2(t)}{\Pi_1(t)} = \theta_2 \frac{\delta_2^t}{\delta_1^t} \frac{(1-\eta_2)\left[\frac{M_1(t)}{\Pi_1(t)}\right]^{\eta_1}}{(1-\eta_1)\left[\frac{M_2(t)}{\Pi_2(t)}\right]^{\eta_2}} \tag{1.5}$$

when $\eta \neq 1$. In the log utility case ($\eta = 1$) real exchange rate is given by:

$$s(t) = \theta_2 \frac{\delta_2^t}{\delta_1^t} \frac{M_1(t)}{M_2(t)} \tag{1.6}$$

Equations (1.5) and (1.6) encompass many existing models of exchange rate, which have typically been derived in settings with one or two goods (when one good is tradable at a cost) and constant relative risk aversion.[6]

Note that the prediction is that higher real spending abroad lowers the real value of the foreign currency. Expression (1.4) assumes that the discount factor is constant and that agents are impetient. Many recent theories, however, allow for time varying intertemporal marginal rates of substitution. That is, if $c = (c_0, c_1, ..., c_t, ...)$ is a non-zero consumption stream, then the forward shifted consumption stream $c' = (0, c_0, c_1, ..., c_{t-1}, ...)$ is strictly worse than c. Agents may, for example, be more optimistic in good times and the opposite in bad times. This would induce heterogeneity in δ's across time as well as across countries. If we assume that the representative consumer maximizes the expected discounted sum of future utilities but her time preference is itself a deterministic time varying function then she maximizes the following expression:

$$\sum_t^T [\delta_k(t)]^t \, \Phi_k(u_k(t)) \tag{1.7}$$

Where $\Phi_k(u_k(t))$ is given by (1.4) and the impatience parameter is a deterministic non-linear country specific function of time, denoted $[\delta_k(t)]^t$, which depends on the current state of the economy.[7] Since δ is given by a deterministic function then (1.5) reads:

$$s(t)\frac{\Pi_2(t)}{\Pi_1(t)} = \theta_2 \frac{[\delta_2(t)]^t}{[\delta_1(t)]^t} \frac{(1-\eta_2)\left[\frac{M_1(t)}{\Pi_1(t)}\right]^{\eta_1}}{(1-\eta_1)\left[\frac{M_2(t)}{\Pi_2(t)}\right]^{\eta_2}} \tag{1.8}$$

In the present study we aim to estimate the expression above and evaluate the significance of relaxing the constant a_t parameter. So far on the empirical front studies are trying to establish relationships between consumption and exchange rates. Koedijk et al. (1996)

[6] See for example Sercu et. al. (1995), Backus and Smith (1993) and Stulz (1987).

[7] The requirement for the impatience parameter is that it takes values less than one so that (1.7) exists

complement their first-pass regression on differenced data by a thorough analysis, which offers more power provided that there is a long–run relation between the levels of variables included in the analysis. Kollmann (1995) and ASU also apply such cointegration tests. While Kollmann (1995) finds no relation between real consumption spending and real exchange rates, ASU do and they also demonstrate that their model outperforms the nested PPP model. We next show next that our specification bits the ASU model not only in terms of significance of coefficients but also by estimating correctly theoretically predicted signs. That is, while, the SRBC model does not give numerical values for the η and δ coefficients, we do know that the relative risk aversion should be positive, the impatience parameter less than one and that the theory implies a non-linear relation between real exchange rates and real consumption spending.[8]

1.3 Empirical Specification

We propose a new method to model the non-linear time varying impatience parameter which is deterministic but still can be estimated using the business cycle properties of the observed macroeconomic data. Following Enders and Lundlow (2001) a simple modification of the autoregressive framework allows the intercept term to be a time-dependent function, denoted $a(t)$. If we temporarily abstract from explanatory variables and denote the series of interest by $\{y_t\}$, we can write:

$$y_t = a(t) + \varepsilon(t) \tag{1.9}$$

Where: $\epsilon(t)$ is a white noise disturbance with variance σ^2, and $a(t)$ is a deterministic function of time, t. Since $E_{t-1}y_t = a(t)$, the nature of equation (1.9) is such that the conditional mean of y_t is allowed to vary over time. However, (1.9) does not attempt to explain the reason that the conditional mean changes over time. The change in the conditional mean of y_t can be due to the presence of an unobserved explanatory variable or a structural

[8] See also Taylor (2001) for the non-linear properties of real exchange rates.

break. Although $a(t)$ is unknown, if it is an absolute integrable function, under very weak conditions , (1.9) can be exactly represented by a sufficiently long Fourier series of the form:

$$a(t) = A_0 + \sum_{k=1}^{s} \left[A_k \sin \frac{2\pi k}{T} t + B_k \cos \frac{2\pi k}{T} t \right] \tag{1.10}$$

In order to keep the problem tractable we consider a single frequency for the intercept term such that (9) reads:

$$a(t) = a_0 + a_1 \sin \frac{2\pi k}{T} t + b_1 \cos \frac{2\pi k}{T} t \tag{1.11}$$

where k is an integer in the interval 1 to $T/2$. The interpretation of (10) is that any fluctuations in the intercept term will be captured by nonzero values for a_1 and b_1.

The key point in using (10) is that the behavior of any deterministic sequence can be readily captured by a sinusoidal function even if the sequence in question is not periodic. As such, missing explanatory variables can be represented by a deterministic time-dependent coefficient model without first specifying the actual variables in question. The nature of the approximation is such that the standard constant intercept term emerges as a special case when all A's and B's are zero. The problem becomes one of finding the most appropriate values for a_0, a_1, b_1 and k. A limited number of studies have used this new methodology. Lundlow and Enders (2001) show that in contrast to the Dickey-Fuller test, the new method implies that the real French/German exchange rate, and US inflation as measured by the CPI, are mean reverting. Also, Enders and Hoover (2001) study the effects of robust growth on poverty to find that a Fourier approximation provides a better empirical model of poverty than the standard linear model. Enders and Sandler (2001) apply a Fourier approximation to get improved estimates for an alternative method that does not rely on pre-specified interventions for identifying significant policy and political impacts in the time series of transnational terrorism during 1970-1999.

1.4 Data

Since we test a model that assumes perfect capital mobility, we have to restrict ourselves to countries that have the minimum possible capital frictions and they are large in the sense that their currencies dominate the foreign exchange market offering enough liquidity.

Thus we choose the US, the UK despite a dual exchange rate in the early eighties; Germany; Japan despite its exchange controls prior to 1982; and Switzerland, a mainstream non-ERM economy with unrestricted capital flows. The data consists of quarterly consumption spending, CPI data in the last month, and end-of-quarter exchange rate data taken from the IFS CD-ROM, for the United States (US), Germany (GE), Japan (JA), United Kingdom (UK) and Switzerland (SW) starting with the first quarter of 1973 and ending in the first quarter of 2004 . We use the USD as the reference currency, currency 1 in the theoretical model, and convert all exchange rates into USD. All variables are in log since a Cox transformation of order 1 was found superior to levels. Table 1.1 gives a summary of all varibales.

1.5 Empirical Estimation and Results

The modified SRBC model predicts that the impatience parameter is given by a Fourier approximation of the form of equation (1.11). The intuition is that agents are assumed to discount the future differently at every point in time according to their observation of the level of economic fundamentals. Next, our objective is to evaluate the SRBC model, that is the Prakash-Uppal-Sercu equation extended so as to allow not only for noise, and different risk aversion parameters but also for country specific and time varying impatience parameters. Taking *logs* in equation (1.8) one obtains:

$$RATE(t) = \Sigma(t)t + B_{us}LC_{us} + B_kLC_k + \varepsilon(t) \tag{1.12}$$

Here $RATE, LC_{us}, LC_k$ and Σ are given by:

$$RATE(t) = \log\left[S(t)\frac{\Pi_{us}(t)}{\Pi_k(t)}\right], LC_{us} = \log\left[\frac{M_{us}(t)}{\Pi_{us}(t)}\right],$$

$$LC_k = \log\left[\frac{M_k(t)}{\Pi_k(t)}\right], \Sigma(t) = \log\left[\frac{\delta_k(t)}{\delta_{us}(t)}\right]$$

that follows a deterministic sinusoidal function of the form of (1.11) such that

$$\Sigma(t) = a_0 + a_1 \sin\frac{2\pi k}{T}t + b_1 \cos\frac{2\pi k}{T}t + \varepsilon(t)$$

with $a_0 = \log\left[\theta_k \frac{1-\eta_k}{1-\eta_{us}}\right]$ and $\varepsilon(t)$ an error process. Table 1.2 reports a summary of the constructed variables whereas Table 1.3 the corresponding cross-correlation matrix.

Note that the ASU model arises as a special case of our model when $\Sigma(t)$ is a constant, and the PPP model arises when $\Sigma(t)$ is equal to one and all B's insignificant. The interpretation of (1.12) is that the magnitudes of any fluctuations in the intercept term are captured by non-zero values of a_1, b_1. The frequency of the fluctuations in the intercept term is given by k.

If the frequency was known, one could simply construct the variables $sin(2\pi kt/T)$ and $cos(2\pi kt/T)$ and estimate (1.12) using Ordinary Least Squares (OLS). Unfortunately, the issue is quite complicated since the relevant frequency is unknown. Thus, for values of k in the interval 0 to π, using increments of $1/T$, we use a two step procedure. In the first step we estimate with OLS the following equation:

$$RATE(t) = a_j \sin\frac{2\pi k}{T}t + b_j \cos\frac{2\pi k}{T}t \tag{1.13}$$

The value of k yielding the smallest residual sum of squares is called k^* and the coefficients associated with that frequency are called a^* and b^*. Next we calculate $sin(2\pi k^* t/T)$ and $cos(2\pi k^* t/T)$ and estimate (1.12) in one step estimation by applying Non Linear Least Squares ($NLLS$) to obtain the results reported in Table 1.4.

On the left hand side we report the SRBC model and the relevant coefficient estimates. On the right hand side we report the coefficient estimates for the GRBC model. It is apparent that the explanatory variables in the linear model (GRBC) fail to capture changes in real exchange rate due to changes in the foreign consumption spending in all four cases. The linear model is a «poor average» of the estimated path of $\alpha(t)$. The sinusoidal model tracks the series much better that the linear one based on the Akaike Information Criterion (AIC) as well as the Schwartz Bayesian Criterion (SBC), which trade off a reduction in the sum of squared residuals for a more parsimonious model . For Germany, Japan and UK the SRBC model outperforms the GRBC model although the AIC and SBC criteria penalize for the fact that we estimate one parameter more.

The most interesting result is for Germany, Japan and Switzerland, where the previously insignificant coefficient of foreign real spending is now significant. In the case of Japan a 1% increase in the Japanese log real home consumption spending will result in a decrease of the log real YEN/$ by 0.52%. For Germany the result goes in the same direction indicating that a 1% increase in the log real home consumption spending will decrease the log real DM/$ by 0.11%. For the UK, the estimated coefficient for home consumption has the correct sign but is only significant at the 12% significance level, although the SRBC model outperforms the GRBC model in terms of AIC and SBC criteria. For Switzerland, although the GRBC model outperforms the SRBC model, we do get an improved estimate for the coefficient of home consumption but surprisingly with the opposite sign . Overall the model with non-linear time varying impatience parameter outperforms the GRBC model and improves the estimates for foreign (from the US perspective) real consumption spending in three out of four cases. It is also evident that the PPP model is rejected in all four cases from both the GRBC and SBC models. The implication of these results is that micro-founded theoretical models for the real exchange rates should be tested with flexible and rich enough techniques, which allow for nonlinear dynamics in order to distinquish them from simpler, linear models.

Last, in order to expose the model into the standard test of predicting (in-sample) real exchange rates, we reconstruct the in-sample prediction based on the estimated coefficients from the two models. We then calculate the deviation of real exchange rates from each of

the two models. Fitting and AR(1) process into this deviation allows us to calculate the half-lifes for the deviations. Figure 1.1 plots all deviations as well as the estimated half-lifes for all four currencies. Consistent with ASU, deviations of real exchange rates from the GRBC model last between 3-5 quarters. Interestingly, deviations from the SRBC model last between 1 and 2 months. This happens because the nonlinear dynamics imbedded in the preference parameter add considerable volatility in the smooth real consumption spending rations. Econometrically, the effect of such addition is a shift in the spectral domain towards higher frequency components. Given that spectral densities of real exchange rates also have considerable mass in high frequencies, the new model does well in explaining this. This is a significant improvement in the direction of explaining real exchange rates within a microfounded general equilibrium framework. We need to note, however, that all this analysis is in-sample, it extends only until the end of 2000 (due to model restrictions for degrees of freedom) and the sinusoidal function is estimated in-sample. Tests like out-of-sample predictive power, robust estimations to endogeneity issues, and longer data are some of the obvious routes one needs to take in order to establish stability of the above results. We leave all these for future research.

Several drawbacks using this method have to be reported. First, we do not make the impatience parameter an endogenous variable but we model it as a deterministic function of time although we do estimate it from the observed data. Second, the sample size is small for the purposes of revealing much of the dynamics that are hidden in higher frequency data for real exchange rates, as pointed out by Taylor (2001). Third, the empirical observation of a long-run relationship between consumption spending and real exchange rates is not yet clear enough to discard well-documented theoretical and empirical regularities associated with PPP as a workhorse for explaining real exchange rates.

1.6 Conclusions

A fair amount of the literature on exchange rate behavior is based on PPP, with PPP being on the basis of the consumption opportunity set. In contrast, the standard micro-economic

equilibrium analysis views relative prices, and thus also exchange rates, as determined not just by consumption opportunity sets, but also by marginal utilities. We use non-linear regression analysis to test a sinusoidal real business cycle model (SRBC) for exchange rates assuming homothetic, state-independent power utility with a non-linearly time varying impatience parameter (time preference parameter); using levels of observed variables and efficient estimation criteria. We found that our model outperforms the generalized real business cycle model (GRBC); in that real spending and time dependent differences in impatience parameters across countries have a significant influence on the real exchange rate. We aslo show that half-lifes of deviations of real exchange rates from the new model can be as low as one-to-two months. Future research could elaborate the method used here to accommodate problems resulting from the low frequency of the data and the deterministic nature of the non-linear process for the impatience parameter, to reveal a stronger (or to prove a poorer) relationship between real consumption spending and real rates. The proposed way of relaxing the assumption of constant impatience parameter might be incorporated into a richer model to tackle problems in empirical asset pricing, like the equity premium puzzle.

Chapter 2

Capital Flows and Exchange Rates: An Empirical Analysis

2.1 Introduction

Explaining nominal exchange rate movements is one of the most intriguing themes in international macroeconomics. The empirical relationship between capital flows and nominal exchange rates, in particular, is important. Dornbush (1976, p. 1166), states that *'the exchange rate adjusts instantaneously to clear the asset market'*. The available empirical evidence on such a link, however, is scarce. In this paper, we employ a unified empirical model for net capital flows and nominal exchange rates, in which the same forces that drive the latter also influence a countries' net cross border asset holdings. Through our findings we argue that capital markets can explain certain aspects of foreign exchange markets.[1]

This study documents that the in-sample performance of standard linear empirical exchange rate models improves when incorporating net cross-border *equity* flows. Net cross-border *bond* flows, however, are immaterial for exchange rate movements. Positive innovations to home equity returns (relative to foreign markets) are associated with short-run home currency appreciation and equity inflow. Positive shocks to home interest rates (relative to foreign countries) cause currency movements that are not consistent with the long-run

[1]In 1975 cross border transactions in bonds and equitys for U.S. were only 4% of GDP. In 1990's went up to 100% and in 2000 reached 245% (source USTO). See also Portes and Rey (2002) and Portes, Rey and Oh (2001), who document that cross border capital flows have increased sizably in the last decade.

interpretation of uncovered interest rate parity (UIP). Furthermore, in several cases, the empirical model outperforms a random walk for out-of-sample forecasting. This is extremely interesting in light of the seminal contribution by Meese and Rogoff (1983). Our findings are consistent with the recent international finance literature.[2] Our empirical methodology deviates, however, from it in some important dimensions: We use unrestricted VARs and treating all variables as endogenous; (i) we formally test which asset flow affects monthly exchange rates; (ii) a structural shock to net capital flows has a dynamic effect on exchange rates. We study the significance of such effects; (iii) We assess theoretical implications for the dynamic cross-correlations of exchange rates and net equity flows controlling for both equity return differentials and interest rate differentials. Ideally, one would identify the shocks based on a structural approach. There are, however, no theoretical results strong enough for such identification. At best, models have implications for the contemporaneous correlation structure between exchange rates, capital flows and equity return differentials but not the dynamic interactions. For robustness, we also perform the analysis with the Canova and De Nicolo (2002) methodology that does not use zero restrictions.

Specifically, we examine the bilateral exchange rate of the U.S. dollar defined against the pound, yen, mark and Swiss-franc while introducing bilateral net cross-border capital flows into a standard linear framework.[3] Although, we do not perform a formal test for a specific theoretical model, we do empirically evaluate the following theoretical hypothesis:

H1: Positive shocks to home equity flows are associated with a home currency appreciation. Hau and Rey (2002) show that if a price inelastic supply of currency balances and equity exists, as a consequence of limited arbitrage in foreign exchange and equity markets, then equity flow innovations affect positively the demand for currency and equity.

H2: Higher foreign equity market returns, relative to the home market, are

[2]See the recent work from Hau and Rey (2002), which model explicitly capital flows, exchange rates and equity returns in a unified framework to provide microfoundations to the portfolio balance theory. Bailey, Millard and Wells (2001) provide a model that predicts capital flows due to interest rate differencials generated from productivity shocks whereas Pavlova and Rigobon (2003) provide a model for optimal dynamic portfolio equity holdings. In the last two models, however, exchange rate determination is based on relative good prices and not equity flows.

[3]These are the top 5 currencies as regards the daily turnover of all pairs worldwide (see also Table 1).

associated with a home currency appreciation. Hau and Rey (2002) show that positive equity returns to the foreign market induce portfolio rebalancing due to higher forex risk exposure in the foreign market, which initiates both home equity inflows and foreign currency depreciation.

Furthermore, we assess whether bond flows impact exchange rates differently than equity flows and whether equity-augmented linear models have predictive power for exchange rate movements. We also test whether net purchases of foreign assets behave differently from net purchases of U.S. assets.

Our main findings are as follows: First, net U.S. equity purchases have a stable and consistent impact on the British, German and Swiss currencies (consistent with **H1**). Net purchases of U.S. or foreign bonds, however, are irrelevant for exchange rate movements, most likely due to exchange rate hedging.[4] For the dollar/pound, dollar/Swiss-franc and the dollar/mark, a one standard deviation positive shock in net U.S equity purchases is associated with a 10% appreciation of the dollar. This is statistically significant for 13 months on average. The evidence for Japan contradicts theoretical predictions since net purchases of U.S. assets from Japanese residents are associated with a strong yen.

Second, conditional on capital flows, positive innovations to foreign equity returns (relative to U.S.) are associated with short-run U.S. currency appreciation and equity inflow in three out of four countries (consistent with **H2**). This channel, however, is not robust to the identification of the VAR. Positive shocks to U.S. interest rates (relative to foreign countries) cause U.S. currency appreciation not consistent with uncovered interest rate parity (UIP).

Third, dynamic forecasts from an equity augmented-VAR support exchange rate predictability and outperform both a random walk and a standard VAR that includes only exchange rates and interest rate differentials. Forecast performance is evaluated with the root mean square error criterion and the Diebold and Marianno (1995) statistics. However, the specification that produces such superior forecast performance depends on the exchange

[4] A survey of proprietary data from a Fund of Funds that (investing in more than 200 Funds around the globe every year for the last 10 years) shows that cross border bond holdings are heavily hedged (more than 90%) whereas equity holdings are not (less than 12%). For a first documentation see Hau and Rey (2002).

rate and the forecast horizon. These findings are robust to a number of specifications, including identification and lag length of the VAR, structural breaks, data mis-reporting and measurement error in the regressors.

Traditional exchange rate models based on macroeconomic fundamentals lead to poor in-sample performance for floating exchange rates.[5] Additionally, a random walk forecast generally outperforms models based on purchasing power parity (PPP), uncovered interest rate parity (UIP), and simple versions of the monetary and portfolio balance models of exchange rates (Meese and Rogoff (1983)). This finding has remained unchanged for horizons shorter than four to five years. Frankel and Rose (1995) therefore note that *"[t]he dispiring conclusion is that relatively little explanatory power is found in models of the exchange rate with traditional observable macroeconomic fundamentals, whether based on the monetary or portfolio-balance models."* This paper shows that for certain OECD countries exchange rate movements can be partially explained by net equity flows.

Our work is closer to the microstructure approach. Investor order flows cause exchange rate changes through private information which, when released, permanently impacts exchange rates. Evans and Lyons (2002) find that daily inter-dealer order flow explains an astonishing 60% of daily exchange rate changes. Consequently, they argue that such flows are a proximate cause of exchange rate movements. Rime (2001) finds that weekly flows help explain exchange rate movements. Within the portfolio rebalancing workhorse and conditional on any exogenous quity return and exchange rates shocks, it is possible that changes in net capital flows and order flows are interlinked. Conditional on an exogenous appreciation of her foreign portfolio value, for example, the domestic investor is likely to initiate the selling of foreign assets as well as the selling of foreign currency balances. This will result in a parallel movement to the currency and assets market. Wei and Kim (1997), for example, and Cai, Cheung, Lee and Melvin (2001) find that large trader positions explain currency volatility far better than do news announcements or fundamentals. Froot and Ramadorai (2002) conclude that investor flows are important for understanding exchange rate deviations from fundamentals, but not for understanding long-run currency values. These studies,

[5]For a good survey see Frankel and Rose (1995).

however, do not consider asset trades or any dynamics both of which we examine. We also use publicly available data, which makes the results easily replicable. The capital flow data is obtained from the U.S. Department of Treasury, and consists of monthly observations over 1988-2000.

We know of three papers that are similar in spirit to our work. Hau and Rey (2002) find that for daily, weekly and monthly data for 17 OECD countries, equity flows have become increasingly important over time and correlate strongly with exchange rates.[6] Brooks, Edison, Kumar and Slok (2001) estimate OLS regressions using yearly bilateral flows for three major currencies and show that equity flows correlate weakly with the euro/dollar exchange rate. Lastly, Pavlova and Rigobon (2003) estimate a structural model and show that demand shocks, associated with increased equity returns and capital inflows, correlate strongly with nominal exchange rates.

The essay proceeds as follows. Section 2.2 reviews structural exchange rate models whereas Section 2.3 makes the case for the use of capital flows in exlapining forex dynamcs. Sction 2.4 describes the data and our methodology. Section 2.5 reports the results from simple OLS regressions and from VAR analysis. Section 2.6 presents sensitivity analysis. Section 2.7 concludes.

2.2 Some theory and empirics on exchange rates and fundamentals

Three decades of active research has produced numerous structural models of exchange rate determination, linear or non-linear, market microstructure or macro fundamentals-based.[7] In this section, we review several classic models of exchange rate determination that have been used extensively in economics. The simplest model of equilibrium exchange rate de-

[6]Hau and Rey (2004), in a contemporaneous and independent study, use structural VAR's to reach in similar results regarding the movements of equity returns, exchange rates and net equity flows. Our study is reacher in the sense that investigates which flow is relevant for exchange rate movements (bonds or equities), we offer an extensive sensitivity analysis, we check if the results are robust using both theoretically based identified and unrestricted VAR's and we condition on UIP.

[7]For a survey see the last issue of the Journal of International Economics.

termination is based on the concepts of purchasing power parity. Whether reflecting the idea that the law of one price holds in traded goods or the Casselian view that the value of a currency should reflect its relative purchasing power, the concept of PPP is central to international economics.[8] Abstracting from transport costs, risk premia, and other factors, a standard specification is:

$$s_t = a + p_t^* - p_t + \xi_t \tag{2.1}$$

All variables are in logs and s_t is the foreign currency price of a unit of home currency so a larger number means an depreciation of the home currency, p and $p*$ are home and foreign CPI-price levels respectively, and ξ_t represents an error process. The monetary models build on PPP by imposing additional structural restrictions and treating non-money assets as perfect substitutes. One such additional assumption is money market equilibrium, which states broadly that the log real money demand depends linearly on the log real income and the nominal interest rate:

$$m_t - p_t = \gamma y_t - \delta i_t + \xi_t \tag{2.2}$$

Combining this equation with equation[9] (1) log nominal exchange rate results to a positive relation with the relative money stocks and the nominal interest rate differentials and a negative relation with the relative income levels between the two countries:

$$s_t = \alpha + (m_t^* - m_t) - \gamma(y_t^* - y_t) + \delta(i_t^* - i_t) + \xi_t \tag{2.3}$$

This form of monetary equations was initially considered by Mussa (1976) and Frankel (1976), and more recently by MacDonald and Taylor (1994) and Flood and Rose (1995). They entail that an increase in expected inflation (captured by interest rate differentials) at home, agents will switch from domestic currency into bond holdings, inducing a deprecia-

[8] See, for example, recent evidence on the validity of relative PPP (Imbs, Mumtaz, Ravn and Rey (2005)).

[9] Assuming the same equation holds for the foreign country and equal income and interest rate elasticities of money demand.

tion of the home currency. Early empirical tests failed to establish a relationship between exchange rates and fundamentals.[10]

The next class of models we consider is the asset approach or portfolio balance models. These models brought together aspects from economics and finance. They stated that the exchange rate is determined by the supply and demand of all foreign and domestic assets. These models are particularly useful when someone is interested in understanding the effects of changes in the supplies of assets on exchange rates' levels (see also Lyons (2001), pp. 162). They predict that purchases of foreign assets drive up the price of the foreign currency. Assuming that domestic and foreign assets are perfect substitutes, international capital markets equilibrium reads through the uncovered interest rate parity (UIP hereafter):

$$i_t^* - i_t = E_t(s_{t+1} - s_t) \tag{2.4}$$

Incorporating the UIP into the flexible price monetary model above gives us a first-order stochastic difference equation for the nominal exchange rate, whose current value embodies its expected future values (and thus future development of fundamentals). Solving for the rational expectation no-bubbles solution, we can express exchange rate as the expected present-value of relative money stock and relative real income, a model emphasized in several important recent papers such as Mark (1995) and Kilian (1999), among others. It is then common to assume the two sets of fundamentals to follow a driftless random walk, leading to the following reduced-form equation for exchange rates:

$$s_t = \alpha + (m_t^* - m_t) - \gamma(y_t^* - y_t) + \xi_t \tag{2.5}$$

Lyons (2001) notes that for estimating this models from real world data, more structure is added to both the demand and supply sides. Even as an account of changes these models do not fare well empirically (see Branson and Henderson (1985), Lewis (1988)). Dornbusch (1976) showed that in the presence of short-run price stickiness, purchasing power parity

[10]Meese and Rogof (1983a and 1983b) showed that most of the structural models of the first generation could not outperform the random walk model in out of sample predictions.

condition would be violated temporarily, and the relation between interest rates and the exchange rate needs to capture the short-term liquidity effects of monetary policy. Frankel (1979) proposes using a real interest rate differential term which consists of a nominal interest rate with short-term maturity, capturing the liquidity effects, and an inflation expectation component that operates through Cagan money demand function. The reduced form equation can be represented as follows.

$$s_t = a + (m_t^* - m_t) - \gamma(y_t^* - y_t) - \delta(i_t^* - i_t) + \eta(\pi_t^* - \pi_t) + \xi_t \tag{2.6}$$

Early empirical tests validated this model (Frankel (1979)), but results begun to collapse when out of sample tests were employed (see Frankel (1983a and b) and Backus (1984)). Cointegration techniques that followed, failed to establish a relationship between exchange rates and interest rates although some relationship seemed to hold in lower frequencies (Baxter (1994)). Recent studies do find some week relation between exchange rates and easily observable fundamentals in short horizons (two to four years). For example, Clarida and Gali (1994), Eichenbaum and Evans (1995), Kim and Rubini (1997), Mark and Sul (2001) and MacDonald and Ricci (2002). Other recent studies emphasize particular market or data characteristics and offer new insites in the exchange rate literature. Notable examples are Clark and McDonald (2000) (behavioural aspects of the market), Chen and Rogoff (2003) (commodity currencies), and Imbs, Mootaz, Ravn and Rey (2005) (aggregation bias in data). However, not only at shorter horizons most exchange rates often appear unrelated to current measures of fundamentals but many researches remain sceptical (Rapach and Wohar (2001b) and Faust, Rogers, and Wright (2003)). The inability of traditional factors to explain the movements in major currency markets led to considerations of alternative explanations. In particular, many market commentators have pointed to differences in relative output and corporate earnings' growth rates between major economies and the United States, and associated capital flows as a possibly important element of the explanation.

2.3 Some theory and empirics on exchange rates and capital flows

Recent empirical work shows a vast increase of capital flows in developed as well as developing countries. Portes and Rey (2002) and Portes, Rey and Oh (2001) document that gross cross border capital flows have increased sizably in the last decade. Changes in cross border net asset holdings may affect the value of a country's currency through multiple channels. In this section, we present a brief discussion on this topic in order to justify why capital flows may be an important additional control in empirical exchange rate equations.

A substantial recent body of work points to the importance of financial factors in understanding exchange rates in emerging as well as in developed economies. Many writers have questioned the neglect of financial market structure in standard macroeconomic models.[11] Devereux and Lane (2003) note:

"...all these papers highlight a fundamental failure of the 'Modigliani–Miller' theorem: balance sheet effects matter for macroeconomic outcomes and especially for the exchange rate."

One conclusion of this literature is that, combined with these balance sheet effects, the presence of external debt (denominated in foreign currency) may have an important effect on the way in which movements in the exchange rate impact on an economy. Moreover, given the significant differences in the determinants of equity flows compared to debt flows, fluctuations in exchange rates, in the presence of large shocks of un-hedged foreign-currency denominated equity may be important through its effects on the financial sector and corporate balance sheets.[12]

As a first justification Hau and Rey (2002) develop a model that analysis the interaction

[11] See, for example, Bernanke et al. (1999) that stress the importance of balance sheet effects in understanding the properties of business cycles. Among others, Krugman (1999), Aghion et al. (2001), Devereux and Lane (2001), Gertler et al. (2001) and Eichengreen (2002), have extended these ideas to the open economy.

[12] See, for example, Bailey, Millard and Wells (2001), Hau and Rey (2002), and Pavlova and Rigobon (2003).

between optimal portfolio choice under incomplete markets and exchange rate dynamics. They document that exposure to exchange rate risk implies that the international investor cares not only for the volatility of the exchange rate but also for the correlation structure of exchange rates and foreign equity returns. Higher exchange rate volatility will induce a home equity bias but a negative correlation between foreign exchange rate returns and foreign stock market returns reduces the return volatility in home currency terms and makes foreign investment more attractive. Portfolio choice therefore depends on exchange rate dynamics. But dynamic portfolio choice should simultaneously affect the exchange rate too. Differences in stock market performances generate imbalances between the dividend income of home and foreign investors. Dynamic rebalancing of equity portfolios then initiates forex equity flow, which in turn induces exchange rate movements.[13] Summarizing, the two main testable implications of their model are that: 1) Higher returns in the home equity market relative to the foreign equity market are associated with home currency depreciation; 2) Net equity flows into the foreign market are positively correlated with a foreign currency appreciation. Their view contradicts the conventional wisdom that a strong equity market comes with a strong currency.

Pavlova and Rigobon (2003) go one step further and show that the correlation of the exchange rate with equity returns depends on the source of the shock that hits an economy. If a positive home equity return has been caused by a productivity shock at home, the response of the home currency is depreciation along with a fall in bond prices (the Ricardian effect, since good news for home production means that home goods become relatively less scarce and hence their price has to fall). A fall in the domestic currency implies that the foreign currency appreciates. *Ceteris paribus* this leads to positive returns in the foreign stock market (contagion).

On the other hand, if instead of a productivity shock a country had a positive demand shock, then home consumers will want to consume more (biased toward the home good) and bid up the price of the home good relative to foreign. The terms of trade of home improve

[13] They also claim that changes in bond holdings should not induce any exchange rate movements due to the fact that they are largerly hegded against exchange rate risk. We offer empirical evidence that this claim is valid.

and its exchange rate appreciates. Leaving unaffected the home output, the appreciation of the exchange rate increases the value of the home output and hence there is going to be a positive return on the home stock market. Given the two effects, it is not clear which one will dominate, and hence it is not clear what the correlation should be (the authors document that the correlation in their empirical exercise is positive, and claim that this happens because demand shocks are far more important). Meredith (2001) argues that demand in the US was disproportionately affected by the emergence of the New Economy, because new economy sectors are bigger than in Europe, overall capitalization is higher, and stock ownership is more pervasive. Thus, even with a decline in the equity premium that is broadly equal across the US and Europe, the positive impact on demand in the US was greater because the stock market is of greater relevance to the economy than in Europe.

A second justification relates to the Balassa-Samuelson-type effect, which points to the importance of productivity differentials on traded goods. These differentials can lead to an increase in the domestic real interest rate relative to the world real interest rate and induce large capital inflows and home currency appreciations. Bailey, Millard and Wells (2001), for example, argue that a productivity shock that raises expected future output in the home country will tend to lead to capital inflows for two reasons. First, if domestic consumers expect to be richer in the future, they want to borrow more from abroad to finance present consumption. Second, expected increase in future productivity would raise expected future profits, which in turn will increase equity prices and ceteris paribus this will encourage investment. Domestic residents would want to take advantage of current investment opportunities without reducing current consumption and foreign residents would want to take advantage of the higher rates of return to capital. These two effects combined will lead to capital inflows to the home country and to an increase in the demand for home currency. They also argue that the persistence of the real exchange rate appreciation depends crucially on the effect of the shock on the productivity of the tradable sector relative to the non-tradable sector.

So far, we identified two channels through which capital flows affect exchange rates. The first one relates to the equity return differentials between two capital markets that induce

dynamic rebalancing of equity portfolios, which initiate forex equity flow and exchange rate movements. The second channel relates to productivity differentials on traded goods that can lead to interest rate differentials which in turn generate large capital inflows accompanied by currency revaluations.

We use an unrestricted VAR to study empirically the dynamic interactions of capital flows, equity return differentials, exchange rates and interest rate differentials in a unifying framework. The remainder of the paper will be empirical in nature, aiming to examine the following questions: 1) Do cross border net capital flows explain exchange rate movements? If yes, is it bonds or equities that matter?; 2) Is it the net accumulation of foreign equities or that of U.S. equities that matter? 4) What are the channels through which equity returns and interest rates work? 5) Does the capital flows-augmented model offer more predictive content than naive random walks?

2.4 Data and Methodology

2.4.1 Data description

Most exchange rate puzzles concern open trade countries with liberalized capital markets, where currency values are most likely to reflect macroeconomic market forces. We therefore focus our analysis on the top five OECD countries for foreign exchange market turnover worldwide: U.S., U.K., Germany, Japan and Switzerland. These countries have unrestricted capital flows for the entire sample period. Table 1 reports the total turnover by currency pair adjusted for cross border double counting. In 1998, the turnover for these four currency pairs sums to more than 50% of the total turnover of all currency pairs worldwide.

The sample period is from January 1988 to December 2000. The exchange rate data consists of monthly averages from the International Financial Statistics (IFS), published by the IMF. To have a sufficiently long time series for all currencies we use the Euro exchange rate (from January 1999 onwards) to calculate monthly averages for the dollar/mark. We are not able to construct all cross rates due to unavailable capital flow data. Interest rates

are three month money market rates obtained from the IFS.[14] As a proxy for the relative stock market performance between two countries we use equity return differentials.[15] These are based on equity returns (in local currencies) from EcoWin.[16] The data on bilateral asset flows is from the U.S. Department of Treasury.[17] The broadest measure that we construct is the net total purchase of equities and bonds for all four pairs, given by the following expression:

$$NF = NB + NE \tag{2.7}$$

where NF is the net capital flow between the US and a foreign country. NB and NE are the net accumulation of bonds and equities respectively. These can be further decomposed:

$$NB = NAUSB - NAFB \tag{2.8}$$

where $NAUSB$ are the net purchases of U.S. bonds from foreign residents, and $NAFB$ are the net purchases of foreign bonds from U.S. residents.

$$NE = NAUSE - NAFE \tag{2.9}$$

where $NAUSE$ are the net purchases of U.S. equities from foreign residents, and $NAFE$ are the net purchases of foreign equities from U.S. residents. A positive value for NF, NB and NE, indicates a net inflow to the U.S. (see Figures 2.4 for a schematic representation).

2.4.2 Methodology

An unrestricted VAR is a good approximation for the dynamic process of any time series vector, as long as enough lags are included.[18] Implicitly we assume that there exists an

[14] Long-term interest rates may be more appropriate for capturing inflation premia. We also performed the analysis with money supply differentials. The results do not depart in any significant way from those reported.

[15] We could also use the ratio of performance of the two equity markets. Doing so, results do not depart in any significant way from those reported here.

[16] See data Appendix for more detailed definitions and sources.

[17] This data is in U.S. dollars adjusted for exchange rate changes.

[18] See, for example, Canova (1995). Although the optimal lag length obtained with the Schwarz Bayesian (SBC) and the Hannan-Quinn (HQC) information criteria is one lag for all systems, we use two lags in order

equilibroum for asset holdings, exchange rates and asset prices. Hau and Rey (2002) show that under incomplete forex risk trading such equilibrium exists and they derive the optimal portfolio holdings for home and foreign investors as well as the price vectors that support this equilibrium. This equilibrium is, however, dynamic in the sense that is characterized by constant rebalancing of the optimal portfolios. Such time varying equilibria are better captured by unrestricted systems since they do not impose a priori a long-term attractor for the underlying factors. Thus, consider the Wold moving average representation of the system:

$$Y_t = \phi + B(l_p)\widetilde{e}_t \text{ with } \widetilde{e}_t \sim (0, \Sigma) \tag{2.10}$$

where $\widetilde{e}_t$ is an error process with variance-covariance matrix Σ, Y_t is a vector defined as $[ncf_t, (er^* - er)_t, s_t, (i^* - i)_t]'$, with ncf_t the net cross border capital flow, $(er^* - er)_t$ and $(i^* - i)_t$ are the realized equity return differentials (stated in U.S. currency) and the interest rate differential between the foreign country and the U.S., s_t, is the log domestic (U.S.) currency price of a unit of foreign currency. $B(l_p)$ is a matrix polynomial in the lag operator. Any orthogonal decomposition of the Wold representation with contemporaneously uncorrelated unit variance-covariance matrix is of the form:

$$Y_t = \phi + D(l)e_t \text{ with } e_t \sim (0, I) \tag{2.11}$$

where $D(l) = B(l)S$, and $e_t = S^{-1}\widetilde{e}_t$ with an orthonormalized matrix $\Sigma = SS'$. There is an infinite number of such orthonormal decompositions since for any orthonormal matrix Q, with $QQ\prime = I$: $\Sigma = \widehat{S}\widehat{S}' = SQQ'S'$ is also an admissible decomposition of Σ. In this study we will use the 'naive' orthogonal decomposition where $S = L$ (S is a lower triangular). This identification scheme implies an ordering for the variables in the VAR. The importance of this ordering depends on the magnitude of the estimated correlation coefficient between the

to accommodate problems that arise with near unit root regressors. We experiment with higher orders (up to 4 lags) to check the robustness of our results. All tests are available upon request. In all models we calculated the characteristic roots of the AR polynomials and they are all inside the unit circle. Also, the Ploberger, Kramer and Kontus (1989) recursive flacutation test is computed by bootstrapping (1000 replications). In all models, the test fails to reject the null hypothesis of structural stability.

variables included in the VAR. If this correlation is low, the ordering is immaterial.[19]

Optimally, one would identify the shocks based on a structural approach. There are, however, no theoretical results strong enough for such identification. At best, models have implications for the contemporaneous correlation structure between exchange rates, capital flows and equity return differentials but not the dynamic interactions. For robustness, we also perform the analysis with a methodology that does not use zero restrictions. Canova and De Nicolo (2002) show how one can obtain a moving average representation of the data generating process based on the conditional correlations obtained from hypothesis **H1** and **H2**. Section 5.1 shows that our results are robust to different identification schemes.

Modelling levels of exchange rates poses a question of stationarity and validity of the impulse response functions. Many authors suggest to either model the first difference of the nominal exchange rates or to perform a test for long-term attractors. Regarding the first suggestion Appendix A.2 offers a detailed explanation for using levels and not first differences. Although we argue against differencing the variables included in the VAR, particular caution has to be paid in the stationarity of the system. Phillips (1995) showed that in nonstationary VAR models, with some roots at near unity the estimated impulse response matrices are inconsistent at long horizons and tend to random matrices rather than the true impulse responses. However, it is well known that even if the estimated coefficients are biased, their distribution will be exact. A Monte Carlo procedure will therefore produce sensible standard error bands. For more details see also Appendix A.1. Regarding the second suggestion, we not that estimating long-run attractors from short span samples is not only implausible (since we aim to discover long-run relationships) but also misleading. Imposing a mis-estimated long-run relationship into a VAR system results in severe miscalculation of the true dynamics of the system. Last, for the theory we advocate, estimating and imposing a long-run relationship is not plausible.

[19] For more details on this issue see Enders (2003).

2.5 Exchange rates determinants

2.5.1 Which capital flow is relevant for exchange rates?

Exchange rate fluctuations resulting from large shocks to un-hedged foreign-currency denominated equity may be distinctly different to that from hedged bond flows.[20] To determine which form of capital flow has the greatest impact on exchange rate movements, we decompose the net total accumulation into its parts, as described in section 2.2.

Figures 2.1, 2.2, and 2.3 illustrate the bilateral exchange rates and net asset flows for the four countries against the U.S.[21] Capital flows are clearly correlated with exchange rates. Table 2.2 reports the simple contemporaneous unconditional correlations for the full sample and two sub-periods: 1988:1-1994:12 and 1995:1-2000:12. The coefficient estimates for U.S. net asset flows across all currency pairs is remarkably similar; a one percent increase in purchases of U.S. assets from foreign residents causes a contemporaneous appreciation of the home currency by around 20 to 30 basis points for all countries except Japan.

To assess asset flow impacts on nominal exchange rate modelling, we perform likelihood ratio tests (see Figure 2.4). We first estimate bilateral VAR's for all four currency pairs, including capital flows, exchange rates, equity return differentials, and interest rate differentials. We then test linear restrictions for the various capital flow coefficients estimated VAR's, as described in Table 3.[22]

The first column reports results for the impact of net total asset purchases on exchange rates. The test indicates that, for all currency pairs and at the 1% significance level, equity flow dynamics are distinctly different from bond flow dynamics. The second and third columns report the test statistics for the null hypothesis that bond flows do not impact exchange rates in the presence of equity flows and vice-versa. For three of four currencies,

[20] See, for example, Bailey, Millard and Wells (2001), Hau and Rey (2002), and Pavlova and Rigobon (2003).

[21] A Box-Cox transformation test indicates that the logarithmic transformation is slightly preferable to levels for all the exchange rate pairs and all capital flows studied.

[22] See also Lutkepohl (1993) for more details on linear restrictions in VAR's.

and at the 5% significance level, equity flows impact exchange rate movements. Bond flows, however, do not. This evidence supports Hau and Rey (2002)'s assumption that cross-border bond holdings are usually hedged and do not alter for exchange rates.

Column four probes deeper testing whether net purchases of foreign assets behave differently from net purchases of U.S. assets. For all currency pairs, and at the 1% significance level, the two types of assets have asymmetric effects on exchange rate movements. Columns five and six report the test statistics for the impact of each equity type separately (foreign and U.S. respectively). Purchases of U.S. equities by German, Japanese and Swiss residents are much more important than purchases of equities originated from these countries by U.S. residents. For the U.K., both flows are relevant, reflecting the UK market's importance as a financial center (see Warnock and Mason (2000) for an empirical analysis on the importance of financial centers). Normalized capital flows with market capitalization correlate stronger with exchange rates. Certain authors (e.g. Portes and Rey (2002) and Hau and Rey (2002)) have also proposed using GDP for standardizing capital flows. Given that we want to analyze monthly data, using interpolated monthly GDP forecasts would add noise to our data sets.

Therefore, we find evidence that for all currency pairs except the dollar/yen, changes in net U.S. equity purchases by foreign residents are important for bilateral exchange rate movements. Moreover, net purchases of U.K. assets, have important implications for the dollar/pound exchange rate. For Japan, capital flows seem to have an effect opposite to that expected in theory (e.g. Hau and Rey (2002) and Pavlova and Rigobon (2003)). Brennan and Cao (1997) and Brooks, Edison, Kumar and Slok (2001) also document that, during the 1990's, Japanese residents were buying US assets even as the yen strengthened.[23]

[23]Some commentators claim that this is the outcome of strategic interventions from the central bank of Japan to keep the yen low relative to the dollar so as to boost exports (see, for example, The Economist, September 20th-26th, 2003).

2.5.2 Impulse responses and variance decomposition

The model infers capital flow information from two main sources. First, the model identifies informed flows from impulse responses of exchange rates to capital flows. Specifically, informed flows induce a significant exchange rate change. Second, variance decomposition allows one to determine the proportion of monthly exchange rate movements due to capital flows. This statistic represents the overall equity flow contribution to currency determination.

More formally, if the shocks in the VAR are orthonormalized, the coefficients of the vector of lag polynomials $C(l)a$, with $a'a = 1$,represent each variable's impulse response to any structural shock.[24] The variance decomposition of Y_{it}, allocated to a at horizon τ is given by:

$$x_\tau(i,a) = \frac{\sum_{s=0}^{\tau-1}\left(C_a^i a\right)^2}{\sigma_{i\tau}^2} \tag{2.12}$$

where $\sigma_{i\tau}^2$ is the forecast error variance of Y_t at time t.

For each currency pair we specify the more important capital flow as indicated by the likelihood ratio tests. We use the net purchases of U.S. equities for the dollar/Swiss-franc, dollar/yen and dollar/mark and the net purchases of foreign as well as U.S. equities for the dollar/pound.

Figure 3 tests the two main hypothesis as described before and plots the estimated impulse responses of nominal exchange rates to one standard deviation shocks to all other variables. For the U.K., Germany and Switzerland, a one standard deviation shock to net purchases of U.S. equity significantly impacts exchange rates. Specifically, the U.S dollar appreciates 1% on average against the four relevant currencies with monthly net purchases of 100 million worth of U.S. equities. The appreciation lasts between 10 and 17 months. This

[24]Throughout this paper, the lag order for the Monte Carlo to obtain standard errors will be selected using the SBC and HQC criteria allowing for up to 10 lags for all models. Since the Jarque-Bera test rejects the null of Gaussian innovations for some countries, all Monte Carlo inference will be based on non-parametric resampling of the residuals. Also, we report the generalized impulse responses as derived by Pesaran and Shin (1998).

offers empirical support for the dynamic portfolio balance theory, summarized in **H1**, which states that shocks to equity flows should change the demand for currency in the presence of a price inelastic supply of currency balances. It also agrees with recent empirical evidence from Froot and Ramadorai (2002), Rime (2001) and Lyons and Evans (2002) although, these studies use order flows and higher frequency data sets. It is also in line with Brooks, Edison, Kumar and Slok (2001), Hau and Rey (2002) and Pavlova and Rigobon (2003) who use similar data sets but do not study the dynamic interactions between exchange rates and equity flows. The behavior of the dollar/yen exchange rate behaves differently. It implies that Japanese residents bought U.S. equities even as the yen strengthened.[25]

Figure 3 also indicates that positive shocks to foreign equity returns (relative to U.S.) have small and insignificant effects on nominal exchange rates for all periods across all currency pairs rejecting hypothesis **H2**. The direction of the impulse responses for the U.K. and Switzerland, however, are partially in line with Hau and Rey (2002) who claim that higher foreign equity returns relative to U.S. should be associated with a U.S. currency appreciation. Shocks to interest rate differentials are also insignificant for exchange rates for all horizons, and responses are not consistent with UIP. In section 3.2.3 we pool the data from all countries and we provide conclusive empirical evidence for the last two hypothesis.

Table 4 reports the percentage of in-sample forecast error of exchange rates that can be explained by equity return differentials, interest rate differentials and capital flows. For the dollar/pound, dollar/mark and dollar/Swiss-franc net equity flows explain on average 20%, equity return differentials 2%, and interest rate differentials 1%. A counterfactual analysis further illustrates the net equity flow explanatory power for monthly nominal exchange rate movements.

2.5.3 Counterfactual analysis

In the VAR modelling framework one can use the decomposed shocks to perform in-sample tests for the validity of each of these shocks in explaining nominal exchange rate fluctuations.

[25] Brooks, Edison, Kumar and Slok (2001) document the same finding at an annual frequency.

This is often called counterfactual analysis, since it shows the evolution of the depended variable if only one shock was present at each point in time. We note that if one is interested for the overall fit of the model, all shocks should be included in the exchange rate equation. Figures 2.6, 2.7 and 2.8 illustrate the in-sample prediction for the nominal rate when only one of the following variables enters the estimated equation for exchange rates: net equity flows, interest rate differentials, equity return differentials. We also report the monthly contemporaneous correlation between the predicted exchange rate and the actual one. For equity flows (Figure 2.6), the estimated correlation is on average 43% (and goes up to 55% if we exclude Japan). This agrees with Evans and Lyons (2002)'s findings, who document that daily inter-dealer order flow explains an astonishing 60% of daily exchange rate changes.

Figure 2.6 illustrates that in early 1990s equity flows were a primary determinant of the dollar/pound exchange rate. In the latter part of 1990s, equity flows correlate more weakly with the monthly movements. Equity flows seem to have played a crucial role in the German mark appreciation in mid-1990s and the almost monotonic depreciation thereafter. Equity flows are informative for the dollar/Swiss-franc rate since for the entire sample period, the equity flows-predicted exchange rate closely follows the actual one. For Japan, equity flows track the long-run trend but fail to explain any short-run exchange rate movement.

Interestingly, interest rate differentials are not informative for the dollar/pound or the dollar/yen movements, but they do capture longer swings of the dollar/mark and dollar/Swiss-franc exchange rates (see Figure 2.7). Equity return differentials are immaterial for exchange rates, although a weak negative correlation is present for three out of the four currencies, indicating that higher equity returns in the foreign (relative to the U.S.) market are associated with dollar appreciations (see Figure 2.8). Counterfactual analysis reveals some interesting in-sample correlations between the variables included in the VAR system. The information content is purely descriptive and cannot replace the impulse response and variance decomposition validity. A pooled VAR, as analyzed below, provides stronger implications for the dynamics of equity returns and exchange rates.

2.5.4 Pooled VAR

The estimated impulse responses are likely to be imprecise due to the relatively short data set. The resulting estimator, when pooling the data from all countries, collapses to a standard OLS estimator of the structural impulse responses, unit by unit, when there is no information in the cross section, and to a pooled estimator, when the cross sectional information swamps the one present in a single unit.[26] We pool all countries except Japan due to its distinct dynamics of capital flows compared with the other countries. We then perform a Hausman-type test for country fixed effects.[27] The test indicates that different intercept terms must be included. One easy way to do that in a VAR is to include country specific dummies. Figure 2.9 plots exchange rate impulse responses to one standard deviation shocks in all other variables. Shocks to net purchases of U.S. equities cause a significant appreciation of the dollar against the mark, the swiss-franc and the pound consistent with **H1**. This appreciation lasts up to 20 months.

In addition to the net equity flow effects, positive shocks to U.S. equity returns are associated with a significant U.S. currency appreciation, lasting between 5 and 10 periods. This finding rejects **H2**. A further analysis, not reported here, includes dividend yield differentials as a proxy for relative capital markets performance. A positive shock in dividend yield differentials (defined as foreign minus U.S.) results in a significant appreciation of the pound and the mark against the dollar, which is both robust to the VAR ordering and inconsistent with **H2**. In addition shocks to equity flows are associated with decreases in dividend yield differentials, implying that net equity flows are associated with decreases in the actual dividend yield. We are not aware of any other study that documents this link for these countries. Bekaert, Harvey and Lumsdaine (1999) show that the same result holds for

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[26] See, for example, Canova and Ciccarelli (2003). Note that while pooling countries can overcome certain inference difficulties, however, it is unclear that a panel approach would necessarily draw the relevant variation, as currency values in different countries may be driven by different forces (such as monetary policy and exchange rate regimes) and have different underlying data generating processes. Rapach and Wohar (2001) and Neely and Sarno (2002) discuss this issue and cast doubt on pooling exercises. We feel, however, that pooling the data for U.K., Germany and Switzerland seems plausible.

[27] We choose not to report these tests to save space. All tests are available upon request. Note also that in the pooled VAR's we include both the U.S. and foreign asset flows for completeness.

a number of emerging economies.

Positive shocks to U.S. interest rates cause an appreciation of the U.S. currency that is marginally significant from the 15^{th} to the 25^{th} month. This is in line with Froot and Ramadorai (2002), who document how short-run interest rate differentials behave as a permanent component in institutional investor's flows shocks.

Impulse responses, obtained from the pooled-VAR, provide empirical evidence that equity flows affect exchange rates through two channels. In the short-run, equity return differentials play the most important role, whereas in the long-run, interest rate differentials dominate. Table 2.5 reports the percentage of the in-sample forecast error variance of exchange rates that can be explained by all other variables. These results illustrate that for U.K., Germany and Switzerland, net equity flows have important explanatory power for exchange rate behavior vis-a-vis the U.S. Their omission may explain some earlier exchange rate equation failures.

2.6 Exchange rate predictability

Meese and Rogoff (1983) showed that, for out of sample exchange rate forecasts, a random walk model outperforms structural models, including the flexible-price and sticky-price monetary models and the sticky-price model which incorporates the current account. This finding holds for major countries with floating exchange rates. Two decades of post Meese-Rogoff empirical work shows that their original finding is remarkably robust. While several strands of literature claim to have found models with forecast performance superior to that of a random walk (especially at horizons greater than 3-4 years), the inference procedures and methods used are still debated.[28] The main problem that empirical research faces is the relatively short period of floating exchange rate regimes. This paper, deals with the even shorter cross border international capital flows data in monthly frequency. Certain recent papers have shown success in outperforming a random walk forecast, but their approaches

[28]For good coverage of the debate see, among others, Berden and van Dijk (1998), Kilian (1999), and Rossi (2002).

depart significantly from the linear time series framework.[29] It is therefore indeed difficult to beat the random walk forecast, especially when using fundamentals-based specifications.

In this section, we return to our conventional linear time series setup for analyzing exchange rate predictability. We look at out-of-sample point forecasts based on our empirical specification, and compare them to the results from a univariate random walk. We measure out-of-sample accuracy, as in Meese and Rogoff (1983), by the root mean square error (RMSE) as defined below:

$$RMSE = \left\{ \sum_{s=0}^{N_{k-1}} \left[F_{t+s+k} - A_{t+s+k} \right]^2 / N_k \right\}^{1/2} \tag{2.13}$$

where $k = 1, 3, 6, 12, 18$ denotes the forecast horizon, N_k the total number of forecasts in the projection period for which the actual value A_t is known, and F_t the forecast value. As Meese and Rogoff (1983) note, since we forecast the logarithm of the exchange rate, this statistic is unit-free, approximately in percentage terms, and comparable across currencies.

Table 2.6 reports the root mean square error statistics for the five forecast horizons from January 2001 to May 2003. The linear model beats the random walk for some currency pairs and some horizons. For the dollar/Swiss-franc, the capital-augmented VAR beats the random walk at all 5 forecasting horizons. For the dollar/mark, the VAR beats the random walk after the 6th month, whereas for the dollar/pound after the 23rd. Recall that the in-sample explanatory power of net equity flows for exchange rates closely mimics their ability to forecast them.[30] The equity augmented-VAR fails for the dollar/yen. This comes at no surprise, given the poor in-sample capital flow performance in explaining yen against the dollar.[31] To visualize this finding, Figure 2.10 plots actual vs forecasted exchange rates plus 1 standard error bands (calculated using 1000 bootstrapped replications).

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[29] See, among others, MacDonald and Marsh (1997), Kilian and Taylor (2001), Mark and Sul (2001) and Faust, Rogers, Wright (2001), and Chen (2002).

[30] The conventional wisdom of increased out-of-sample predictions validity relative to in-sample has been questioned. See the recent work of Inoue and Kilian (2003).

[31] Many theorists claim that nominal exchange rates are indeed a random walk in the short-run, driven from factors irrelevant to any economic variable. Since we provide empirical evidence to the contrary, we parcially address this concern by imposing a unit root prior for all currency pairs (a Litterman prior) and repeat the forecasting exercise. The results are robust. We do not report them to save space but they are available to the reader upon request.

The empirical evidence that a linear VAR with net equity flows and interest rate differentials outperforms a univariate random walk can be due to the data generating process becoming more responsive to interest rate differentials in recent years. We partially address this point by using the Diebold-Mariano (1995) statistics to compare an equity augmented-VAR, a standard VAR that includes only exchange rates and interest rate differentials and a univariate random walk.[32] The test uses the mean square error criterion and tests the null hypothesis that there exists a significant difference between two forecasts. Table 2.7 presents the test statistics with their associated *p-values* for $k = 3, 6, 12, 18, 24$ forecast horizons. We see that an equity augmented specification indeed provides support for exchange rate predictability and outperforms a random walk and a standard VAR. However, the specification that produces such superior forecast performance depends on the exchange rate and the forecast horizon under investigation.

2.7 Sensitivity analysis

So far we document that nominal exchange rates greatly depend on capital flows in the monthly frequency. In this section we investigate the robustness of our findings. We first assess the potential problems related to our 'naïve' identification scheme; Second, we assess the validity of our results in different sub periods; third, we investigate the impact of data mis-reporting and fourth, we develop a methodology to deal with measurement error in the regressors.[33]

2.7.1 Identification

The ordering of the variables in the VAR becomes important when the correlation coefficient between them is high. When this is the case, the ordering may affect the estimated impulse response functions.

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[32] Using the modified Diebold-Mariano statistic suggested by Harvey, Lybourne and Newbold (1997) that accounts for size distortions that occur in small samples, does not change the results in any significant way.

[33] For illustrative purposes and to save space we use the pooled data set when possible.

Although we use generalized impulse responses that aim to be independent of the ordering, with such rich autoregressive dynamics and the use of 'naive' identification schemes, the ordering of the variables can still play a significant role. To evaluate how sensitive our results are to the variable ordering, we estimate the equity augmented-VAR using alternative ordering. We start with the following ordering: net equity flows in foreign assets, net equity flows in U.S. assets, equity return differentials, exchange rates and interest rate differentials. Table 2.8 reports the estimated residual correlation matrix. We then reverse the ordering of net equity flows in U.S. assets with that of exchange rates and interest rate differentials, since they exhibit the highest correlation. We illustrate the new set of impulse responses in Figure 2.9. Re-ordering leaves, as expected, the equity return differential effect unchanged. The interest rate differential effect moves forward, now significant from the 25^{th} period to the 35^{th}. The effect of U.S. equities remains significant for almost 45 periods.[34]

To further asses the robustness of our results we also used the Canova and DeNicolo (2002) method. This involves the estimation of the pooled-VAR using sign restrictions based on the estimated correlations from the true data based on the theoretical hypothesis **H1**. Identification is achieved by a grid search over the space of all feasible decompositions of the variance-covariance matrix and selection of the one which accords best with the theoretical priors. Figure 2.11 plots the impulse response of exchange rates to shocks in net equity flows when this method is used. It is almost identical to the one obtained with a Choleski decomposition (shown in Figure 2.9).[35]

2.7.2 Structural Breaks

Some authors (e.g. Hau and Rey (2002)) assert that capital flows have become more important for exchange rates in recent years due to the increasingly integrated global financial system. This implies that the relationship between capital flows and exchange rates may change over time. We therefore split the data into two sub periods and estimate a VAR

[34] We also employed the Sims-Bernanke identification method (not reported). This treats the contemporaneous coefficients as free parameters that can be estimated with non-linear least squares. It then uses these estimates to identify the system. The results resemble those reported here.

[35] One could also use the Rigobon and Sack (2003) identification methodology through heteroscedasticity.

for all currency pairs, including net equity flows (in foreign and U.S. assets), equity return differentials, exchange rates and interest rate differentials. We report only the sub periods generated when we split the data in the last month of 1994 as in Hau and Rey (2002).[36]

Table 9 reports the historical decomposition of nominal exchange rates for the two sub-periods. Four features stand out: first, the increased importance of equity flows (from 12% on average, to 18% in a 36 month horizon); second, the slight increased importance of net foreign asset flows relative to U.S. assets (reflecting the rise of U.K. and Germany as European financial centers); third, the increased importance of interest rate differentials (reflecting investors' run to fundamentals as a guide of economic prosperity and future growth)[37]; and fourth, the relatively large increase in the equity return differential importance in the recent period, reflecting the downturn of the Japanese and European economies in the second half of the 90s. The results reenforce the validity of **H1** and the capital flows dominance in explaining exchange rates, regardless of the time period. This is line with Hau and Rey (2002)'s assertion about increased international capital markets integration and Portes and Rey (2002)'s evidence on the increased importance of cross-border equity flows.

2.7.3 Data mis-reporting

The TIC accounting system disagrees with that of national income and product accounts data. For example, under TIC, a Japanese CEO of Toyota USA (a Japanese national) who gets a salary in US dollars and invests in the US stock market is counted as a US national. On the other hand, a US national with an account in the Cayman Islands, buying US equities, is a foreigner for TIC accounting purposes. The bias introduced by this limitation is likely to be small as long as any failure to correctly recognize the investor nationality is small relative to the flows recorded.

The TIC system also captures portfolio transactions between U.S. and foreign residents,

[36] Hau and Rey (2002) report that the correlation of equity flows and exchange rates for 17 OECD countries has indeed become stronger in recent years.

[37] See also the article of Jeffrey Garten on the "The Economist" of the 4th of January 2003, pp. 54. Brooks, Edison, Kumar and Slok (2001) report similar results but for long term interest rate differentials.

but fails when U.S. residents acquire stocks through merger-related stock swaps. When a company, based in a foreign country (from the US perspective) acquires a U.S. firm, one form of financing is an exchange of equity in which shareholders of the target (U.S.) firm are given stocks in the acquiring (foreign) firm. The US company acquisition is recorded as a foreign direct investment in US but not in the TIC system. However, the TIC system will report if US residents subsequently sell their new foreign equities.

Since the TIC system does not record the initial acquisition, but does capture subsequent sales, measures of stock swaps might be important. This form of financing cross-country mergers and acquisitions is a recent strategy that evolved in importance in 1998 and 1999. Some of the most important mergers and acquisitions took place in these years, including that of Daimler Chrysler, BP Amoco, and Airtouch Vodafone. Prior to 1998 the only deal that involved a substantial exchange of stocks was the Beecham/SmithKline merger (1989). To assess the importance of such flows in our analysis we collected data in all gross cross-border foreign direct investments (FDI hereafter) between US and the four counter parts. Figure 2.12 plots the volume of cross-border transactions in equity and debt flows and that of FDI's (we also report the cross correlation of portfolio flows and FDI's). It is clear that the data on FDI does pick up the unusual mega-merger wave of the late 1990s. However, the percentage of FDI's in gross cross-border transactions does not exceed 5%. This problem is therefore unlikely to affect the results significantly. Furthermore, Warnock and Mason (2001) and Edison, Brooks, Kumar and Slok (2001) document that equity swaps are insignificant and do not entail any large bias in the reported TIC data.

2.7.4 Errors in variables

A more serious concern that is neglected in most studies using the TIC data is the effect of measurement errors in the regressors. Recall the simultaneous equation setup of an unrestricted VAR(p). If $z_t = (x_{t-1}, x_{t-2}, \ldots : y_t)$ and $\Gamma = (D_1', D_2', ..., a)'$ is a $(q + pN) * N$ matrix then:

$$x_t = z_t\Gamma + e_t \tag{2.14}$$

where $e_t \backsim (0, \Sigma_e)$. This model can be identified (as shown in section 3) and estimated equation by equation with OLS. For linear regression models, Wansbeek and Meijer (2002) show that in the presence of measurement error, in one or more of the regressors in a system of equations, the estimated coefficient relating to the mis-measured regressor is asymptotically biased towards zero. This phenomenon is called attenuation. Both the size of the coefficients on the independent variables and the model's explanatory power are underestimated. Moreover, in VAR's, the measurement error accumulates when calculating the impulse response functions and will obscure the true response to any structural innovation.

One way to circumvent this problem is to generate impulse responses from the theoretical asymptotic distribution of the regression coefficients and compare them with that obtained by estimation with a regressor perturbed by an i.i.d. measurement error. Suppose that capital flows are measured with error. Let x^* denote the vector of regressors that contains the observed value of capital flows and x the vector that contains its true value. The model is given by:

$$x_t^* = z_t^* \Gamma + e_t \tag{2.15}$$

where $z_t^* = (x_{t-1}^*, x_{t-2}^*, \ldots : y_t)$, AND equity flows which are contained in x_{t-i}^* are augmented with an i.i.d. measurement error, u, with zero mean and constant variance σ_u^2. Thus, $x_t^* = x_t + u$. We first estimate (6) and calculate the impulse response function of exchange rates to innovations in capital flows. We then use bootstrap techniques to construct 2 standard error bands for the impulse responses resulting from the presence of measurement error.[38]

Figure 2.13 shows the impulse response of exchange rates to a one standard deviation shock in net equity flows and the two standard error bands obtained from equity flows perturbed by three different i.i.d. measurement errors. The significance of exchange rate responses to innovations in net equity flows is mitigated and eventually disappears for i.i.d. errors with standard deviation (*sigma* in the graph) greater than 0.25. For the effect of

[38] Sampling error is also included in the two standard error bands. For more details on the methodology, see Appendix C.

equity flows on exchange rates to completely disappear, we need more than 5% monthly measurement error in equity flows.

2.8 Conclusion

This paper investigates whether recent theoretically supported correlations between net capital flows, nominal exchange rates, equity return differentials and interest rate differentials are consistent with recent data. We use VAR's to test the hypothesis that positive un-hegded home capital flow innovations are associated with home currency appreciations (**H1**), consistent with a dynamic portfolio rebalancing of equity positions from international investors. Furthermore, we asses whether shocks to foreign equity returns (relative to home market) are associated with a foreign currency appreciation (**H2**).

We document that for U.K., Germany and Switzerland, a positive shock to net purchases of U.S. *equities* has a significant effect on exchange rates that lasts between 10 and 17 months supporting **H1**. Monthly net purchases of $1bn$ worth of U.S. equities is accompanied by, on average, a 1% appreciation of the U.S dollar against the mark, pound and Swiss-franc.[39] On the contrary, net purchases of U.S. *bonds* are immaterial for exchange rate movements. The evidence for Japan is not consistent with theory, since net purchases of U.S. assets from Japanese residents are associated with a strong yen. These findings are robust to a number of specifications, including identification and lag length of the VAR, structural breaks, data mis-reporting and measurement error in the regressors.

Positive shocks to foreign equity returns (relative to the U.S. market) are associated with U.S. currency appreciation. This channel, however, is not robust to the identification of the VAR and the sample size. Thus, it does not provide solid empirical evidence for **H2**. Positive shocks to U.S. interest rates (relative to the foreign countries) cause U.S. currency appreciations not consistent with uncovered interest rate parity (UIP).

[39] This elasticity might seem small. These flows, however, are private flows. Such an elasticity, however, is possible in a single day if markets are thin and large Central Banks intervene in the international forex market by buying or selling U.S. assets.

For a short to medium-horizon outlook (1 month to 2 years), we show that dynamic forecasts from an equity augmented-VAR provide support for exchange rate predictability. These forecasts also outperform both a random walk and a standard VAR that includes only exchange rates and interest rate differentials. Forecast performance is evaluated with the root mean square error criterion and the Diebold and Marianno (1995) statistics. The particular specification that can produce such superior forecast performance, however, depends on the exchange rate and the forecast horizon.

The empirical results indicate how increased capital mobility (seen in recent years) has impacted exchange rate movements. As net purchases of cross-border equities has become a larger share of total cross-border flows (including foreign direct investment, bank flows etc.), their effect on nominal exchange rates has become increasingly important. Monetary policy makers, in their pursuit for inflation and output gap stability,[40] should pay particular attention to equity flows as a determinant of exchange rate movements.

[40] See Abreu and Brunermeir (2003), Rigobon and Sack (2003), Bernanke and Gertler (2000), and Cecchetti, Genberg, Lipski, and Wadhwani (2000) for a recent debate on whether policy makers should respond to excess asset price movements.

Chapter 3

Democratization and Growth

3.1 Introduction

Leading politicians and influential academics have recently argued that democratization will bring long-term prosperity into "pariah" and economically poorly performing countries.[1] Others, however, remain sceptical, pointing to the mixed and inconclusive empirical evidence. So, the old debate in political science, sociology and economics, dating back to Plato and Aristotle, on which political regime is socially and economically optimal arises again in international relations, press and academic thinking. This debate is not purely academic and philosophical. It has important policy implications. In the last thirty years, the world has experienced an unprecedented move towards democratic institutions. Influential policy-makers and scholars urge Western authorities to foster democratic movements in totalitarian countries (e.g. *The Economist* article "*Philosophers and Kings*" (June 19th 2003)). So the question is: "*Will democratization bring economic growth?*"

To assess whether a successful democratic transition is associated with faster growth, we first examine numerous resources to identify the countries and the exact timing of permanent democratizations in the period $1960 - 2000$. We then employ an event study approach and analyze the evolution of GDP growth before and after such incidents of political moderniza-

[1]For example, in a recent article (24-6-2003) in the *New York Times*, former United States Secretary of State Colin L. Powell suggested that the stepping-down of President Mugabe and the re-establishment of democracy in Zimbabwe would quickly bring back prosperity.

tion.[2] The panel results reveal new evidence that contrast the previous mainly cross-sectional work. First, conditioning on various growth determinants, global shocks and business cycle effects, a permanent democratization is associated with approximately half to one percent increment in real per capita growth. Second, a dynamic J-shaped effect emerges implying sizable transition costs, but significant benefits in the medium and especially the long run. Third, when we control for democratization endogeneity (using "treatment" models), we find an even higher effect, implying that political reforms yield the highest return in countries where democracy is (theoretically) more likely to emerge and stabilize. In line with this interpretation, we further show that the positive impact of reforms is concentrated in countries with (relatively) high levels of human capital. Fourth, the results suggest that even moderate reforms can have sizable positive growth effects.

Following the development of the empirical growth framework (Barro, 1997; Barro and Sala-i-Martin, 1995), many authors have augmented a classical growth regression with various subjective measures of political freedom. An overall assessment of this literature suggests that democracy has an economically small and statistically insignificant effect on economic growth (see, for example, the reviews of Przeworski and Limongi, 1993; Sirowi and Inkeles, 1990; Przeworski *et al.*, 2000). A parallel work has, however, established a strong causal link between the quality of institutions and long-run economic performance (Knack and Keefer, 1995; Hall and Jones, 1999; Acemoglu, Johnson and Robinson, 2001). Jointly this evidence, therefore, implies that while institutional enhancement significantly contributes to economic growth, democracy specifically seems to be immaterial. La Porta *et al.* (1999) summarize this somewhat surprising result: "*The relationship between democracy and economic success has been difficult to find in recent data.*"

The empirical work from which this conclusion is drawn has several, well-documented drawbacks. First stands measurement error. Classical measurement error yields an attenuation bias (at least in a univariate framework) and reduces the economic significance of results. This problem is particularly important in studies of democracy, which "*has meant different*

[2]The phrase "*political modernization*" is taken from Huntington. We do not make any distinction between different types of autocratic and democratic regimes and thus use non-democracies, autocracies, or dictatorships as rough synonyms.

things to different people at different times and places" (Dahl, 2000, p.3). There is now a considerable literature in political science that discusses the conceptual problems in defining and measuring democracy (see for a review Bollen and Paxton, 2000). We address this problem in two ways: First, we investigate numerous historical resources, electoral archives and democracy indicators in order to identify as precisely as possible the exact timing and nature of a permanent democratic transition. Second, we avoid ranking countries based on the level of political liberties and classify a regime as a binary outcome. This is not only theoretically grounded (Huntington, 1993; Przeworski *et al.*, 2000), but also significantly reduces subjectivity and year-to-year measurement error. Our results hint that measurement error was a serious drawback of previous efforts that studied the freedom-growth nexus.

The second shortcoming is the empirical question itself. Previous work does not directly ask whether a successful democratic transition and the consolidation of democratic institutions are associated with higher growth. Rather, the literature utilizes the cross section variation to identify long-run data patterns. Given, however, the nature of the policy question, we want to explore the within country variation to investigate whether growth accelerates, declines or remains stable following the regime change. A related problem arises due to omitted variables. Since a growth regression can only incorporate a limited number of independent variables, it is necessary to employ panel data techniques that control for individual heterogeneity and omitted variables. We therefore add country fixed effects that capture (part of) the unobserved heterogeneity.[3] In addition, the differences-in-differences technique employed circumvents many reverse causality concerns that emerge from pooling and comparing countries that are heterogeneous in various dimensions. Acknowledging the difficulties of establishing causation, the main message of our analysis is that the "within" correlation between democracy and growth in countries that abandoned autocratic rule and established representative institutions is positive and significant.

Third, although theory has called for a dynamic analysis, previous work has focused on unified long-run correlations. The descriptive evolution of mean growth around the timing

[3]This is not to say that "fixed-effect" estimates are a panacea. Fixed-effects exacerbate problems arising from measurement error. They do not also solve a problem caused by the omission of relevant time-varying regressors.

of democratization, given in Figure 1.1, reveals a clear J-shaped pattern. Output growth drops during the democratic transition, but then fluctuates at a higher rate. The graph suggests a "short-run pain", due to high transition costs, followed by "long-run gain" due to higher growth after the consolidation of democracy. A five or ten year average will simply obscure this information. We thus depart from the standard growth regression framework and employ dynamic panel data methodologies with annual frequency observations.[4] This enables us to quantify both the short and the long-run correlations of political modernization and growth. Friedrich Hayek (1960) eloquently summarized this point: *"It is in its dynamic, rather than in its static, aspects that the value of democracy proves itself. As is true of liberty, the benefits of democracy will show themselves only in the long run, while its more immediate achievements may well be inferior to those of other forms of government."* Our empirical results validate Hayek's intuition, by showing that growth accelerates after a transition period of approximately three to five years.

This Chapter's work is related to a new wave of research that studies the effect of institutions (structural policies) on economic performance (for a recent review, see Acemoglu *et al.* 2005). The results suggest that besides legal norms or property rights protection, the type and quality of political institutions correlates substantially with economic growth. By analyzing probably the most important institutional change (the transition from an autocratic rule into a democratic political organization), this work is also related to studies on how constitutional rules influence economic policies (e.g. Persson and Tabellini, 2003; Persson, 2003). From a methodological standpoint, the identification of successful democratic transitions and the before-after within approach resembles recent analogous event-studies in macroeconomics. Sachs and Warner (1995) and Wacziarg and Welch (2003) study the within-effect of trade liberalization, while Bekaert, *et al.* (*forthcoming*) the aftermath of financial liberalization policies on growth. Interestingly a gap exists in studying the probably most fundamental "event", a permanent democratic transition. This study aims to fill this gap both by providing a new data set and by comparing growth rates before and after

[4]Previous work has relied on cross section or panel data techniques using 5 or 10 year period averages. See Attanasio *et al.* (2000) for an analytical discussion on the merits of using annual observations rather than n-year averages in growth regressions.

incidents of permanent democratic changes.[5]

This Chapter is structured as follows: Section 3.2 outlines the theory on how political liberalism affects growth and discuss previous empirical work. Section 3.3 presents the identification strategy for both the timing and nature of permanent democratizations. It also provides preliminary evidence on within country growth differences before and after regime change.Section 3.4 describes the econometric framework of the analysis and presents benchmark results. Section 3.5 gives additional evidence and perform various robustness checks. Besides addressing issues of omitted variables, selectivity in identifying democratic transitions, and transition costs, we also control for the endogeneity of democratization itself. We also investigate through which channels the positive impact of reforms mostly operates. Section 3.6 summarizes this Chapter's main findings and offers some directions for future research.

3.2 Theory and previous empirical evidence

Before the empirical analysis, it is useful to go through the recent theoretical arguments on the inter-relations between representative institutions and economic performance. For analytical purposes we distinguish between theories that emphasize the negative consequences of democratic institutions on growth ("sceptical" theories) and those that stress positive aspects ("development" theories). These theories are complementary rather than competing.

[5]In a brief paper released after completing the present study, Rodrik and Wacziarg (2004) also explore the "within" effect of democratic reforms. Their analysis differs, however, significantly from our work both methodologically and in the results. First Rodrik and Wacziarg do not identify permanent democratic transitions, but rely on the Polity coding, which has many conceptual drawbacks (discussed in detail in Section 3 and Appendix *A*). Second, they do not measure the intensity of reforms and do not isolate permanent transitions from temporary incidents, which capture political instability. Third they do not control for business cycle effects, pre-reform trends and standard growth covariates. Fourth, they do not quantify the dynamic evolution of output growth around the transition. Fifth, we provide a tedious series of robustness checks, including Heckman-type estimates that control for reforms' endogeneity. We also reach different results. Rodrik and Wacziarg find that democratic transitions do not produce negative growth outcomes. They, too, document some regressions, where democratic reforms exert a significantly positive impact on growth, but this applies only in the short-run. Our dynamic approach and the careful identification of democratic reforms, reveals in contrast that in the short-run there are sizable output costs. Yet, our results imply that democratic reforms yield positive growth effects after the volatile years of the transition. We elaborate and clarify further these differences in the text.

Both the classical old theoretical literature and the new political economy models suggest that multiple equilibria can emerge.

3.2.1 Negative Consequences

"Sceptical" approaches of democracy emphasize the inefficiencies of representative government. Influential scholars such as Plato, Alexis de Tocqueville (1835) and more recently Huntington (1968) feared the malignant effects of increased demands for current consumption. The "public choice" tradition (Buchanan and Tullock, 1962) has focused on the agency conflict between elected politicians and the public. It was also deeply concerned about the possibility of a big state satisfying the demands of the masses. Becker (1983) models how a democratic polity can yield inefficient outcomes by enabling various interest groups to compete for political influence. Not only is lobbying wasteful per se, but the generated policies yield a large deadweight loss for society. Besley and Coate (1998) synthesize a vast literature that models the distortions caused by incumbent politicians running excessive deficits to guarantee re-election. The endogenous fiscal policy literature models the interaction between the polity type, income inequality and economic performance. "Median-voter" models in the spirit of Alesina and Rodrik (1994), and Persson and Tabellini (1994), among others, show that high levels of income inequality raise the demand for redistribution. If fiscal policy and taxation are selected endogenously by majority voting, then high inequality will lead to lofty taxation, lower investment and consequently slower growth.[6] This growth equilibrium, however, does not necessarily apply to non-democracies, since the "median voter" cannot enforce redistribution.[7] Proponents of the sceptical-conflict approach stress the need for a "*strong state with an iron hand that neglects populist demands and enforces developmentalist policies with its insulation from particularistic pressures, particularly those originating from large firms and unions*" (Rao, 1984).

[6]In line with this argument, Persson and Tabellini present evidence that the negative impact of income inequality on growth is present only in democratic societies, hinting that electoral institutions enable voters to enforce costly redistributive policies. For empirical studies on the association between inequality and growth see Perotti (1996), Barro (2000), Forbes (2000), and Banerjee and Duflo (2003).

[7]In Acemoglu and Robinson (2000, 2004) redistribution pressures are present in autocracies as well. The ruling elite, thus has to implement social-welfare policies to prevent a revolution.

The economic success of the East Asian countries, which flourished under non-democratic regimes, offers the most illustrative validation for this theoretical approach. The government in these countries played an active role in the economy, but neglected pressures for redistributive policies and secured foreign investment (Rodrik, 2004). Tavares and Wacziarg (2001) study the channels through which democracy influences growth and find supportive for "public choice" theories evidence. Specifically, they show that democracies are associated with low levels of private investment and high government spending.

3.2.2 Positive effects

"Development" theories of democratic rule stress, in contrast, the aspects of representative institutions that are beneficial to growth . Many positive channels have been put forward: First, redistribution need not play a malignant role, since taxation revenues can be used to subsidize investment on physical or/and human capital. Saint-Paul and Verdier (1993) and Bourgouignon and Verdier (2000), for example, model how redistribution can spur growth through increased public spending in education. Likewise, in Galor and Zeira's (1993) model, redistribution enables beneficiaries to overcome capital market imperfections. Second, democratic institutions can be more efficient, by minimizing transaction costs of sociopolitical organization (Wittman, 1989). Olson (1993) argued that the electoral process solves commitment issues, while Sen (2000) pointed out the superiority of democratic rule in gathering and transmitting new information. Douglass North (1990), summarizes this Coasian approach "*...The institutional structure most favorable to approximate such conditions (efficient markets of the neoclassical model) is a modern democratic society with universal suffrage.*" Third, democratic societies are typically more open, there are less barriers to firm entry and they can therefore faster and more easily utilize productivity innovations (Acemoglu, 2003). Fourth, Olson (1993) claimed that democracy brings prosperity because it requires the same institutions (such as high quality legal system and the security of property rights), which are necessary for steady growth. Acemoglu, Johnson, Robinson and Yared (2005a, b) build a similar argument claiming that early institutions influenced both economic and political development.

Although, the empirical literature has not revealed a direct unambiguous positive effect of democracy on growth, there exists some evidence supportive to development theories. Probably, the most widely known empirical finding in favour of the democratic process is Amartya Sen's (2000) observation that a famine has never occurred in a democratic society. Rodrik (1999) shows that democracies cope better with adverse economic shocks. Wacziarg (2001) and Tavares and Wacziarg reveal a positive channel of democracy on growth through higher human capital accumulation. Democracies are also associated with lower political instability and smoother government changes (Alesina and Perotti, 1996; Alesina, *et al.*, 1996). Jones and Olken (2004) document sizeable growth gains following a leader's death. In line with democratic development theories, this positive impact is greater when it is accompanied by moderate democratic reforms.

Exploiting our new dataset of democratization episodes in the period $1960-2000$ and using an event study methodology that explores the "within" country variation, we provide compelling evidence that democratization is associated with sizeable growth gains. This positive impact is present even when we control for indirect channel effects through education, investment or government spending. We thus presume that democratic reforms might operate through alternative non-widely studied channels (e.g. raising productivity, fostering entry of new firms, strengthening other pro-growth institutions).

3.3 The Democratization Data & Preliminary Evidence

3.3.1 Event Identification Methodology

To precisely identify the countries and exact timing of a permanent democratic transition and minimize measurement error, we relied not only on both previously-used Polity and Freedom House (FH) indices, but also on numerous other historical resources.

We first locate significant changes in political organization according to either the FH or the Polity indicator in the period $1960-2000$: 1) When the 21 range Polity measure (ranging from -10 to $+10$, with smaller values indicating a lower level of political freedom) suddenly

increases from a negative to a positive value. 2) When the trichotomous FH political status characterization jumps from "not free" to either "partly free" or "free". We also require that both indicators remain at the higher democracy score for at least five years. The stability requirement is essential. All political organization theories focus and model the economic consequences of stabilized, permanent regimes.[8] We therefore exclude spikes of either of the two democracy indicators, since a careful investigation shows that they represent political instability rather than an actual transition.[9]

Second, we examine numerous political science and historical resources, as well as election databases, to precisely identify the timing and characterize the nature of each transition. By doing so, we minimize the conceptual and measurement problems of the democracy indicators (Munck and Verkuillen, 2002; Bollen and Paxton, 2001). The timing of democratization is either at the first internationally deemed "free and fair" elections or at the adoption of a new democratic constitution after a prolonged period of autocratic rule.

Third, we distinguish between "full" and "partial" democratization episodes. We designate "full democratization" status to countries where both the Polity indicator is greater than +7 and the FH status characterization is "free". All remaining democratization countries are recorded as "partial".[10] For completeness we include in the democratization data-set, countries that became democracies at independence from former Czechoslovakia, U.S.S.R. and Yugoslavia, although in many models we exclude them.

This methodology yields 62 permanent democratization episodes. 38 incidents are clas-

[8]Huntington (1993) summarizes eloquently this point "*...Stability is a central dimension in the analysis of any political system.*" Examples of brief (less than three years) spikes in the two democracy indicators that clearly do not represent the establishment of representative institutions, include Nigeria (in the early eighties), Congo (in the early nineties), Burkina Faso (in 1978-1979), and Argentina (in the early seventies).

[9]A sample selection problem might arise if democratic transitions are more likely to be short-lived if growth is weak. Yet this problem is unlikely to plague our estimates, since most democratic transitions that reversed back to autocracy were extremely short-lived (i.e. the country was in a democracy status for only one or two years); so there was not really time for agents to observe economic performance under representative government. Such concerns arise, however, for some countries that switched from non-democratic to representative government for some years and then reversed back. Pakistan, for example became democratic in 1985, but after ten years of representative rule switched back to autocracy in 1999. Other similar examples are Nepal and Guinea-Bissau. Our results are robust to either the inclusion or exclusion of these countries.

[10]Huntington (1993), who favours a binary distinction for political regimes, explicitly acknowledged the need for a distinction between "full" and "partial" political liberalization cases.

sified as "full" and 24 as "partial" democartizations. In addition in 5 countries some democratization has taken place, but the level of political liberties is still very low compared to Western standards ("Borderline Democratization" episodes). For completeness, we will present results both excluding and including these countries. So with a loose interpretation the democratization episodes become 67. Table 3.1 gives the country, the year of the regime change and a brief historical description of the event. The *Democratization Dataset Appendix A* gives a detailed description of the transition. It also reports the direction of the FH and Polity indicators before and after the event. All other data used in the growth regressions (GDP, investment, trade, government consumption, life expectancy) come from World Bank's World Development Indicators (2002 edition), while schooling statistics are retrieved from Barro and Lee (2001). The *Data Appendix B.1* gives detailed variable definitions and provides their sources.

3.3.2 Preliminary findings

Table 3.2 presents a descriptive analysis of growth and its determinants before and after political liberalization episodes. In the first three columns we explore the mean differences in the full sample of sixty-seven democratization countries. In columns (4)-(9) we investigate growth dynamics in "full" and "partial" (including "borderline") democratizations. The table also reports the mean difference and the p-value for the null hypothesis of mean equality before and after the event.

Mean growth rates do not exhibit notable discrepancies before and after the democratic transition. Yet, significant differences exist in countries that implemented moderate ("partial") reforms. The difference of growth rates after and before the emergence of democracy is non-negligible at 0.8% and statistically significant at the 90% confidence level. Interesting patterns emerge in the evolution of variables that typically serve as controls in growth regressions. First, investment and government spending remain stable, in spite of a vast political economy literature that models how redistribution policies lead to an increase in public spending and a drop in private investment. They are in line, however, with recent

cross-sectional evidence provided by Mulligan, Gil and Sala-i-Martin (2004) who challenge the fiscal redistribution channel. Second, democratizations are followed by a substantial increase in trade, schooling and health. Establishing causality, however, is not straightforward due to the trending behavior of these variables.

In Figure 3.2 we explore differences in growth volatility. Rodrik (1997, 1999) and Quinn and Woolley (2001), find that democratic societies experience less volatile growth rates. Likewise, the results show that the standard deviation of growth drops significantly from 5.5% to 4.9% after the transition. An F test suggests that we can reject the null hypothesis of standard deviation equality before and after the regime change at the 99% confidence level. Democratization (among probably other policies) has led to a significant decline in growth volatility in the highly (politically and economically) unstable African and Latin American countries. A substantial decline in growth volatility has also taken place in the high income countries that democratized. So, although, Spain, Portugal, and Greece experienced growth acceleration during their non-democratic era, democratization has brought some sizable gains in the form of lower output volatility.

3.4 Benchmark Estimation Technique and Results

3.4.1 Econometric Specification

To formally assess how democratization affects growth, we follow Jones (1995), Li (2002) and Bond *et al.* (2004) and consider the following autoregressive distributed lag specification (ADL):

$$\ln(y_{i,t}) - \ln(y_{i,t-1}) \equiv g_{i,t} = A(L)g_{i,t-1} + B(L)X'_{i,t} + \alpha_i + \eta_t + \varepsilon_{i,t} \tag{3.1}$$

where growth in country i in year t is defined as the logarithmic change in real per capita GDP. $A(L)$ and $B(L)$ are polynomials of order p and q in the lag operator (L) with roots outside the unit circle. $X'_{i,t}$ is a vector of growth determinants, such as investment, education, etc. α_i and η_t denote country and year fixed effects that capture unobserved individual

country heterogeneity and common global shocks respectively. The above expression can be more conveniently expressed in an error correction form:

$$\bar{g}_{i,t} = A(L)\bar{g}_{i,t-1} + B'(1)\bar{X}'_{i,t} + C(L)\Delta\bar{X}'_{i,t} + \alpha_i + \varepsilon_{i,t} \tag{3.2}$$

To eliminate the time nuisance parameters (η_t), g and X' are now expressed as deviations from period means.[11] $B'(1)$ isolates the long-run effect of covariates on growth. Throughout the empirical analysis, we report estimates with 2, 3 and 4 lags in growth and X', allowing for rich business cycle dynamics and investigating the sensitivity of our estimates to the exact specification.

3.4.2 The Unconditional Effect of Democratization

Before proceeding to the fully specified model, it is informative to ignore growth dynamics and the effect of other covariates on growth and quantify the unconditional effect of a permanent democratization. We, thus, begin our analysis by estimating the following regression equation:

$$\bar{g}_{i,t} = \alpha_i + \delta Democ_{i,t} + v_{i,t} \tag{3.3}$$

where $Democ_{i,t}$ is a dummy variable that takes on the value one in the year and in all years following a permanent democratization episode. Table 3.3 displays fixed effect estimates that isolate the within country effect of a permanent democratization. For comparability we also report estimates from a simple least squares regression (ignoring both the time and country fixed–effects).

The OLS specification yields an economically and statistically zero coefficient. This result is primarily driven by the cross country variation and given the existing literature

[11] Formally, we have $\bar{g}_{i,t} = g_{i,t} - \frac{1}{N}\sum_{i=1}^{N} g_{.,t}$ and $\bar{X}_{i,t} = X_{i,t} - \frac{1}{N}\sum_{i=1}^{N} X_{.,t}$. For a similar approach, see Caselli *et al.* (1996), Forbes (2000) and Bond *et al.* (2004). We use data from all available countries, as countries with no democratization incident can be used to more accurately estimate time fixed effects (see also Jones and Olken, 2005) .

comes as no surprise. In contrast, however, to previous studies, isolating the within effect of democratization results to a large and statistically significant growth effect. In the full sample of democratized countries, controlling both for individual country and time heterogeneity, the estimated coefficient on the political liberalization dummy is 0.0066, which is statistically significant at any conventional level. This implies that relative to the autocratic period, in a given democratized country, GDP grows faster by 0.662 percent during the democratic years.

Differences-in-differences estimators, however, exacerbate the downward bias in standard errors arising from the presence of positive error autocorrelation. Due to the non-inclusion of autoregressive terms, autocorrelation can seriously hamper inference. Following the solution proposed by Bertrand *et al.* (2004), we re-estimated all specifications adjusting standard errors based on a generalized White-like formula, allowing for country clustered heteroskedasticity and autocorrelation. Not surprisingly, standard errors increase, yielding higher p-values. Yet the democratization dummy is still at least two standard errors above zero.[12] The preliminary evidence suggests that, contrary to the documented zero relation between democracy and growth, political modernization does correlate strongly and significantly with the within country evolution of growth.

3.4.3 Democratization in Reforming Countries

Turning now to the conditional "within" effect of permanent democratization, we augment the basic ADL model with the democratization indicator variable. Since most variables typically used in growth regressions appear immaterial or have small in magnitude coefficients in the annual frequency, we report in this section ADL specifications including only lagged

[12]Using Monte-Carlo simulations, Bertrand *et al.* (2004) show that the clustering solution is appropriate when the panel dimension is around 50. We also implemented an alternative correction method to address the serial correlation problem. We regressed growth on country and year fixed effects, then divided and averaged the residuals into two groups, residuals before and after democratization, and then ran an OLS regression of the residuals on the democratization dummy, which appeared again with a significantly positive coefficient. We also corrected standard errors with the Newey-West method; the point estimates retain their statistical significance.

growth and investment ($\bar{I}_{i,t}$) in the RHS. Our exact specification takes the following form:

$$\bar{g}_{i,t} = \alpha_i + A(3)\bar{g}_{i,t-1} + B'(1)\bar{I}_{i,t} + C(4)\Delta\bar{I}'_{i,t} + \delta Democ_{i,t} + \varepsilon_{i,t} \tag{3.4}$$

The above specification enables us to quantify both the short and long-run effect of democratization.[13] We also control both for common business cycle fluctuations and unobserved country heterogeneity.[14] In the following Section we will present estimates controlling for other covariates.

Table 3.4 reports the results. In column (1) we report OLS estimates mainly for comparability with the other techniques. OLS will most likely yield biased and inconsistent estimates, even in the absence of individual heterogeneity, since investment is most likely endogenous. In column (2) we present IV estimates instrumenting both the contemporaneous level and first differenced investment.[15] The coefficient on contemporaneous investment, which was significant in the OLS becomes now indistinguishable from zero.[16] In the presence of individual country heterogeneity, however, both IV and OLS estimates are biased and inconsistent. Moreover, our aim to identify the dynamic within country effect of a successful democratic

[13] A significantly positive coefficient ($B(1) > 0$) on investment implies that the sum of coefficients in the investment polynomial in equation (1.2) is positive. This suggests that a permanent rise in investment will lead to a proportional increase in the steady-state of the growth rate.

[14] We also considered the stationarity properties of the endogenous and exogenous covariates. Specifically, we performed the Im, Pesaran and Shin (2003) unit root test for heterogeneous panels for growth, log level of GDP, investment and differenced investment (and also for the additional control variables, which we consider in the next Section). For growth (the dependent variable) and differenced investment, we can reject the null hypothesis of non-stationarity (with and without trend), at any conventional significance level. For investment we can reject non-stationarity if we rely on the version of the test that does not include a trend. For the log level of GDP the test fails to reject non-stationarity in both versions. Panel cointegration tests (e.g. Pedroni, 1999) between investment share and the log level of GDP indicate that these two variables are cointegrated when we allow for country specific trends. Given the poor reliability of these tests, we prefer to present results from a range of specifications that are theoretically grounded and at the same time do not violate the time series properties of the corresponding processes.

[15] Following Bond *et al.* (2004) we use in the set of instruments: $\bar{I}_{i,t-4}$, $\bar{I}_{i,t-5}$, $\ln(\bar{y}_{i,t-4})$, $\ln(\bar{y}_{i,t-5})$, $\bar{g}_{i,t-4}$, $\bar{g}_{i,t-5}$, lag 2 and 3 of inflation rate, trade openness and government consumption. The results are robust to alternative instrument sets, such as excluding government consumption or trade. The over-identification tests do not cast doubt on the validity of the instruments. We also explored the possibility of weak instruments. The first stage R^2 and the $t-$statistics of the instruments are much larger than those required by the weak instrument literature (Stock, Wright and Yogo, 2002).

[16] Jones (1995) finds no significant long-run effect of investment on growth. Attanasio *et al.* (2000) and Barro and Sala-i-Martin (1995) show that the causality runs from growth to investment. Li (2002), in contrast, documents some positive effects of investment on growth in OECD countries. Bond *et al.* (2004) find a significantly positive impact of capital accumulation on growth in a panel of 98 countries.

transition calls for panel techniques that exploit the time variation.

We thus proceed in columns (3)-(6) to fixed-effect estimates.[17] Although the joint presence of individual effects and the lagged dependent variable yields corrupt estimates, recent Monte Carlo studies show that the bias sharply decays when the time horizon exceeds 20 periods. Judson and Owen (1999) estimate that the bias on the lagged dependent variable is around 1 to 2 percent of the true coefficient value when T is 30 and between 2 and 3 percent when T is 20. More importantly, the bias on the other explanatory variables (and consequently the democratization dummy) is found to be less than one percent. We thus exclude from the specification in column (5) (as well as in many sub-sequent specifications) countries, where we have less than 20 time-series observations.[18]

The regression diagnostics and the coefficient estimates on the lagged growth and investment variables indicate no major mis-specification. Given the rich dynamics included in the specification, autocorrelation is absent. In addition, the over-identification tests in the IV estimates do not cast doubt on the instruments' validity.

The surprising result is the effect of democratization on growth, which is always positive and significant. According to the specification in column (5), which minimizes the bias arising from the joint presence of lagged dependent variable and the individual effects, a permanent democratic transition has a significantly positive short-run impact on output growth of 0.62 percent. The magnitude of the δ coefficient on $Democ_{i,t}$ is similar in the other fixed-effect estimates in columns (3) and (4), which estimate the model on the full sample of democratization countries and instrument for investment's endogeneity. The long-run effect is in general somewhat larger ($0.7 - 0.8$ percent), reflecting a certain amount of growth persistence.

A concern is whether the coefficient is picking up the market reforms that former socialist countries and new independent states that emerged after the collapse of the Soviet Union,

[17] We also estimated "random- effects" models. A Hausman (1978) specification test strongly rejects the no correlation of the country specific terms with the other explanatory variables assumption.

[18] A similar approach have been followed by the parallel studies of Giavazzi and Tabellini (2004) and Persson (2005).

Yugoslavia, and Czechoslovakia implemented in the nineties. In addition the data quality for these countries before 1990 is problematic. Thus in the model reported in column (6) we exclude these countries. To gain efficiency we now bring in the system all countries with available data, since non-reforming countries (that have been throughout the sample-period either always democratic or always autocratic) can provide additional information on growth dynamics. We continue to exclude countries with less than 20 years of observations. We also replace the time nuisance parameters with a simple linear trend. Finally we exclude investment, since one might be uncomfortable with investment in the RHS of a growth regression (even if instrumented). The estimates suggest a similar and if anything somewhat larger impact of democratization on growth of approximately 0.8 percent. [19]

This effect is not only at odds with the literature on political freedom and growth, but is also economically large. Its interpretation is, however, different than in previous studies. As mentioned above, previous work utilized methods that explored the cross-section variability. To assess whether a permanent democratization is associated with faster growth, we use a different sample, identify a discrete variable for democratization that minimizes measurement error and explore the "within" country variation. The estimates imply that in a given country that abandoned autocratic ruling and consolidated representative institutions, growth accelerated by approximately 0.7 percent faster relative to the absence of the regime change.

3.4.4 Dynamic Effects

Although the ADL specification enables reliable estimations of the long-run correlations between democracy and growth, this association might not be monotonic. To quantify this dynamic effect of democratization, we defined dummy variables for five non-overlapping three-year spaced periods around the transition and a dummy variable for the long-run effect of democratization. We then ran fixed-effects growth regressions on these dummies. Our exact specification reads:

[19] We provide additional results with all non-reforming countries as control group and various (regional and/or income-level) trends in subsequent sections.

$$\begin{aligned}\bar{g}_{i,t} &= \alpha_i + a_1\bar{g}_{i,t-1} + a_2\bar{g}_{i,t-2} + \beta_1\bar{I}'_{i,t-1} + \beta_2\bar{I}'_{i,t-2} + \pi\ln(\bar{y}_{i,t-3}) \\ &\quad \delta_1 D^1_{i,t} + \delta_2 D^2_{i,t} + \delta_3 D^3_{i,t} + \delta_4 D^4_{i,t} + \delta_5 D^5_{i,t} + \delta_6 D^6_{i,t} + \nu_{i,t} \qquad (3.5)\end{aligned}$$

We also include in the set of explanatory variables the logarithm of income per capita to control for the well-documented convergence effect. We also employ a more parsimonious specification with two lags of growth and investment and use lagged investment to (partly) address simultaneity. Turning to the democratization indicator variables, $D^1_{i,t} = 1$ in the fifth, fourth and third pre-democratization year, $D^2_{i,t} = 1$ in the second, first pre-democratization year and at the transition year (T^*); $D^3_{i,t} = 1$ at the first, second and third post-democratization years; $D^4_{i,t} = 1$ at the fourth, fifth and sixth post-democratization year; $D^5_{i,t} = 1$ at the seventh, eighth, and ninth post democratization year; finally $D^6_{i,t} = 1$ at the tenth and all subsequent post-democratization year. Each dummy equals zero in all other years than those specified. Thus the base period is the non-democratic years, excluding the transition period (i.e. from $T^* - 5$ backwards).

These results are given in Table 1.5. In column (1) we perform the estimation to the full sample of democratization countries. In columns (2) and (3) we exclude countries that implemented democratic reforms after 1998 (where not enough post democratization observations are available) and drop countries where democratization occurred alongside independence. Finally in columns (4) and (5) we exclude all countries where we lack data for more than 20 years to avoid the bias arising from the joint inclusion of country fixed-effects and the lagged dependent variable.

The estimates hint at some negative transition costs. These costs, however, appear small and insignificant, given the indistinguishable from zero coefficient on $D^2_{i,t}$. This is most likely due to the sizeable differences in transition. Some countries, for example moved to representative government with minimal costs, while others democratized in periods of turmoil and thus experienced huge output contraction. The estimates for the other indicator variables are consistent with Figure 3.1. The growth gains associated with a successful

democratic transitions become noticeable and significant after $T^* + 3$. The δ coefficients that measure the dynamic effect of reforms increase monotonically as democratic institutions stabilize. In all specifications the coefficients on $D^5_{i,t}$ and $D^6_{i,t}$ are highly significant and large. For example, in the last specification δ_5 is 0.093 implying that (controlling for business-cycle effects and convergence) growth accelerates on average by one percentage point faster in the medium-run (specifically during the seventh, eighth and ninth post-democratization year and compared to the autocratic years before the transition). As implied by the even larger coefficient on $D^6_{i,t}$, democratizations seem to have sizable long-run benefits. In all specifications reported in Table 3.5, δ_6 implies a growth impact of more than one percentage point after the tenth post-democratization year. The results are further strengthened by the estimates reported in the last column where we drop investment, ignore countries with less than 20 years of observation, skip former socialist nations, but bring in the system non-reforming countries as "control" group.

3.5 Further Evidence & Sensitivity Analysis

The results given so far stand in contrast to previous studies. A successful democratic transition is associated with considerable (particularly long-term) growth gains. This result is not sensitive to the exact dynamic specification or the sample. In this section we provide some additional evidence, checking at the same time for the sensitivity of our estimates. We start by augmenting our basic specification with other regressors suggested by the empirical growth literature. Second, we test for the robustness of our results when we allow for regime switching costs. Third, following recent political economy models, we embrace the possibility that democratization might be a strategic decision related to future growth opportunities and control for its endogeneity. Fourth, we address self-selection concerns arising from our event identification methodology and discuss measurement error issues. Fifth, we investigate through which channels the positive impact of political liberalization comes. We provide a variety of additional robustness checks, also studying growth in countries that moved from democracy to autocracy (adverse transitions).

3.5.1 Additional Growth Determinants

The empirical growth literature has considered numerous variables that explain differences in growth patterns among countries. While controlling for capital accumulation, human capital and public policy differences is vital in cross-sectional studies, it is less so in our annual frequency regressions. Typical growth regressors exhibit small annual within country variability. Consequently, part of their impact is captured by the fixed effects. Furthermore, only a few of these regressors survive extensive sensitivity checks (Sala-i-Martin, 1997; Levine and Renelt, 1992). We need, however, to control for omitted variables bias. By doing so we also place and interpret our results in typical cross-country growth framework.

We employ (for robustness) two conditioning sets. The first one (the "simple" set) is based on an augmented neoclassical model with human capital (see Mankiw, Romer and Weil, 1992), and includes: 1) the initial level of real per capita GDP, 2) average years of schooling (*Schooling*), 3) investment rate (I), and 4) population growth (POP). We also perform our estimation with a richer set of controls (the "policy" set), which includes in addition to the simple set: 5) trade, defined as imports plus exports as a share of GDP ($Trade$), and 6) government spending as a share of GDP (CG). Due to data unavailability on educational attainment for 22 democratization countries, we replace in the "policy" set the schooling variable with the log of life expectancy (LE). This enables us to use all countries with a democratization event. The estimates reported in Table 3.6 are based on the following model:

$$\begin{aligned}\bar{g}_{i,t} &= \alpha_i + a_1\bar{g}_{i,t-1} + a_2\bar{g}_{i,t-2} + \pi \ln(\bar{y}_{i,t-3}) + \beta_1\bar{I}_{i,t-1} + \beta_2\bar{I}_{i,t-2} + \beta_3\bar{I}_{i,t-3} \\ &\quad \beta_2\overline{HC}_{i,t-1} + \beta_3\overline{GC}_{i,t-1} + \beta_4\overline{POP}_{i,t-1} + \beta_5\overline{Trade}_{i,t-1} + \delta Democ_{i,t} + \varepsilon_{i,t} \quad (3.6)\end{aligned}$$

The above specification also enables us to isolate the long-run effect of a permanent democratization on income level $[\ln(y_{i,t})]$. Examining whether democratization exerts a significantly positive effect on output level is simply a test of $\delta/\pi > 0$.

Most of the control variables we consider are strongly significant and have well behaved coefficients. Yet, given the annual frequency, the coefficients' magnitude is small. Our preferred specifications in columns (5) and (6) imply a speed of convergence of around 3.5 percent. This is somewhat higher than the conventional wisdom of 2 percent, but is more theoretically plausible and in line with recent studies using annual observations.[20] The coefficient on schooling is positive, (marginally) significant, but small in magnitude. Although not very supportive to human capital models, it is in line with recent studies, showing that educational attainment is far from an ideal proxy for human capital. Life expectancy and trade enter with a significantly positive coefficient. Government consumption enters with a negative coefficient, in line with previous studies, which suggested that it captures inefficiencies caused by lofty taxation and inefficient state activities.

Most importantly, the coefficient on the political liberalization dummy is positive and at least two standard errors above zero in all perturbations. Its size almost doubles when we move from OLS to fixed effects. This illustrates the significant gains from the utilization of the "within" country variation. Conditioning on the rich "policy" set and performing the estimation in either the full sample of sixty-seven democratization countries (column (4)) or in the sample of forty-eight countries, where more than 20 yearly observations are available, yields a significant democratization coefficient of approximately 0.01. Quite interestingly the positive impact of democratic reforms does not seem to come either from human capital accumulation or from increased trade, since the δ coefficient remains stable when we add these controls. This suggests that besides the standard-documented channels, democratization impacts growth through other mechanisms.

Turning now to the dynamic specification, we estimate the following specification (which is a variant of equations (5) and (6)) allowing the effect of democratization to differ during the transition and controlling for other covariates:

$$\bar{g}_{i,t} = \alpha_i + a_1 \bar{g}_{i,t-1} + a_2 \bar{g}_{i,t-2} + \pi \ln(\bar{y}_{i,t-3}) + \bar{X}'_{i,t-1} B + \sum_{j=1}^{6} \delta_j D^j_{i,t} + \varepsilon_{i,t} \tag{3.7}$$

[20] Hauk and Wacziarg (2004) provide a careful and insightful analysis on the underlying reasons behind the documented differences in estimates among different panel methodologies in growth regressions.

The indicator variables ($D^j_{i,t}$) are defined as in equation (5). Figure 3.3 plots the δ_j coefficients on the democratization dummies, when we condition on the rich "policy" set. The figure suggests a J-shaped growth pattern with output costs around the transition, but with sizable gains after the consolidation of representative institutions. The effect of democratization becomes statistically significant after the third post-democratization year, where $D^3_{i,t}$ is 0.008 (t-stat.= 2). The democratization effect is magnified after the seventh and especially after the tenth post-democratization years. For example δ_6 is 0.012, suggesting a long-run effect greater than 1.2 percent.[21]

3.5.2 Transition Costs

Figures 3.1 and 3.3 reveal the presence of sizable switching costs. Haggard and Kaufman (1995) document that many countries democratized either during a financial crisis or after the end of an armed conflict (for example in Argentina after the Falklands War). If countries democratized in the middle of an economic slowdown, then these countries might experience growth even if they hadn't implemented the reforms. We therefore need to control for the recovery, since this can yield an upward bias to the coefficient on democratization (this is similar to the Ashenfelter's dip argument in the program evaluation literature). Even though in the previous subsection we allowed for different growth dynamics during the reforms, we further address this concern by defining a transition indicator variable ($Trans_{i,t}$) that takes on the value one in five-year period of turmoil and uncertainty around the regime change ($Trans_{i,t} = 1$ if $T^* - 2 \leq t \leq T^* + 2$).[22] This also enables us to control for measurement error on the exact timing of reforms. The democratization variable ($Post\_Democ_{i,t}$) is adjusted accordingly and now takes the value one in the third and all subsequent post-democratization years ($Post\_Democ_{i,t} = 1$ if $T^* + 3 \leq t$).

The results are reported in Table 3.7. We continue to control for other growth determinants and unobserved time heterogeneity. To show that the estimates are not driven by

[21] The estimated coefficient on $D^6_{i,t}$ is in line with the optimistic belief of Peace Nobel Laureate Frederik Willem de Klerk. In a recent article ($14 - 9 - 2004$) on the *Wall Street Journal*, de Klerk argued that the benefits of democratization should be expected after the elapse of the first post-reform decade.

[22] Changing this requirement to three or seven years does not change the results in any significant way.

the exact specification, we use a more parsimonious model with only two lags of growth and investment. We perform further checks for the sensitivity of our results by adding each of the controls one by one. We also exclude in columns (7), (8) and (9) "borderline" democratization countries and countries that democratized after 1998. The coefficients on the control variables are not particularly sensitive to the specification and still enter with well-behaved coefficients, implying no serious model mis-specification. The coefficient on $Trans_{i,t}$ is small and insignificant, reflecting most likely the substantial volatility of growth around the transition period, which produces large standard errors.[23] Most importantly in all specifications, regardless of the sample used, the coefficient on $Post\_Democ_{i,t}$ is positive and significant. The estimates imply that, controlling for transition costs, the consolidation of democratic institutions leads to an almost 1% long-run growth acceleration.

3.5.3 Endogeneity

Is the decision to democratize exogenous or a consequence of high growth prospects? Both theory (Aristotle; Lipset, 1994) and empirical studies (e.g. Barro, 1999) suggest that democracy is endogenous to income level rather on growth. However, to investigate the sensitivity of our estimates to the potential endogeneity of democratization, we fully specify a limited dependent variable (LDV) model on $Democ_{i,t}$. We then form fitted probability values and insert them in a second stage fixed-effects growth regression (Alesina *et al.*, (1996) employ a similar approach to quantify political instability's impact on growth). To accurately pin down the probabilities of democratization, we estimate the model in all countries that were non-democratic at the beginning of the sample.

For the "treatment" equation we rely on the literature on political institutions' determinants (for a review, see Acemoglu and Robinson, 2004). We obtain predicted values for $Democ_{i,t}$ using: 1) Years since independence as a proxy for history's role in the evolution of political development (for the theoretical basis on the use of this variable see Acemoglu, *et al.*

[23] Democratic transitions differed substantially across countries. For example, in Latin America countries the democratic transition period was marked by severe recessions. Yet, other countries (e.g. Spain, Portugal) democratized with minimal transition costs, probably at the peak of the business-cycle. Adding a dummy for the transition period is surely a rough way to quantify the costs of transition.

2005); 2) Fractionalization, measured by the probability that two randomly selected individuals belong to different ethnical and religious groups (see Aghion, Alesina and Trebbi, 2004 for a formal model of the effect of polarization on political institutions). 3) Religion, measured by the share of the population that follows Islam and Confucian/Buddhist ideology. 4) Regional dummies to control for the potentially non-random selection of democratizations and spillover effects from adjusting countries. 5) Lagged income level or/and schooling to capture the strong wealth-democracy nexus (e.g. Epsetin *et al.*, 2004). 6) Oil abundance to estimate the effect of the "natural-resource curse" (e.g. Ross, 1999). 7) Trade openness, to capture feedback effects of economic to political freedom. 8) Finally, in all specifications we include a linear time-trend to control for the trending behavior of the democratization variable and some of the independent variables. We thus estimate the following system with a democratization selection equation (the first-stage) and a classical growth regression (the second-stage):

$$Democ_{i,t} = \begin{cases} 1 & if \quad G(Z_i^1 + Z_{i,t}^2) + e_{i,t} > 0 \\ 0 & \qquad otherwise \end{cases} \tag{3.8}$$

$$g_{i,t} = a_1 g_{i,t-1} + a_2 g_{i,t-2} + \pi \ln(y_{i,t-3}) + X'_{i,t-1} B + \widehat{Democ}_{i,t} + \alpha_i + \eta_t + \varepsilon_{i,t} \tag{3.9}$$

The time invariant first-stage variables (Z_i^1) enable us to identify the countries that eventually abandoned autocratic rule (compared to countries that have remained throughout the sample period non-democratic), while years since independence, trade openness and lagged income ($Z_{i,t-1}^2$) are mainly correlated with the timing of the event.[24]

In line with recent studies on the determinants of democratization, all variables of the Z vector enter the first-stage probit model with significant and theoretically-consistent coefficients. The estimates, for example, imply that democratization is blocked in religiously heterogeneous countries, while political modernization is more likely to emerge and consolidate in countries open to international trade. Moreover, democratization is more likely in

[24] Although empirical growth studies have documented that some of the time-invariant (Z_i^1) variables are directly significantly associated with growth (e.g. Alesina *et al.* 2003 on the role of fractionalization on growth), this does not influence our specification, since we add country fixed-effects. Time-invariant controls only help us to distinguish countries that enter our sample as non-democracies, but which eventually abandoned autocratic rule.

rich, but not oil-producing countries. The fit of the probit specifications is good with Mc Fadden's pseudo-R^2 at the range of 0.30 and the ratio of observed to predicted outcomes around 0.80.

Table 3.8 reports the estimates of the growth specification. To further investigate the robustness of our estimates, we present various models, changing both the set of growth determinants in the second-stage (X) and the set of independent variables in the "treatment" regression (Z). We estimate the first-stage model with maximum-likelihood. Although assuming error normality MLE yields consistent and efficient results, it can be sensitive to intractable problems, like autocorrelation and individual effects. We thus experiment (in columns (2) and (5)) with Heckman's (1978) two-step method, which requires less restrictive assumptions, but with a loss of efficiency.

The estimated correlation coefficient (ρ) between the residuals of the "treatment" model and the growth regression is negative at a range of $0.13 - 0.23$. Although ρ is not always statistically below zero, this explains why the Heckman correction methods yield larger coefficients on $DEMOC_{i,t}$. For example, the fixed-effects specification reported in column (6), where we condition on the "simple" set and predict democratization using years of independence, the share of Muslims and Confucians in the population, and religious and ethnical heterogeneity suggests that democratizations are associated with a more than two percent increase in GDP growth. ($\delta = 0.0219$). The magnitude of the estimated coefficient on $\widehat{DEMOC}_{i,t}$ in the other specifications is similarly at a range between $1.7\% - 2.7\%$.

Clearly the Heckman correction analysis does not fully resolves the endogeneity and reverse-causality concerns; in addition the point estimates, although in line with our previous pro-democracy evidence, imply unrealistically high effects of reforms. Yet, together with theory and parallel studies that have not detected a significant effect of growth on democratization, it strengthens our confidence that the results are not being driven by selectivity and endogeneity.[25] Furthermore, an interesting interpretation of these estimates

[25] We further experiment with numerous other variables in the "treatment" equation (e.g. mortality rates, urbanization, etc.), without detecting any major differences. In most specifications, the fitted democratization values enter the second stage growth regression with a significantly positive coefficient. Although one could clearly produce first stage specifications that yield an insignificant (yet still positive) coefficient for the

is that democratization is particularly beneficial, when the theoretical conditions required for its emergence and consolidation are favorable (i.e. when the estimated probabilities of democratization are high, growth accelerates). If this interpretation is valid, then one should not expect democratic reforms to always yield growth gains. Growth accelerates probably not only due to the reforms, but because the conditions are favorable for the development of democratic institutions.

3.5.4 Alternative Democratization Events & Measurement Error

A possible critic is that self-selection of both the democratization countries and the transition year drives our results. Since we identify which country and in which year an autocratic regime is abandoned, one could argue that this is done in such a way that favours our empirical results. Although we consider the identification of political liberalization as a key ingredient of our research, we performed various modifications to our sample based on different definitions of democracy.

Table 3.9 reports our results for alternative democratization samples. First, we re-estimate our basic specification excluding "borderline" and recent (post-1998 or post-1995) democratization events where insufficient observations are available after the regime change. The estimated coefficient in the specification in column (2) implies that democratization has a more than 1% effect on growth.

Second, we identify a democratization relying solely on either of the two widely used political freedom indicators. This changes both the exact timing and the country sample of democratic transitions.[26] Specifically, we identify a permanent regime change when the Polity index jumps from a negative to a positive value and when the Freedom House characterization jumps from "Not Free" to either "Free" or "Partially Free" and remains there for five years.[27]

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democratization variable, a successful democratic transition still has a positive impact on economic growth.

[26]For example, according to the Polity index democratization occurred in Ghana in 1996, while according to the Freedom House it occurred in 1992. According to our methodology, the timing of democratization is in December of 1996, when the first, internationally declared "free and fair" elections took place.

[27]A similar threshold is used in the parallel studies of Persson (2005) and Giavazzi and Tabellini (2004). Persson points out that using the median of the polity index as the threshold is the most appropriate, since most reforms are "*in accord with conventional views of political history*". In contrast Rodrik and Wacziarg

We also exclude new independent states that emerged after the fall of the Iron Curtin.

These results given in columns (4)-(6) illustrate why our findings differ from previous studies. Following Robert Barro's influential work, the literature so far has examined the democracy-growth link using panel methodologies with five years averages that primarily utilized the cross-country variation. In addition, these studies primarily used the FH ratings as a measure of democracy.[28] The almost zero and statistically insignificant coefficient in column (4) is thus in line with previous work. When we explore the "within" country variation in countries that experience a democratic transition, $Democ_{i,t}$ asserts a significant impact on growth. Yet when we bring in the system non-reforming countries as a "control group", exclude the former socialist republics and countries with less than 20 observations, the impact of democratization according to Freedom House becomes zero. In contrast when we employ the Polity ratings then democratization is always positive and significant. This suggests that measurement error is a serious drawback of the previous cross-sectional work.

3.5.5 "Control Groups" and Differential Effects of Political Reforms

The analysis so far has primarily examined growth dynamics in countries that liberalized their political system. This approach has the benefit of examining a homogeneous group of countries; yet, we do not utilize information from the two non-reforming groups. To further investigate the stability of the previous estimates we now focus on the universe of countries. The identifying assumptions are, however, somewhat restrictive: First, we assume that democratizations are not systematically related to other omitted factors that directly affect growth (and are thus imbedded in the error term). Second, and most importantly we

(2004) employ the Polity transition coding, which requires a jump of greater than 3 points in the 21-range index. Consequently, Rodrik and Wacziarg identify many minor efforts towards democratization or temporary incidents of political instability in autocracies. For example according to this definition Cambodia experienced a democratic transition in 1972, when the Polity index jumped from -9 to -5. Likewise Chad had a democratic transition in 1994 when the index jumped from -7 to -3 and Iran in 1982 when the index jumped from -10 to -6.

[28] Munck and Verkuilen (2002) show that among the available democracy measures the FH measure is the most problematic. They show a systematic bias against: 1) communist regimes, 2) left-wing governments, and 3) countries not open to international trade.

assume that the same growth dynamics govern democratization countries with non-reformer nations.[29] The results of the following model are given in Table 1.10:

$$g_{i,t} = \alpha_i + a_1 g_{i,t-1} + a_2 g_{i,t-2} + a_3 g_{i,t-3} + \pi \ln(y_{i,t-4}) + \delta Democ_{i,t} + \sum_{k=1}^{K} \lambda_k t_k + \varepsilon_{i,t} \quad (3.10)$$

where we have replaced the year constants with a k vector of regional and/or income-level trends (t_k).[30] This enables us to capture any pre-reform existing dynamics and to control for the non-stationarity nature of the democratization dummy.[31] This approach also (partly) controls for the over-representation of some regions in the reforming group.[32] Besides new independent states, we also now exclude all former socialist countries (like Albania, Bulgaria, Poland, etc.) as well as all countries with less than 20 observations. The results are reported in columns (1)-(2). The impact of democratization on growth is estimated to be around $0.6 - 07$ percent, very much in line with our benchmark estimates.

Regional Effects

It is worthwhile to examine in which countries political reforms had the largest impact. In addition, in spite of the inclusion of regional and income-level trends, there is still the concern that a particular group of countries drives our results. For example, Latin America

[29] Thus we impose that investment, trade, government spending, etc. exert a similar growth effect to all three country categories, always democratic nations (like the United States or Western European nations), always autocratic (like most Sub-Saharan and Asian nations) and the countries that experienced a successful democratization. This assumption, therefore, seems quite restrictive. Mancur Olson (1993), for example, has specifically claimed that democracies differ fundamentally from autocratic countries and thus factors that influence economic development in democratic environments need not have the same effect under non-representative institutions. Therefore we only report parsimonious models with only lagged income terms. We note, however, that the results in Table 10 are robust to the inclusion of standard growth determinants.

[30] Following the World Bank country classification we have seven regions: 1) East Asia and Pacific, 2) Europe and Central Africa, 3) Latin America and Caribbean, 4) South Asia, 5) Sub-Saharan Africa, 6) Middle East and North Africa, and 7) High Income. We also get five income groups: 1) High Income-OECD, 2) High-Income Non-OECD, 3) Higher Middle-Income, 4) Lower Middle Income, 5) Low Income.

[31] Note that by construction $Democ_{i,t}$ exhibits a trending behavior, since after a successful transition to democracy countries do not revert back. Even if we hadn't imposed the stability requirement, reversals are very rare, so this problem is not resolved. Thus by adding the trends in the RHS we explicitly control for this concern. Panel-unit root tests in $Democ_{i,t}$ strongly reject the presence of non-stationarity when a linear trend is added.

[32] In a previous version of the paper we also added regional dummies, decade constants and even decade-region fixed effects. The results do not depart in any significant way.

countries experienced fast growth after the debt crisis in the early eighties, which coincided with many democratization episodes. A similar concern is whether the results are driven by the three European Union member countries (Spain, Portugal and Greece). These countries received substantial political and economic assistance from the European Union (through the regional and structural funds) after the democratic transition.

We thus report in columns (3) and (4) estimates of the following dynamic growth regression, which allows the effect of democratization to differ in Latin America and EU countries:

$$\begin{aligned} g_{i,t} &= \alpha_i + a_1 g_{i,t-1} + a_2 g_{i,t-2} + a_3 g_{i,t-3} + \pi \ln(y_{i,t-3}) + \sum_{k=1}^{K} \lambda_k t_k + \\ & \delta_1 [Democ_{i,t} * Latin_i] + \delta_2 [Democ_{i,t} * Eur_i] + \\ & \delta_3 [Democ_{i,t} * (1 - Latin_i - Eur_i)] + \varepsilon_{i,t} \end{aligned} \tag{3.11}$$

$Latin_i$ takes on the value one if country i is in South America (or in the Caribbean) and zero otherwise. Likewise Eur_i equals one for the three EU15 countries that democratized in the mid-seventies. There are no significant differences between the estimated growth impact of democratization in Latin America and the other (non Latin American and non EU15) reform countries. If anything, controlling for other determinants, the effect of political reforms on growth was somewhat milder in Latin America. Turning now to the three European countries, given the fast growth rates that all these countries experienced during their non-democratic era, growth is not significantly higher (but actually lower) after the regime change. Most importantly for our analysis, the δ_3 coefficient on the other democratization country groups is still significantly positive and quite robust, implying an effect of approximately 1% additional growth at an annual basis.

"Full" and "Partial" democratizations

It is interesting to compare growth rates between countries that "fully" as opposed to "partially" consolidated democratic governments. The descriptive evidence point out that countries with a partial democratization experienced weak growth before the transition and thus gained the most from the regime change. In columns (5) and (6) we further investigate these differences, estimating equation (9), but allowing the effect of "full" and "partial" reforms to differ.

The results are somewhat contradictory. When we estimate a simple autoregressive model with no convergence term, the estimates suggest that "partial" reforms appear to yield the largest growth gains. This result is in line with Barro's (1996, 1997) non-linear finding that growth accelerates when a country moves from total autocracy to intermediate levels of political freedom, but future progress to a perfect democracy level is associated with slower growth. The estimates in column (6) complement this evidence, but from a "within" standpoint, which we believe is more important from a policy perspective. Yet when we add the lagged income term to control for convergence, then the coefficient on "full", rather on "partial" reforms dominates. Although this is in line with the pro-development evidence given so far, we are sceptical on this interpretation for three additional reasons: First, there are some endogeneity concerns, since richer countries implemented large reforms (like Chile, Spain, Portugal), while poorer nations only "partially" liberalized their polity (like Bangladesh, Nicaragua, Zambia). Second, most "partial" reforms occurred in the nineties and consequently we do not have as many post as pre-democratization observations. Third and most importantly given the conceptual challenges in defining democracy, the line of separation between "full" and "partial" democratizations is not always crystal-clear. Yet, together with the descriptive evidence, we believe that these results provide optimism for countries that implement moderate reforms.

"High" and "Low" Human Capital

It is possible that countries with a high level of human capital will manage to benefit more form the political reforms. Formal political economy models suggest that democracy is indeed most likely to yield positive effects in educated societies. Likewise the previous two-stage results in Table 3.9 hint that reforms have the greatest impact in countries, where the theoretically required preconditions for democracy (one of which is education) are present. We thus split the democratization countries to two groups depending on the average years of schooling in the population. We first estimated the median of the schooling variable in all countries (4.24 years) and then allow the δ coefficient on the democratization dummy to differ in the above and below schooling median (these countries are given in the *Data Appendix*). The results are presented in the last two columns of Table 3.10. Countries with high levels of human capital clearly benefit the most from the new political equilibrium. We estimate the impact of political reforms in countries with relatively high level of schooling to be close to 1% and statistically significant at the 99% confidence level. In contrast countries with low levels of human capital do not experience faster growth after the reforms. A Wald test of coefficient equality suggests that the coefficient difference is statistically significant at standard significance levels. These results demonstrate that democratization will not automatically bring growth. There appears to be a strong complementarity between political liberties and human capital. Thus political liberalizations are expected to have a significantly positive impact on growth only if they take place in countries with high levels of schooling or accompanied with investment in education.

3.5.6 Reversals from Democracy to Non-democracy

A potential shortcoming of our analysis is that we study only regime changes from autocracy to democracy. An obvious critic is that a transition from democracy to autocracy might also yield positive growth, most likely because countries abandon a regime of any type if economic performance is poor. If growth rates accelerate after the transition from democracy to autocracy, then all our previous results are seriously challenged. In this case a transition of

any kind is associated with faster growth. Yet during the period of our study, there are very few such (stable) adverse reversals. These are: Gambia, Lebanon, Sierra Leone, Zimbabwe, and Somalia (a brief description of political developments and the evolution of the democracy indicators for these countries is given in the *Democratization Dataset Appendix A*). One could also add in the list Pakistan and Fiji.

In spite of the small sample and data unavailability, we examined in detail the pattern of growth in these countries. Statistical tests of mean and volatility equality in growth rates not only do not contradict, but actually strengthen our previous results and further support development theories. A simple t-test of mean equality in growth rates during the democratic and the autocratic years suggests that during the democratic period these countries grew by on average 1.09%, while during autocracy experienced negative growth rates of -2.3% on average. Despite the low number of observations, the almost 3.5% percent difference is not only economically sizable, but also statistically significant at the 95% confidence level. Furthermore, output volatility in the democratic period was almost half that of autocratic years (4.65% and 9.61% respectively). The conclusion is that countries that had stable representative institutions and switched to non-democratic polities experienced not only slower, but also significantly more volatile growth. This result, which adds to previous evidence of Minier (1998), re-enforces our previous pro-democracy estimates and offers reassurance that our estimates do not pick a change-to-new regime effect.

3.5.7 Additional Robustness Checks

We performed several other permutations of the basic model to check for the stability of the democratization coefficient, which always appears in the range of 0.5 to 1.2 percent. We moved the democratization event window. We conditioned on other typically employed growth regressors, such as inflation, the price level of investment, mortality and fertility rates. We excluded all democratization episodes that occurred after 1990 or 1995, since one could reasonably argue that no adequate post-democratization observations are available. No significant departures from the results reported so far were present.

In the previous version of this paper we estimated a dynamic growth model only for the autocratic years, obtained forecasted growth rates for each of the subsequent democratic years and then compared the realized growth rates with the projected values. This approach allows for different growth dynamics during the autocratic period. Not only did democratization appear to have a similar positive impact of around 1 percent, but its dynamic evolution was similar to the results given in Table 3.5 and Figure 3.3.

In Tables 3.11 and 3.12 we provide additional regression results, ignoring growth persistence. OLS, which primarily utilizes the between variation give the smaller coefficient estimate, followed by the "random" effects estimates, which utilize both the between and within variation. The striking feature is again the increase in the estimated coefficient for democratization resulting from fixed-effects regressions. The within estimators suggest a 1 percent impact of $Democ_{i,t}$ on growth. We also allow for dynamic effects and report growth regressions with the Arellano and Bond difference and the Arellano-Bover (1995) and the Blundel-Bond (1998) system GMM estimators. These estimation procedures allow for panel heterogeneity and the inclusion of endogenous variables. In spite of the documented drawbacks of these methods, we report in Tables 3.11 and 3.12 estimates of this and the more efficient, newly developed system GMM technique (Bond, Hoeffer and Temple, 2001). We report estimates of both the one and the more efficient two-step GMM procedure (the latter with corrected standard errors, following Windmeijer (2004)). Although the point estimates are not stable, the effect of democratization on growth was found to be positive and significant in most specifications. The GMM estimates imply a disproportionately large positive annual growth effect of democratization of more than 2 percent. We are, however, skeptical on the interpretation, since the coefficient estimates of the GMM methods are not very stable. Although these methods yield imprecise estimates, the coefficient on democratization is always at a positive range.

3.6 Summary and Conclusion

The relationship between the type of polity and economic performance is probably the oldest and one of the most controversial issues in the social sciences. Although there has been a burgeoning theoretical and empirical research on the relation between political liberalism and economic performance, both the economics and the political science literature lack a comprehensive analysis of the effects of democratization on growth. After identifying recent episodes of permanent democratic transitions, we analyze the dynamic evolution of growth before and after democratization. Our methodology addresses issues arising from time-invariant omitted variables, unobserved country heterogeneity, reverse causality and measurement error, and primarily explores the within country growth variation. Our before-after event study clearly challenges the recently documented insignificant correlation between political freedom and economic growth.

To quantify the effect of a successful democratization, we focus on countries that liberalized their polity during the so-called Third Wave of Democratization. We augment a standard growth regression with an indicator variable for the democratic years. Using annual frequency data and employing various dynamic panel data techniques, we first show that democratization leads to an almost one percent increase in annual real per capita GDP growth. Second, our analysis reveals that during the transition period growth drops substantially; it stabilizes, however, at a higher rate after the transition. Third, the post-war democratization experience suggests that countries with high-levels of human capital manage to gain the most from the new democratic regime. These results withstand a wide range of sensitivity checks including: alternative ways of identifying democratized countries, the exact timing of the episodes, various control sets, different panel techniques, correcting for the endogeneity of democratization with Heckman-type treatment effect methodologies, allowing for pre-existing trends or common business cycle effects, and more. The pro-democracy evidence is also strengthened by the descriptive analysis of countries that switched from representative institutions to autocracy, which on average experienced slower growth after the adverse regime change.

Our findings have important implications. From a theoretical standpoint, they offer direct support to "development theories" of democratic government that stress the beneficial aspects of representative institutions (i.e. through lower conflict resolution costs, less political instability, and the growth-enhancing aspects of redistribution). From a policy perspective, our results suggest that even moderate reforms can have sizable positive effects. They also suggest an important role for the international community: it should help mitigate the transition costs, which can be substantially high.

The results, however, do not imply that democracy is a panacea. Our methodology and research objective was to quantify an average within country growth effect of democratization. Clearly country-specific deviations from the average exist. Influential scholars (Huntington, 1993) have argued, for example, that democratic norms might not be suitable for all countries. The Heckman correction estimates are in line with this interpretation, since the effect of democratization is high when the conditions for its emergence and stability are favorable (and thus the predicted first-stage probability of a country being democratic is high). Our results clearly point out that the effect of political liberalization is the highest, when reforming countries have high-levels of human capital. Moreover, we want to stress that our analysis examined countries that not only moved to representative government, but also managed to consolidate democratic institutions. It should be clear that short-period democratic intervals and political instability can decelerate growth. In addition we didn't distinguish between different types of autocracy (e.g. left or right wing dictatorships) and democracy (presidential or parliamentary).[33]

The identification of democratization episodes in the period 1960 – 2000 and our event-study approach offer a basis for further research in the inter-relation between political freedom and economic development. First, political economy theories model how income inequality influences the democratization process. They also foretell a decline in the level of inequality after the consolidation of representative institutions. It is thus worthwhile to check

[33] In a recent study Persson (2005) shows the income gains following a democratization are high, when the transition leads to a proportional representation (versus a majoritarian) or a parliamentary (versus presidential) system. In the very latest revision of his work, Persson also documents that short-lived democratic transition have a negative impact.

for their validity and examine how democratization affects the distribution of income within democratized countries. Second, future work is needed to assess the impact of democratization on public policies and the accumulation of physical and human capital. Our descriptive analysis hints that no major differences are evident. Yet clearly more work is needed. Third, together with the fast-growing work on the prerequisites of political institutions, the results offer new basis for further theoretical work clarifying the channels of interrelation between income inequality, wealth, democratization and economic growth. The positive effect, for example, of democratization might be far greater if it is associated with other policy reforms, such as trade or financial openness.[34]

[34]In a parallel, independent and complementary work, Giavazzi and Tabellini (2004) document significant interactions between economic and political liberalization. Using a methodology similar to ours they show that countries experience substantial growth gains when they first liberalize the economy and then the polity.

Chapter 4

What Drives Democratization?

4.1 Introduction

As shown in Chapter 3 democratic institutions have a significantly positive "within" effect on growth. In addition numerous other studies reveal that the type of political institutions, representative or non-democratic, crucially influence economic development and public policies. There is quite surprisingly very little empirical evidence on the factors that shape the political equilibrium itself. In this Chapter we proceed a step further and ask what determines democratic transition. This is key in understanding the deep determinants of other institutional structures (like property rights, legal quality, corruption, bureaucratic quality), which correlate highly with democracy. Specifically we identify which economic and social factors are behind the recent unprecedented wave of "*global democratic revolution*" that Samuel Huntington (1993) has coined as the "*Third Wave of Democratization*" (henceforth the 3rd Wave).[1] We analyze the countries that had non-democratic governments at the beginning of the 3rd Wave (1960) and explore *why* some of them moved to representative institutions, while others failed to do so. For example, Spain, Argentina, and South Africa abandoned autocratic rule and consolidated democratic institutions. Other countries, such

[1] According to Huntington (1993) "*.. the first wave began in America in the early nineteenth century and culminated at the end of World War I with about thirty countries having democratic regimes. Mussolini's march in Rome in 1922 began a reverse wave, and in 1942 there were only twelve democracies left in the world. The Allied Victory in World War II and decolonization started a second movement towards democracy, which, however, pattered by the early 1960's when about thirty-six countries had democratic regimes....The Third wave began....on Thursday, April 25, 1974 in Lisbon, Portugal.*"

as those in the Middle East and Africa, however, are still ruled by oppressive regimes. In addition, we identify the factors that influence *when* democratic transitions are more likely to occur. Our contribution is, therefore, to identify and quantify *leading indicators for both democratic transitions and permanent democratizations.*

Where and *when* representative institutions are more likely to emerge and consolidate? This question has so far been largely ignored by economists, which is surprising for many reasons. First, the theoretical work on the political economy of inequality (see Person and Tabellini (2000) for a thorough review) suggests that the trade-off between efficiency and redistribution is fundamentally influenced by political organization. Second, along with the evidence given in Chapter 3 ample additional studies (e.g. Tavares and Wacziarg (2001); Mulligan, Gil, and Sala-i-Martin (2004); Rodrik (1999); Quinn and Wooley (2001); Persson (2004)) indicate that public policies and growth patterns differ substantially between democratic and non-democratic countries. Third, the strong correlation of democracy with high quality legal institutions, low instability and corruption implies that understanding what drives representative norms may also shed light on which factors shape other types of institutions. This is vital since recent studies (see Acemoglu, Johnson and Robinson, 2005 for an extensive review) have shown a strong causal effect of institutional quality on long-run economic development. Fourth, most theories on the prerequisites for democratization advocate economic conditions as the prevailing factor. Recent theoretical work on the political economy of development (e.g. Acemoglu and Robinson, 2000, 2001, 2005) models how economic factors, such as income level, output growth and natural resource abundance, affect the type of political organization, democracy or autocracy, and consequently economic development. Fifth, form a policy perspective it is key not only to provide recommendations that institution χ or y can be growth enhancing, but also to understand how such an institutional arrangement can be established.

This Chapter provides large-sample evidence that democratization is more likely to occur in affluent and educated societies, thus offering support for the traditional "*modernization hypothesis*" (Aristotle; Lipset, 1959, 1994). It also documents that natural resource abundance, religious fractionalization and reduced exposure to international trade impede the

likelihood of democratization. We also identify *in which periods* democratization is more likely to emerge and stabilize. The analysis suggests further that a recession (a currency or a banking crisis) as well as the ending of an armed conflict strongly increase the likelihood of a democratic transition. In line with the belief of many liberal economists (Friedman, 1962; Hayek, 1960) the results indicate that political modernization is more likely after (or jointly) with economic (trade) reforms. We also clarify some theoretical issues on the inter-relation between income inequality and democratization. Finally, we exploit the "*quasi-natural*" experiment of independence and liberalization that occurred after the Iron Curtain fall to test the main democratization theories. Specifically, we examine those countries that emerged after the collapse of the U.S.S.R., Yugoslavia and Czechoslovakia as well as other formerly centrally planned economies to validate our findings.

This Chapter's work is mostly related to the literature that examines the determinants of political freedom and institutional quality. Barro (1999) reverses the democracy-development nexus and regresses democracy measures on the initial income level and along with various other factors. His panel regressions reveal that wealth and human capital are the key determinants of the long-term level of civil liberties and political rights. Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004) show that these results apply more generally and that human capital is thus the key determinant of institutional quality broadly defined. The political science studies (Bollen and Jackman (1985, 1995); Muller (1995); Epstein, Bates, Goldstone, Kristensen, and O' Halloran (2004)) have primarily examined the determinants of long-run changes (not levels as in Barro, 1999) in democracy, emphasizing besides wealth the role of income inequality. These studies have, empirically established a strong high income-democracy nexus. Yet, even this empirical regularity has been recently challenged by Acemoglu, Johnson, Robinson, and Yared (2004, 2005) and Przeworski, Alvarez, Cheibub, and Limongi (2000), who, using different methodologies, document that short-run increases in income do *not* increase the likelihood of democratic transitions.

This Chapter has a different objective and thus departs significantly from these studies. Instead of focusing on either the long-run level or change in democracy, we first identify the nature and exact timing of a permanent democratic change. We then employ inherently

probabilistic models using annual frequency data to analyze the impact of both short-term economic conditions as well as long-term socioeconomic fundamentals on democratization.[2] We identify the systematic driving forces behind permanent democratizations, distinguishing between socioeconomic factors that influence the *emergence* of democracy and factors favoring its *stabilization*. Thus, we look solely at those countries that were non-democratic before the 3rd Wave began. This allows us to isolate and quantify as precisely as possible the *causal* effect of various theoretically proposed measures on the likelihood of democratization. We believe that this approach, which stands in contrast to previous studies, has many advantages: first, both traditional and recent political economy theories on the determinants of democracy focus on countries with non-representative institutions. Therefore, it is theoretically required to study autocratic countries for such an empirical investigation. Second, pooling all countries together (i.e. those that are always democratic, always autocratic, and those that transition) contaminates estimates by reverse causation. For example, it would not be clear if income level or human capital causes or is the consequence of democratization. This concern is magnified, since the distribution of political regimes at the beginning of the sample was far from random. For example the richest countries in 1960 were all mature democracies.[3] Third, from a policy perspective it is most important to understand the mechanisms by which representative norms emerge and stabilize in non-democratic societies. We thus limit our analysis to a sample of over a hundred countries that had an autocratic regime in the late sixties and early seventies. However, we expand the analysis by studying in a systematic way all theories on the determinants of democratization and we do not restrict our attention to a specific hypothesis.

This Chapter is structured as follows: In section 4.2 we discuss the main theories on democracy prerequisites and outline the reduced form hypotheses. In section 4.3 we describe the data and our methodology for identifying the timing of democratic transitions. We then provide some preliminary evidence. In section 4.4 we present our econometric specification

[2]For an eloquent reasoning on the probabilistic nature of *all* democratization theories, see the classical analysis of Seymour Martin Lipset (1994) and Samuel Huntington (1993).

[3]A similar criticism of sample-selection is made by Boix and Stokes (2003). They resolve it using data covering the entire nineteenth and twentieth centuries.

and give our benchmark regression evidence. In Section 4.5 we focus on democratic transitions, identifying the factors that influence the timing of democracy's emergence. In Section 4.6 we explore the "*quasi-natural experiment*" of the fall of communism and test our main hypotheses on the sample of new independent states and other former centrally planned countries. In Section 4.7 we perform several robustness checks. These include alternative methods of identifying and timing permanent democratic transitions, additional controls for the social and economic environment and various econometric techniques. In section 4.8 we discuss in detail why and to what extent our results differ from recent and parallel studies. In Section 4.9 we summarize this Chapter's main results.

4.2 Theory: Background and Main Hypotheses

Here, we outline the four main testable hypotheses on the driving forces of democratization. 1) The "*modernization*" hypothesis, recently advocated by Seymour Martin Lipset (1959, 1994) reasons that richer and more industrialized societies are more likely to democratize. 2) "*Social structure*" theories stress the role of culture, religion and the ethnolinguistic composition of society in shaping the political equilibrium. 3) The "*liberal hypothesis*" asserts that economic and political freedom are mutually re-enforcing. 4) "*Endowment*" theories (Acemoglu *et al.*, 2004, 2005) advocate the importance of geography, history and early institutions in driving *both* economic and political development.

4.2.1 "Modernization" Theory

The "modernization" hypothesis asserts that a high income is the single most important prerequisite for both the transition and consolidation of democratic institutions. Przeworski *et al.* (2000) label the first effect as the "*endogenous*" and the later as the "*exogenous theory*". Wealth influences the likelihood of democratization through many channels. First, industrialized and bourgeoisie societies are typically complex and difficult to govern under a centrally planned administrative system. The ruling class has thus many economic benefits to realize under a liberal political organization and thus will not oppose democratic

reforms. The ruling elite can afford some wealth redistribution, which usually takes place after democratization. Second, rich countries can more easily cope with the costly transition from an authoritarian to a democratic regime.[4] Third, a high level of education may lead the ruling elite to initiate, not oppose, democratization. Lipset (1959) referred to opinion polls from numerous countries on citizen's attitude towards democratic institutions that unambiguously concluded "*...the single most important factor differentiating those giving pro-democracy responses to others has been education.*"

- *Reduced Form Hypothesis* [H_1]: Other things being equal, democratization is more likely to occur and stabilize in relatively rich and educated societies.

We find considerable support for the "modernization" theory. When we study the likelihood of being a democracy (sum of the probability to transit and the probability to consolidate democratic institutions), given the initial non-democratic condition, the empirical evidence is clear-cut: higher income and human capital levels lead to democratization.

4.2.2 Social Structure Theories: Religion & Fractionalization

Religion

Building on the Weberian idea of economic sociology, many influential scholars (Huntington, 1993; Landes, 1998) have recently argued that cultural and religious norms are the key driving forces for both political and economic development. This approach suggests that democratization requires the simultaneous occurrence of economic and social factors. According to Weber (1930) and Huntington (1993), there are two main channels through which religion influences political and economic development.[5]

[4]Lipset (1959) writes "*...there is enough wealth so that it actually does not make a difference if some redistribution takes place..*"

[5]Weber (1930) argued that the Protestant ethos spurred individualism, innovation, and modernization, while other religions, (Islam, Catholicism, and Confucianism) focus on more traditional values, such as family, local community and place of worship. The latter favor a more authoritarian, centrally-oriented political organization.

First, close links between clergy and state impede both the emergence and the consolidation of democratic norms. This argument fits nicely into a political economy framework: when the clergy enjoys substantial benefits from the State, it is part of the "ruling class". It therefore has a clear incentive to oppose democratization. Huntington argues that traditionally strong ties between religion and State were present in Catholic, Eastern-Orthodox, Muslim and Confucian countries, while in Protestant countries a clear separation of powers between the church and State was constitutionally secured from the beginning. Although the Catholic Church decided to abandon its traditional prejudices and "modernize", the Muslim clergy decided to re-emphasize its focus on tradition and dogma. Huntington, for example, attributes the democratization in Latin America in the early eighties to a spiritual change of the Catholic Church.

Second, democratization is blocked by religions that have a strong hierarchical structure. For example, some dogmas and ideologies are characterized by an unchallenged pyramidal hierarchy. This stands conceptually far from democratic foundations of equality and individuality and can have a contagion effect on political beliefs and attitudes.

A third channel has been recently put forward by Wright (1992), who offers an alternative explanation to Huntington's "*clash of civilization*" argument explaining why democracy has not been spread to the Muslim world. He presents compelling evidence that Western powers did not support pro-democratic Muslim political parties and movements due to prejudice and misunderstanding of their cultural background. The consolidation of democracy in many Islamic regions was impeded, therefore, not by the nature or ideology of the religion per se, but by non-supportive Western politics.

- *Reduced Form Hypothesis* $[H_2^R]$: Other things being equal, democratization is less likely to occur and stabilize in countries where close links between clergy and state exist and in societies with strong hierarchical structure. Such ties appear to predominate in Muslim and Confucian societies.

Our empirical evidence agrees with previous studies, which showed a negative association

between the democracy level and the proportion of Muslims and Confucians in the total population. However, this negative association weakens significantly (and even becomes insignificant), when we control for natural resource (oil in particular) abundance.

Fragmentation

A society's ethnic and religious composition can also influence the dynamics of democratization. The expected effect, however, is not straightforward. Aristotle, for example, argued that democracy is the optimal political regime for a society with many ethnic groups and social classes, since it is the polity that can best safeguard their liberties.[6] At the same time, ethnolinguistic and religious diversity can damage the economy and block democratization if it is associated with a polarization of political life (Dahl, 2000). Easterly and Levine (1997) show that ethnolinguistic fragmentation is the major cause behind the devastating economic performance of most (non-democratic) African countries. Ethnic and religious tensions have caused never-ending civil wars, enhanced corruption and destroyed any progress towards democratization.

Aghion, Alesina and Trebbi (2004) formally model the trade-off between delegation of power and policy insulation (ex post control of politicians). Their model predicts too an ambiguous effect of polarization on insulation.

- *Reduced Form Hypothesis* $[H_2^F]$: Other things being equal, ethnolinquistic and religious diversity do not have an unambiguous effect on democratization.

Our empirical results suggest that fractionalization is negatively associated with the likelihood of democratization and thus the negative channel tends to dominate the positive. We also find that religious fragmentation is much more harmful for democratic stabilization than ethnic fragmentation.

[6]Huntington (1984) makes a similar point: "*...pluralism enhances the probability of developing stable democracy in modern society...A second set of often discussed preconditions of democracy involves the extent to which there is a widely differentiated and articulated social class, regional groups, occupational groups, and ethnic and religious groups.*"

4.2.3 The Liberal Hypothesis

Friedman (1962) argued that "*economic freedom is also an indispensable means toward the achievement of political freedom*". Other liberal economists (Hayek, 1960) have similarly viewed economic and political freedom as two mutually self-enforcing powers. Societies that are open to trade are inevitably influenced by foreign ideas and beliefs. Their spirit of representative government and political freedom will naturally be strengthened. David Landes summarizes: "*...if the gains from trade in commodities are substantial, they are small compared to trade in ideas*".

Since no data are available for the degree of economic liberties dating back to the sixties (e.g. level of competition, state intervention in economic affairs, etc.), we use trade openness as a proxy for laissez-faire. Trade openness can also proxy for the structure of economic activity and the middle class' role.[7]

- *Reduced Form Hypothesis* [H_3]: Other things being equal, democracy is more likely to occur and consolidate in countries that are open to international trade.

Our empirical evidence provides strong support for the liberal hypothesis. Not only are democratic institutions more likely to consolidate in countries open to international trade, but economic openness exerts a positive impact on the likelihood of democratic transition ("*endogenous*" democratization).

4.2.4 "Endowment" theories

Endowment theories emphasize the role of geography, the disease environment and "early institutions" in explaining long-run economic development. In a highly influential work Acemoglu, Johnson, and Robinson (2001) argue and present compelling evidence that the

[7]There are numerous historical examples of a strong trade openness-democracy link: for example Athens and Rome in Ancient times, or Great Britain in the late eighteenth century. De Long and Shleifer (1993) provide direct evidence that trade expansion and the consequent emergence of the bourgeoisie have crucially influenced the first wave of democratization in Western Europe.

disease environment crucially influenced the type of colonization, which in turn shaped "early institutions" established by European colonizers. Furthermore, due to their persistence, institutions created by European colonizers endured after independence. In parallel work Acemoglu, Johnson, Robinson and Yared (2004, 2005) apply endowment theories in the context of political development.[8] Their argument is that differences in geography shaped initial institutions and these "early" institutions have resulted in long-lasting repercussions for *both* economic and political development.

- *Reduced Form Hypothesis* [H_4]: Other things being equal, the same factors that influence economic development (type of colonization, history, geography) influence the likelihood of democratization and the probability of the consolidation of democracy.

We find supportive evidence to the insight of Acemoglu *et al.* in our (different) sample and verify their main results on the strong-impact of history and endowments on political development. Yet, we can not rule out the positive impact of income and human capital on democratization.

4.3 Democratization Events, Data and Preliminary Evidence

Table 4.1 gives the sample countries. We distinguish between two groups of countries: those that both experienced a democratic transition and succeeded in consolidating democratic rule ("*Democratization Countries*") and those that remained autocratic throughout the sample period, which serve as our control group ("*Always Authoritarian*"). Since the stability of a political system, representative or not, is as vital as its liberalism, we focus on incidents of political transition that succeeded in consolidating representative institutions. This approach is in contrast to all previous studies either on levels or changes, which not only pool together all countries (democratic, autocratic and democratization countries), but also

[8] Levine, Demirguc-Kunt and Beck (2003) make an analogous application of endowment theories of economic development to financial development.

consider short-lived, unsuccessful democratic transitions. Yet, all theories of political organization implicitly focus on the determinants and economic aftermath of stabilized regimes. A complete description of our methodology in dating successful dmeocartic transition is provided in the *Democartization Data-Set Appendix A*. Panel *A* of Table 4.1 lists the countries we consider for our benchmark estimates, while Panel *B* lists the former communist states that we treat separately in Section 3.6. In Panel *C*, for completeness, we report the continuously democratic countries that we exclude from the benchmark estimates.

4.3.1 Data and Descriptive Analysis

The modernization hypothesis asserts that democratization is primarily driven by wealth. In Table 4.2 panel *A*, countries are classified into five categories depending on their income level in 1975 (when the first democratization event occurred in our sample). There is a clear positive association between the *initial* income level and *future* democratization. Almost all of the relatively richest countries in 1975 have managed to consolidate democratic institutions. Eight of the ten countries with GDP per capita greater than 6,000 US dollars have managed to reach an almost perfect democracy score by 2000. In contrast, only four of the twenty-one countries with per capita GDP less than a thousand USD managed to democratize and reach an almost perfect level of representative institutions.[9]

Human capital appears to be also a crucial factor influencing the prospects of democratization. In Panel *B* we similarly classify countries into five categories depending on average years of schooling in the period 1970 – 1975. Schooling statistics are retrieved from Barro and Lee (2001) and, although have less coverage than GDP, they enable us to investigate some countries where income data is non existent (like Afghanistan and Myanmar). Panel *B* is a mirror picture of the GDP classification. For example, in the top schooling category

[9]The "modernization hypothesis" is further strengthened by the fact that in 1975 the 18 richest countries (those with a GDP p.c. greater than 12,000 US dollars) were already run by democratic governments (although this correlation does not necessarily imply causality). These countries are (GDP per capita in 1975 in parenthesis): Italy (12,523); Japan (13,164); United Kingdom (13,165); Austria (13,283); Norway (13,914); Finland (13,925); France (13,988); Belgium (14,055); Iceland (14,253); Netherlands (14,783); New Zealand (15,185); Australia (15,638); Luxembourg (16,263); Sweden (16,454); Denmark (16,606); Canada (16,636); United States (18,041.28); Switzerland (20,403).

only Singapore has remained autocratic, while ten out of twelve countries in this category (approximately 83%) have moved to an (almost) perfect protection level of political rights (*Full Democratization*). This result is in clear line with Glaeser *et al.* (2004), who argue that human capital has a strong causal effect not only on democracy, but to political institutions quality more general.

The "modernization", "social structure", "liberal" and "endowment" theories emphasize different key driving forces for democratization. In Table 4.3 we quantify these differences at the beginning of the 3rd wave (all values correspond to 1975). Means and standard errors for the group of countries that have remained always autocratic (column (1)) and the group of countries that democratized (column (2)) are reported in parenthesis. Furthermore, we split the democratization group into countries that implemented significant reforms ("*Full Democratization*" - column (3)) and countries that democratized before the nineties ("*Early Democratization*" - column (4)).

[H_1]: Not only did countries that eventually democratize have significantly higher income and educational levels in 1975, but differences are amplified when we compare "*Always Authoritarian*" with "*Full Democratization*" or "*Early Democratization*" countries. This hints at the fact that income level and human capital not only affect the likelihood of democratization, but also how *fast* and *deep* the reforms will be.

[H_2]: Pronounced differences are also present in religion and fragmentation. Data on religion shares and on fractionalization are retrieved from Alesina, Devleeschauwer, Easterly, Kurlat, and Wacziarg (2003). Countries with high religious fragmentation suffer from lower quality of democratic norms. The probability that two randomly selected individuals do *not* belong to the same ethnicity was 55.6 percent for the "*Always Authoritarian*" group, while only 45.7% for countries with a democratization event. The differences are even greater when we compare countries that either democratized before the nineties or succeeded in "fully" liberalizing the polity. Panel C of Table 4.2 provides a visual representation. For example only eleven out of twenty-six (42%) countries where religious heterogeneity exceeds 0.65 abandoned permanently autocratic government. Moreover, of those eleven countries,

only five have reached an almost perfect level in civil liberties and political rights protection.

[H_3]: Turning to the liberal hypothesis the descriptive evidence reveals mixed conclusions. As suggested by the $0-1$ trade openness index, which measures the liberalism of trade policy (taken from Welch and Wacziarg (2003), who extend and upgrade the original Sachs and Warner (1995) index), countries that democratized had a more liberal trade regime. However, the trade share (sum of exports plus imports relative to GDP), if anything was higher in the "*Always Autocratic*" relatively to the "*Democratization*" group.

[H_4]: The preliminary analysis also favors "endowment" theories. Years since independence (normalized to $0-1$) is 0.411 for the group of democratization, while only 0.157 for "*Always Autocratic*" countries. So older, more mature countries appear to have a higher likelihood to democratize. Yet, the mean differences on settler mortality are not very supportive to Acemoglu *et al.* (2004) idea. Colonizers faced, on average, higher (not lower) mortality rates in those countries that eventually democratized compared to those that did not.

4.4 Model Specification and Benchmark Results

4.4.1 Empirical Specification

Overall, although we used initial values the descriptives do not establish causality. To estimate conditional and causal effects we rely on a binary response panel model of the form, considered in Chapter 1 in the first-stage treatmnet model. We rewrite equation (4.9) for convenience as:

$$Democ_{i,t} = \begin{cases} 1 & if \quad G(a + x_{i,t}\beta_1 + z_i\beta_2) + \varepsilon_{i,t} > 0 \\ 0 & otherwise \end{cases} \tag{4.1}$$

where x and z are vectors of time-varying and time-invariant covariates respectively that predict democratization. The response democratization variable equals one in countries (i) that democratized in the year (t) and all subsequent years following a permanent democratic

transition. In the next Section, we repeat our estimates when the response variable equals one only in the transition year. This enables us to isolate the effect of x's and z's on the likelihood of democratic transitions. For our benchmark estimates we assume that $G(.)$ is the normal c.d.f.; estimation of the pooled probit model is performed with maximum likelihood. Since probit estimates crucially rely on error normality, we will also present logit estimates, which assume logistically distributed residuals. To show that our results are not driven by the exact methodology, we will present estimates with various other techniques, like ordered probit and logit, a linear probability model and a nonlinear instrumental variables methodology.

Given the non-linear structure of the probit model, coefficients cannot be directly interpreted. Thus in all tables we report marginal effects, estimated as $g(.)\beta$, where g is the standard normal density evaluated at the mean of the independent variables $\bar{x}_{i,t}$ and $\bar{z}_i$. When using binary response models with panel data it is important to compute variance matrix estimators that are robust to within-group correlation. We thus report below the marginal effects p-values for the null hypothesis that $\widehat{\beta}_k = 0$ based on standard errors adjusted for heteroskedasticity and autocorrelation clustering at the country level, allowing observations to be dependent within clusters.[10]

To assess the goodness of fit of the empirical models in every table we report two measures. First, the observed and the predicted probability of a permanent democratization at the mean of the regressors. Second, McFadden pseudo-R^2, which is calculated as $1-\mathcal{L}_{ur}/\mathcal{L}_o$, where $\mathcal{L}_{ur}$ is the log-likelihood function for the estimated model and $\mathcal{L}_o$ is the log-likelihood function in the model with only an intercept.

[10]For more details on the need to allow for clustering see Wooldridge (2002). We also repeated all estimates without correcting for heteroskedasticity at all and without correcting for clustering. In all estimated models standard errors were significantly lower, yielding therefore more significant estimates. To control for positive residual autocorrelation we also estimated models with five and seven lags of the independent variables and then tested the joint significance of the estimated coefficients. The economic significance of the results presented is virtually unchanged.

4.4.2 Results

Income and Education

Table 4.4 presents our benchmark probit estimates testing the "modernization" theory. In column (1) we estimate a simple pooled probit model using all possible countries and all available data. The democratization (dependent) variable equals one in the year of the democratic transition and all subsequent years in countries that permanently abandoned autocratic government. It equals zero during the autocratic years of the "*Democratization countries*" and in all years of the "*Always Authoritarian*" nations. In column (2) $Democ_{i,t}$ equals one in the transition and all subsequent years for countries that experienced a "*Full Democratization*". In column (3) we also include in the group of democratization countries the five "*borderline*" countries. In columns (4)-(10) and throughout the main analysis we exclude from the estimation new independent states that emerged from Czechoslovakia, U.S.S.R. and Yugoslavia for two reasons: First, insufficient data is available for these countries before independence; second, this independence wave offers a useful "*quasi-natural experiment*" to test theories of democratization. We investigate in detail in Section 4.7.1 the reasons why some countries that emerged from the fall of communism in Eastern Europe and Central Asia became democratic, while others are stilled ruled despotically.

In all specifications, income exerts a significantly positive effect on the likelihood of being a democracy. Columns (1)-(4), for example, report the estimated marginal effect when only income level is included as a predictor of the democracy probability. In columns (5) and (6), we use secondary schooling and literacy rates as human capital proxies. The Lipset hypothesis as well as the recent evidence given by Glaeser *et al.* (2004) is validated, since education significantly increases the likelihood of democratization.[11]

A drawback of the previous estimates is that GDP and schooling are highly persistent and exhibit non-stationary behavior. According to the results shown in the first four columns,

[11] In the previous version of the paper, we employed health variables as alternative human capital proxies. The results are quantitatively alike and available upon request. The correlation between schooling and the health variables (fertility, life expectancy at birth, and infant mortality), is above |0.70|. We present some estimates with fertility rates in Table 2.10.

if GDP continues to increase, then the probability of democratization will approach one (although due to the diminishing marginal effects imbedded in the non-linear estimation, it will never reach it). Although a strong proponent of the "modernization" theory could reasonably argue that this might indeed be the case, we need to control for potential "spurious regression". Thus in columns (6)-(8), (as well as in many of the subsequent specifications) we add a linear time trend (or alternatively we add time fixed effects). The estimated marginal effect on income and human capital decays; yet the coefficients retain both their sign and statistical significance.

The industrial revolution and the sub-sequent urbanization have been regarded as major driving forces behind European democratization in the eighteenth and nineteenth centuries. To assess its impact on the 3rd Wave, we include in the set of explanatory variables the urbanization rate. The estimated marginal effect (in column (7)), although positive, is small in magnitude and statistically insignificant, suggesting that urbanization has not played such a key role. In recent years, the historically strong urbanization-democracy nexus has softened, since many non-developed non-democratic countries in Africa and the Middle East have experienced a huge internal wave of city migration.[12] In column (8) we add in the set of explanatory variables the natural logarithm of radios per 1,000 habitants to proxy for the level of industrialization (as suggested by Lipset, 1959).[13] In spite of a high correlation with radios per capita ($\rho = 0.60$), the marginal effect of GDP has retained its significance.

In column (9) we use an alternative, non-explosive income indicator, the ratio of domestic GDP per capita relative to the United States (taken from the Penn World Tables). The positive and highly significant marginal effect further strengthens H_1 by showing that democratization is more likely to emerge and stabilize in *relatively* richer societies.

The estimates raise a reverse causality concern. For example "*development*" theories of

[12]For example, the non-democratic Arabic Kingdoms of the Persian Gulf have urbanization rates above 90%; Bahrain, for example, which gets a minimum score for political rights protection (according to both the Polity and the FH measures), had an urbanization rate of 92.3% in 2000. Likewise urbanization rates are very high in Saharan African countries. Libya, for example, had an urbanization rate of 87.8%.

[13]Estimating models with only urbanization and radios plus a time-trend enables us to study more countries. The marginal effects resulting from these estimations are higher than the ones reported here, strengthening H_1.

democracy (see Section 4.2) argue that the presence of democratic institutions leads to a higher level of economic development [see Figure 4.1]. This problem, however, is minimized by our methodological approach, since our sample consists of countries that were initially (in the sixties and early seventies) non-democratic and excludes old mature democracies (e.g. the U.S., Canada, and most Western countries). Since, however, reverse causality might still blight our results, we estimated all specifications with the initial values of the main independent variables. In column (10) we use the 1975 levels of GDP, schooling, urbanization and industrialization.[14] H_1 still holds: richer societies (in 1975) managed to abandon autocratic ruling and establish representative institutions, while less educated either failed or have only "partially" restored democratic governance.

Social Structure: Religion, "natural resource curse", and fractionalization

To quantify the effect of religion on democratization, we add in the first column of Table 4.5 the share of Muslims and Confucian-Buddhists in the total population. As previous studies have shown, democratization is less likely to emerge and stabilize in Muslim countries. The estimated marginal effect is, however, not very large implying that a 20 percentage point increase in the share of the population following Islam in a non-democratically ruled nation (approximately from Cameroon where the share is 22% to the Ivory Coast which has a share of 39%) is associated with a 3% decline in the probability that this country will permanently switch to a democratic regime.[15]

Many argue, however, that the real impediment to democratization in the Middle East or Africa is not religion, but the oil-rich soil.[16] Natural resources enable the ruling class to finance an expensive policy-state and buy necessary (foreign and/or domestic) support. Acemoglu, Robinson and Verdier (2003) build a model that formalizes this intuition and

[14] Using the 1960 or the 1970 values yields almost identical estimates. We report estimates based on 1975 values, since the first democratization episode occurred in this year and because our country samples increases.

[15] Although Ivory Cost and Cameroon have similar per capita GDP in 2000, this is just an approximation, since the marginal effects are calculated at the mean of the other covariates. In a previous version of the paper we used dummy variables for whether a country's dominant religion was Islam or Confucianism to find very similar results.

[16] See for example the recent article in the New York Times by Eviatar (2003)"

provides an illustrative description of Mobutu's and Trujillo's "kleptocratic" practices in the Congo and Dominican Republic respectively.[17] In column (2) and in many subsequent Tables, we include in the set of explanatory variables an indicator variable for oil-producing countries.[18] The oil-dummy marginal effect suggests that democratization is less likely to emerge and stabilize in a country with an oil-rich soil by approximately 5%, relative to a similar country that does not suffer from the "*natural resource curse*" (at approximately the mean value of the other controls). Notice that the negative effect of the Muslim share sharply decreases (and in many subsequent specifications becomes insignificant), suggesting that an oil-rich soil rather than religion hinders democratization. Those findings extend previous evidence presented in political science (Ross, 1999, 2001) on the strong correlation between oil dependence, civil wars and totalitarian political organization.

In columns (3) to (6), we investigate the effect of religious and ethnic fragmentation. The probit estimates validate and extend the descriptive evidence on the harmful role of fractionalization. Other things being equal, the probability of a country switching to democracy sharply decreases in the level of religious and ethnic heterogeneity. Interestingly, religious rather than ethnic fragmentation is much more important. The marginal effect of religious fragmentation is always significant at the one percent level and is at least three-to-five times greater than the (not always significant) marginal effect of ethnic heterogeneity. This finding is not only in line with, but also advances the cross-sectional evidence reported by Aghion, Alesina and Trebbi (2004), who show a strong negative correlation between polarization and various measures of policy insulation (like constraints on the executive, autocracy, etc.). Our analysis suggests that religious rather than ethnic (or linguistic) fractionalization is the major impediment to democratization.

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[17] In their model the ruler can use natural resources to divide the masses and block revolution. To orchestrate a revolution the various social groups need to co-operate and resolve the collective action problem. The dictator can bribe one of the groups (or a popular individual) and keep his office. Huntington (1993) argues similarly: "*...oil revenues strengthen the autocratic-oriented bureaucracy and consequently hinder democratization forces that aim to overthrow the structure of the previous regime.*"

[18] We also experimented with an OPEC member country dummy and with a mix of the two sources. The results are similar.

Economic Liberalism

We now turn to the liberal hypothesis using two proxies of economic liberalism. First, in columns (7) and (9) we add to the RHS a trade openness dummy, which equals one if country i in year t has a liberal trade regime and zero otherwise. The estimated marginal effect implies that a country open to international trade enjoys a 4% to 4.5% higher probability of being a democracy relative to a closed-economy. Hence, our findings imply that economic freedom has a positive impact on political freedom. Although at this stage we can not say much about causality, the results however, clearly illustrate that the positive correlation between international trade and democratization extends beyond the historical development of Western Europe in the eighteenth and nineteenth centuries.[19]

Second, in columns (8) and (10) we use imports plus exports to GDP as an alternative proxy of economic openness. In contrast to the previous evidence, we find that nominal trade volume is not positively correlated with democratization. This can be attributed to the high trade to GDP ratios of the East Asian and some oil-exporting (non-democratic) countries. This result appears also in line with the ongoing study of Rigobon and Rodrik (2004), who, using an identification-through-heteroskedasticity method, find a negative correlation between the long-run level of democracy and trade share.

Endowments & History

To investigate the effect of geography on democratization we add in the first column of Table 4.6 latitude from the equator. In columns (2) and (3) we add the share of the population that

[19] Using instrumental variables techniques, Lopez-Cordova and Meissner (2004) claim that there is a causal effect of trade on the long-run level of democracy. They employ Frankel and Romer's (1999) "*natural*" trade openness to instrument trade to GDP, which in turn is entered in a specification with the democracy level as the dependent variable. Using data covering more than 100 years, they show a strong causal effect of trade on democracy. In this direction we also estimated an IV probit model, instrumenting trade openness with geography and legal-origin variables (specifically: distance from the equator, a landlocked dummy, a measure of the share of population living in the tropics, and a British origin dummy). The estimates imply an even larger positive and statistically significant effect of trade openness on democratization: When we include in the set of explanatory variables the (instrumented) trade openness, the logarithm of GDP and a linear trend, the marginal effect of trade openness is 0.641 ($p-value$: 0.00) and the marginal effect of $\ln(GDP)$ 0.0301 ($p-value$: 0.05). In line with these results are IV probit estimates reported in Table 2.15.

speaks a major European language and condition on religion and oil abundance (taken from Dollar and Kraay (2003)).[20] Both variables enter with significant coefficients, suggesting that these factors influence political as well as economic development. Interestingly once we control for geography, institutions and oil-production, the Muslim share is not a significant impediment to democratization.

In columns (4) and (5), we investigate whether the quality of the legal system or the identity of the colonizer influenced the likelihood of democratization. La Porta *et al.* (1998) show that the common-law system offers superior property rights protection, while Lipset (1959) claimed that British colonies were more open and already had some representative institutions, even before independence. The indicator variable for common-law countries, however, enters with an insignificant coefficient. Although this might seem contradictory to Barro (1999) or Przeworski *et al.* (2000), it is not surprising. Most economically and politically successful former British colonies, like the United States, Canada and Australia, were already democracies before the beginning of the 3rd Wave. So, although the British heritage or/and the common law might have influenced the First and Second Wave, it has not been a key factor in the recent democratization episodes.[21]

In columns (6) and (7), we assess the effect of the type of colonization and early institutions proxied by settler mortality on the likelihood of democratization. In line with the descriptive statistics, there seems to be no systematic effect of early institutions (at least when proxied with this measure) on the likelihood of a stable permanent democratization. Although most countries that abandoned autocracy during the 3rd Wave had relatively lower settler mortality rates, the dataset includes some outliers that yield this insignificant effect. For example, settler mortality rates in Mali and Nigeria, countries that experienced a permanent (although late) democratization, were 2940 and 2004 respectively, while in most other democratization countries mortality rates were around 100. When we exclude in column (8)

[20] We note, however, that the European heritage measure is far from being ideal. The literature on the determinants of long-run economic development (Hall and Jones, 1999; Dollar and Kraay, 2003) has used this variable as an "instrument" of social infrastructure (institutional quality, which incorporates democracy), which in turn causes long-run income levels.

[21] Examples of countries with a British legal system that are throughout the sample-period non-democratic include the Persian Gulf countries, Zimbabwe, Liberia, and many other African nations.

Mali and Nigeria, the coefficient on settler mortality enters with the theoretically predicted negative sign and is in most specifications significant. The importance of history and the intuitive argument of Acemoglu *et al.* (2004a) is clearly illustrated in columns (9) and (10), when we augment the specification with the normalized years-since-independence measure. The estimates imply that democratization is more likely to emerge and consolidate in older countries, even when we control for religion, income and the trending behavior of this and the GDP measure.

The results thus offer clear support to *both* endowment/early-institutions theories [H_4] and the "modernization" hypothesis. In spite of the high correlation between latitude, settler mortality, and years-since-independence with wealth, income (and education) retains its significantly positive impact on the likelihood of democratization. H_1 is thus difficult to challenge, even when one controls for variables that are strongly correlated with income and human capital. Therefore, although our evidence validate (in a different sample and with a different methodology) Acemoglu *et al.* (2005) idea, we are not able to fully discard the effect of income or education during the Third Wave.

4.5 Democratic Transitions

4.5.1 Main Theories

In this section we assess haw the four main theories predict the likelihood of transition (the "*endogenous*" theories in the terminology of Przeworski *et al.*, 2000). The dependent variable now takes the value one only in the year of a democratic transition and remains zero during the non-democratic period (and has gaps in all post-transition democratic years).

Table 4.7, columns (1) and (2) report the marginal effect of income and education on the probability that an autocratic country will become a democracy. An increase in income by $1,000$\$ (approximately 6.9 log points) is associated with a 6% ($0.009 * 6.9 \simeq 0.062$) increase in the probability. The marginal effect of income is substantially smaller for explaining democratic transitions relative to the probability of consolidating representative institutions.

Moreover, the estimated coefficient on income or education in some specifications becomes insignificant, implying that the effect of income is primarily safeguarding political rights once they are established. We note, however, that examining the impact of income and education on the transition probability in a sample of just forty or fifty years is far from ideal. For example, if one thinks about the association between income level and democracy over a longer period then it is difficult to dismiss the link. Boix and Stokes (2003) move in this direction using data covering the last two centuries and find strong evidence for both the "*endogenous*" and the "*exogenous*" variants of the modernization theory. Similarly, Acemoglu, *et al.* (2005), find supportive evidence when they examine data going back to the fifteenth century.

Estimates testing the validity of social structure theories are reported in columns (3)-(5). The magnitude of the estimated marginal effects on the Muslim and Confucian share has decayed, as has the effect of oil abundance. Religious fragmentation, however, still exerts a significantly negative impact on the likelihood of a country to successfully transit from autocracy to representative government. In contrast the effect of ethnic heterogeneity is much smaller and in most specifications insignificant.

In columns (6)-(10) we assess the impact of all main theoretical hypotheses on the likelihood of a successful democratic transition. Three main findings emerge: First, history, proxied by years-since-independence is the most robust and statistically significant factor positively associated with the likelihood of democratic transition. This holds even when we control for the trending behavior (column 6) or other covariates. Second, once we control for historical factors and income, the effect of religion in explaining democratic transitions becomes insignificant. Third, the effect of income and education decay drastically and in some specifications becomes insignificant.

4.5.2 Additional Factors Influencing the Transition

Trade Liberalization

In the previous section, we found compelling evidence that democratization is more likely to emerge and stabilize in countries open to international trade. The direction of causality between trade liberalization and political freedom, however, remains an open question. In columns (1)-(3) of Table 4.8 we assess the impact of trade openness on the likelihood of transition. In line with the previous results, we find that while trade openness is positively and significantly associated with the likelihood of transition (column (1)), trade share (measured as the sum of exports and imports to GDP) is not (column (2)). Although these estimates by construction address issues of endogeneity and reverse causality, it is interesting to investigate the association between trade and political liberalization. In column (3) we add a dummy variable that equals one if a trade liberalization has occurred at the current year or during the three previous years and zero otherwise. The estimated marginal effect is significantly positive implying that if economic reforms have been recently implemented then the likelihood of a successful democratic transition increases by 5.5%. This finding is in line with the parallel study by Giavazzi and Tabellini (2004), who document a strong interaction between political and economic reforms. Giavazzi and Tabellini argue that causality is more likely to run from political to economic liberalizations, rather than vice versa, although they cannot rule out feedback effects in both directions. Our results thus strengthen the reverse effect.[22]

[22] Countries that experienced trade a liberalization in the same year or during the three pre-democratization years are (first value indicates trade liberalization year, second number indicates the timing of the successful democratic transition): Albania (1992, 1992); Benin (1990, 1991); Cape Verde (1991, 1991); Czech Republic (1991, 1993); Hungary (1990, 1990); Nepal (1991, 1991); Poland (1990, 1990); South Africa (1991, 1994); Tanzania (1995, 1995); Slovenia (1991, 1992); Slovak Republic (1991, 1993). Countries that democratized and were already open to international trade are (first date indicates trade liberalization, second date indicates democratization): Chile (1976, 1990); Spain (open at the beginning of the sample, 1978); Ghana (1985, 1996); Greece (open at the beginning of the sample, 1975); Guatemala (1988, 1996); Guyana (1988, 1992); Croatia (1993, 2000); Indonesia (1970, 1999); Jordan (1965, 1993); Korea, Rep. (1968, 1988); Mexico (1986, 1997); Mali (1988, 1992); Portugal (open at the beginning of the sample, 1976); Paraguay (1989, 1993); El Salvador (1989, 1994); Thailand (open at the beginning of the sample, 1992).

Crisis

A crisis can initiate actions to change the government. In columns (4)-(7) of Table 4.8 we investigate the impact of GDP growth and crisis on the likelihood of a successful democratic transition. As indicated by the significantly negative marginal effect on GDP growth in column (4), recessions increase the likelihood of democratization. In column (5) we use instead of growth a dummy variable that takes on the value one if a currency crisis has occurred in the current year or during the past three. The crisis chronology is retrieved from Kraay (2003) and includes both incidents of large devaluations and unsuccessful currency attacks (associated with large foreign reserve depletion and/or an interest rate spike). The estimated marginal effect suggests that a currency crisis increases the likelihood of transition from an authoritarian to a democratic polity by 2%.[23] In column (6), we further investigate the impact of crises by augmenting the model with an indicator variable which equals one if a systematic banking crisis (as defined by Caprio and Klingebiel (2003)) is in place. The estimates are similar, since a banking crisis is associated with a significantly higher likelihood (approximately 3%) of a democratic transition.[24]

Not only do these results validate the anecdotal perception and the case-specific evidence (e.g. Haggard and Kaufman, 1995) linking a crisis with the timing of a democratic transition, but they also provide an explanation as to why the "*endogenous modernization theory*" might not find short-term empirical support. Although income growth can make a society more ready to adopt representative institutions, through the channels advocated by H_1, prolonged and sustained growth can also give legitimacy to a non-elected government. The typical example in our sample is the East Asian countries, in which fast growth delayed the democratization process. Acemoglu and Robinson (2001) formally model how the stability of

[23] Countries that democratized after a currency crisis are (first date indicates crisis year, second date indicates the timing of the permanent democratic transition): Argentina (1981, 1983); Bolivia (1981, 1982); Dominican Republic (1976, 1978); Mexico (1994, 1997); Peru (1977, 1980); Spain (1977, 1978); Indonesia (1997, 1999); Philippines (1986, 1987); Portugal (1975, 1976); Thailand (1990 & 1992, 1992).

[24] Countries that democratized in a year when a systematic banking crisis was in place are: Albania (1992); Bangladesh (1991); Spain (1978); Indonesia (1999); Mexico (1997); Mozambique (1994); Nicaragua (1990); Philippines (1987); Turkey (1983); Tanzania (1995). In addition, many former Soviet Republics and two of the five "borderline" democratization countries, Niger (1999) and the Central African Republic (1993), moved towards representative rule, when a banking crisis was in place.

authoritarian regimes crucially depends on their performance. Since non-elected governments do not have formal legitimacy, a solid economic performance is needed to substitute for this gap. They also show that it might not be wise for a society to stop the growth path, due to the sizable transition costs associated with changing political institutions. It can, however, be optimal to democratize during a recession, when society is already incurring part of the transition costs.

War Ending

Historically an international war has also been destabilizing for autocracies. This is not only because growth drops significantly, but for at least two additional reasons. First, the masses can more easily resolve the collective action problem (Olson, 1965) after an armed conflict: A crucial assumption of political economy models is that the two (or more) classes (the "*rich*" and the "*poor*") do not face coordination problems. Resolving the collective action problem, however, is not straightforward in reality. An admired hero-leader is likely to emerge out of the war, who can inspire the people and lead the revolutionary attempt. A war can also provide the necessary military equipment for the revolting parties. Second, is military dissatisfaction: Huntington (1991) bluntly argues "*... The military are the ultimate support of regimes. If they withdraw their support...the regime falls*". If the war is lost and military officials are dissatisfied, they might withdraw their support from the regime. And as Lake (1992) documents, the probability of losing a war is two times higher for dictatorships than for democracies.

There are numerous instances of democratization occurring after the unsuccessful end of an armed conflict, both in the post-war period and before. In our sample, for example, the long-lived military regime in Argentina collapsed after the defeat by the British in the Falklands. Similarly, a crucial factor behind the collapse of the Portuguese junta was its defeat in the colonial war. To quantify this in columns (8) to (10) we add a dummy variable that takes on the value of one if an armed conflict ended in the current or during the last three years.[25] We find strong empirical evidence that the likelihood of democratization sharply

[25]Data on armed conflict are retrieved from the Armed Conflict Dataset (2003), maintained by the Inter-

increases after the end of a war. Even after controlling for a banking and currency crisis or trade liberalization that may coincide with the end of an armed conflict (like in Argentina), its estimated effect is still highly significant. Other things being equal, the end of an armed conflict in an authoritarian nation raises the likelihood of a permanent democratic transition by 2.0% to 2.5%.

4.6 Democratization in Former Centrally-Planned Republics and Countries

In the early nineties, an "independence wave" gave birth to 22 newly established states from the former Czechoslovakia, U.S.S.R. and Yugoslavia. In almost half of these countries (such as Czech Republic, Ukraine, Latvia, and Russia), independence was associated with political liberalization. Many, however, did not succeed in moving away from totalitarian regimes (such as Armenia, Azerbaijan and Moldova). In addition, the fall of the Iron Curtain had an impact not only in the new independent states, but also in the other countries aligned with the Soviet Union during the Cold War. This "*quasi-natural*" experiment enables us to test the main democratization theories and verify our previous evidence. These countries were similar in some aspects (like institutions, geography, economic structure), but differed in many other dimensions that democratization theories stress (such as religion, fractionalization, human capital and income level). Since the typical data sources (World Bank and the Penn World Tables) do not report reliable data before the transition for these countries, we rely on de Melo, Denizer, Gelb and Tener (2001), who, using primary sources have constructed a dataset of initial conditions on thirty transition economies. In Table 4.9, we estimate cross-country probit models, investigating the validity of the main hypotheses in 20 of 22 newly established states (since no data is available for Serbia and Montenegro and Bosnia), while in Table 4.10 we explore the expanded set of 30 formerly centrally planned economies (which besides the 22 new countries includes Albania, Bulgaria, China, Hungary, Mongolia, Poland, Romania

national Peace Research Institute at the University of Oslo (PRIO). In the previous version of the paper we used the widely employed Correlates of War (COW) database, the State Failure Task Force dataset and the on-line source www.onwar.com. The results do not change in any significant way. For the merits of the PRIO database versus other sources, see Sambanis (2002) and Miguel, Satyanath and Sergenti (2004).

and Vietnam).

In column (1) for example of Table 4.9 we predict democratization using the PPP adjusted per capita GDP in 1989. The results support H_1, since the countries that democratized alongside independence were significantly richer even before independence than these nations that have remained autocratic. Since data on schooling is not available for most of these countries, in column (2) we use fertility rates as an alternative human capital proxy. In line with H_1 and the strong negative correlation between fertility and education levels (and income) the estimated marginal effect is significantly negative. Similar results when we focus on the expanded sample of transition countries (in the first columns of Table 4.10).

When we control for religion (in column (3) of both Tables) the coefficient on income becomes insignificant (although it remains positive), while the share of Muslims in the population appears to be the key factor in explaining the different political path of the former Republics. A similar finding emerges from the estimates reported in Table 4.10. We are, however, sceptical of this interpretation for two reasons: First, the sample size is quite small and the correlation between the Muslim share and income in this group of countries is high. ($\rho = -0.62$) Therefore, the estimates are contaminated by multi-collinearity. Second, the negative coefficient on religion does not necessarily imply that democracy is not suitable for Muslim nations. For example Kazakhstan and Turkmenistan, where the share of Muslim in the population is 47.02% and 86.91% respectively, have oil-centered economies. According to the "*natural resource curse*" argument, oil and not religion hinders democracy. The blocking to political reforms impact of natural resources is clearly illustrated by the estimates reported in columns (5) and (6), when we augment the model with an indicator variable for "*rich*" natural resources countries and for "*moderate*" natural resources nations. According to the estimates in column (6), the probability of democratization in Moldova (a non oil-rich country) relative to Azerbaijan (an oil-abundant country) is higher by approximately 45% [GDP p.c. of both countries is similar and approximately at the mean of all sample countries]. The data is not very supportive to fractionalization theories, since both ethnic and religious fragmentation enter with insignificant coefficients. Yet in line with the previous estimates, religious (rather than ethnic) fractionalization appears to play a somewhat significant effect

in blocking the democratization process (the estimated marginal effect is always negative and in most specifications significant).

The democratization experience of these countries strongly supports the liberal hypothesis. All countries that democratized at the same time also opened their economy to foreign investors. In contrast, all nondemocracies (for which we have data) have closed economies. This strong inter-relation is exactly what Friedman and Hayek so forcefully argued about; yet establishing causality is impossible due to the perfect correlation of market economy and political reforms. To further assess the role of trade openness in columns (8) and (9) we add to the set of independent variables the natural logarithm of the geography-predicted trade proposed by Frankel and Romer (1999). This measure is derived by bilateral trade "gravity" equations and measures how favorable geographical conditions are to international trade. Although it does not measure actual trade, the estimates imply that democratization appeared to be much more likely in countries where opportunities for international trade were greater.

We further augment the model with the normalized years-since-independence variable and with a measure of years under central planning to investigate the role of institutions and history on democratization. Due to institutional persistence we would expect, according to H_4 that countries with more years under communism would have a lower likelihood to switch to representative rule. The result supports this expectation and the Acemoglu *et al.* (2004a) intuition. According to the marginal effect given in column (7) of Table 2.10 the probability of democratization in Moldova (which had an experience of central planning of 51 years in 1989) relative to Azerbaijan (which was under communism for 71 years) is higher by approximately 35% ($0.018 * (70 - 51) = 0.342$). Most importantly GDP, "early institutions" and religious (rather than ethnic) fragmentation is still enter the specification with stable marginal effects. In column (10) we assess the effect of income growth before the transition, augmenting the specification with the mean growth in the period $1985 - 1989$ period. In line with our previous results, growth appears to be negatively associated with the likelihood of democratization.[26]

[26]For example the Central Asian republics and China were growing much faster than the European nations,

4.7 Sensitivity Analysis

In this Section, we perform several sensitivity checks. First we investigate the effect of income inequality on democratization. Second, we address concerns regarding our methodology in identifying and dating democratization events. Third, we employ a trichotomous distinction of political regimes and repeat our estimation using an ordered analysis.

4.7.1 Inequality: Theory and Empirics

A potential omitted variable from the previous estimates is inequality. The inter-relationship between income inequality and democracy is one of the most widely theoretically studied issues in political economy. Since, however, previous empirical studies have not fully addressed the theoretical channels, before we investigate the stability of our estimates on the inclusion of inequality in the RHS, we clarify some important issues on this link.

- **Inequality => Democracy:** Both traditional and new political economy theories suggest that the likelihood and the timing of democratization is causally determined by income inequality. However, the association is not straightforward and many channels have been suggested. First, the traditional political science assertion is that *high* inequality increases the likelihood of democratization by spurring opposition. Second, and in contrast to this positive association, Boix and Garricano (2001) model why democratization is more likely to emerge and consolidate when inequality is *low.*[27] Their argument is that a mean-centered income distribution implies that the redistributive impact of democracy diminishes and the conflict between "*the rich*" and "*the poor*" is mitigated. Consequently the ruling elite does not oppose reforms. A third middle-ground between the two previous arguments is formalized by Acemoglu and Robinson (2001, 2005). In their game-theoretic setup, extreme inequality does not

which experienced severe recessions during the last years of communism. Although one could produce specifications, where growth was insignificant, this results is clearly in line with previous estimates.

[27] Barrington Moore (1966) famously claimed that "*no bourgeoisie, no democracy*". The role of the middle-class in promoting democracy was first noticed by Aristotle and was emphasized by Marxist economists.

necessarily lead to a democratization, since highly unequal income distribution implies not only that "*the poor*" have much to gain, but also that "*the rich*" have much to lose if democracy consolidates; "*the rich*" therefore have a greater incentive to oppose democratization.[28] This theory suggests that the likelihood of democratization is an increasing, but not monotonic function of inequality. These opposing effects led even the great Aristotle to suggest that the effect of inequality on democracy is ambiguous.[29]

- **Democracy => Inequality:** Although theoretical ambiguity on inequality's effect on democratization, "*median-voter*" models make a clear prediction of a negative causal impact of representative institutions on inequality. If taxation and public policies are endogenously selected by voting and the crucial "*median-voter*" is a member of "*the poor*" then she will vote for redistributive policies, which in turn will mitigate inequality.

Figure 4.1 provides a graphical illustration of this theoretical point. Formal political economy models suggest that the likelihood of democratization can be a decreasing or an increasing function (not necessarily monotonic) of inequality. Yet, after the consolidation of democratic rule, inequality is expected to fall (controlling for other channels). Pooling democratic, non-democratic and transition countries therefore obscures which is the cause and which is the consequence and does not enable the identification of the two opposing effects. Note that both effects are theoretically grounded and historically present. Therefore, the mixed and inconclusive evidence of both the economics and the political science studies should come at no surprise.[30]

[28] Their models yield multiple-equilibria. First, if inequality is high, "*the poor*" can force the ruling class (by revolution or consensus) to extend the franchise (early democratizations in the nineteenth century in the United Kingdom and the Western European countries). Second, even if inequality is high, the masses might not be able to resolve the "*collective action*" problem and organize a revolution. The ruling class can use, for example, national resources to buy political support and keep their power (as in many African countries). Third, if inequality is relatively low and the economy performs well, "*the poor*" have no incentive to revolt and democratization is delayed (East Asian countries).

[29] Aristotle writes: "*Where one set of people possesses a great deal and the other nothing, the result is either extreme democracy or unmixed oligarchy or a tyranny due to the excesses of the two.*" A similar argument has been recently made by Dahl (2000).

[30] For example, Barro (1999) has documented a (marginally significant) negative effect of inequality on the long-term level of political freedom. Similarly, Muller (1995) studied the determinants of the long-run

In Table 4.11 we quantify the effect of inequality on the likelihood of democratization. In columns (1)-(7) we examine the impact of inequality on the probability of being a democracy (transition plus consolidation), given that a country was non-democratically ruled at the beginning of the sample; in the last three columns we isolate the effect of inequality on the transition probability. To measure inequality we exploit the recently updated income distribution data constructed by the World Bank and use the (unadjusted) Gini coefficient.[31] We are not able to detect a systematic link between inequality and democratization. The estimated marginal effect is insignificant, not stable, and even changes sign. This applies both when we study the democratic transition and when we study the transition plus the consolidation.

In columns (6), (7) and (10) we include in the democratization group only those countries that experienced a *full* consolidation of representative institutions. All *partial* democratization nations are now assumed to have never experienced a transition and are treated similarly to the "*Always Autocratic*" group. When we apply a *strict* definition of stable democratization, we find some (weak) evidence supporting the traditional political science assertion. The estimated marginal effect is now positive, implying that a higher income inequality (high Gini) increases the likelihood of both the transition and the consolidation of democracy. The evidence also hints at the fact that democratization is blocked at extreme inequality levels (as in Acemoglu and Robinson (2001)). However, this non-linear effect, is difficult to identify. Thus, although we can produce specifications (like in column (6)) where both terms appear with the theoretically predicted sign, we note that this result is not particularly robust.[32]

(1965 – 1990) cross-country change in democracy and concluded that "*...income inequality has a robust negative impact on democratization*". Even Bollen and Jackman (1995), who question the significance of Muller's estimates, report negative, yet insignificant, inequality coefficients.

Another feature that magnifies this identification problem is data quality. More inequality data are available from developed and always democratic nations (like the US, Canada, Western Europe, etc.). Therefore the regression estimates using all observations are driven by those countries that were consistently democratic through the post-war era. So if inequality was falling in developed countries (due to the democratic redistribution mechanism, for example) then the negative effect estimated in the previous studies is not surprising.

[31] We also estimated models with the adjusted Gini coefficient (adjusting made on the basis of the source). The results are qualitatively alike. Since the Gini data have many gaps we follow Barro (1999) and interpolate using the last available value. We also estimated models with a five or even a ten year moving average, without any major differences.

[32] A major impediment in testing political economy theories on democracy and inequality is the low quality data on income distribution. In the previous version of the paper (Chapter), using a sub-set of the inequality

Most importantly for our analysis the main evidence regarding the four main hypotheses are quite robust.

4.7.2 Alternative Democratization Chronology

One could argue that self-selection of the democratization events drives our results. Although we consider the exact identification and timing of permanent democratization a key ingredient of our research, we re-estimated our model relying solely on the two most widely used democracy indicators (Polity and FH). We identify a permanent regime change when the Polity index jumps from a negative to a positive value and when the FH characterization jumps from "*Not Free*" to either "*Free*" or "*Partially Free*". To avoid capturing political instability, we require that both indices remain at the new value for at least five years.[33] This re-estimation changes the exact timing of transition and increases the democratization sub-sample.

In Table 4.12, columns (1) to (5) we test the main theories in explaining the likelihood of being a democracy (transition plus consolidation). Most of the estimates have retained both their statistical and, most importantly, their economic significance. Democratization is more likely to occur in wealthy, but not oil-rich, countries. Columns (6) to (10) report estimates only for transiting probabilities to democracy. The "*endogenous*" modernization theory is challenged, since income does not significantly affect the transition probabilities in either sample. In contrast, the previous finding on the destabilizing effect of crisis to autocracies is verified. A recession (proxied by a drop in GDP growth) or a currency crisis has a significant and robust effect in explaining the timing of transition. Controlling for religion, fragmentation and income, the estimated marginal effect suggest that a currency crisis increases the likelihood to switch into democracy by almost 3%.

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data, we found some considerable support for both the Acemoglu and Robinson and the classical political science assertion that greater inequality increases the likelihood of democratization. Yet, utilizing new and richer data attenuated this effect.

Another feature of the income distribution data that makes the quantification of the Acemoglu and Robinson (2001) model non-linear prediction so difficult to test is the fact that we miss inequality data for many non-democratic governments, where anecdotal evidence suggest that inequality is very high (e.g. many Sub-Saharan African countries).

[33]The results are robust to changes in this requirement.

4.7.3 Ordered Analysis

The level of political and civil liberties in a given country can be measured in many ways. One could still be uncomfortable with either our event identification or with the Polity or FH indicators. Although our methodology on identifying permanent democratic transitions addresses issues arising from measurement error, one could argue that the binary distinction is inappropriate. For example switching from a totalitarian regime to a perfect level of democratic institution, as in Spain or in Greece, is not comparable to Nigeria's (1999) path. Furthermore one might not be comfortable with the stability requirement we have imposed in our analysis so far. To address these issues, we follow the Freedom House distinction and estimate an ordered probit regression distinguishing between three types of democracy: Non-politically free societies, partially free and totally free. We do the same using the Polity IV index (which ranges from -10 to $+10$) by splitting it evenly into three parts. We also relax the estimation assumption of normally distributed errors and also report logit estimates.

Ordered probit and logit estimates are reported in Table 4.13. The results not only do not challenge, but actually strengthen the "modernization theory".[34] The natural logarithm of GDP is the single most important driving force behind political modernization. Democratization is less likely to occur in oil exporting nations. The marginal effects of the two religion and the two fractionalization measures have become in many specifications insignificant, while the effect of oil is still negative and highly significant. Finally in line with H_3 and H_4, democratization is likely to occur and stabilize older, more mature countries that are open to international trade.

4.8 Previous Work and our Results: Similarities & Differences

Before we conclude, it is important to explain *why* and *to what extent* our results differ from previous and contemporaneous studies. In doing so, we also investigate the robustness of

[34] This is line with the parallel work of Epstein *et al.* (2004), who show that in a trichotomous classification the positive effect of income and human capital on democracy is strengthened.

our results on alternative econometric techniques.

4.8.1 Dynamic Analysis

Recent studies in political science have examined the driving forces of democracy employing Amemiya (1985) dynamic probit model. Przeworski *et al.* (2000), Garricano and Boix (2001) and Boix and Stokes (2003) model democratization as follows:

$$Democ_{i,t} = \Phi\left\{a + \gamma' x_{i,t} + \delta'(Democ_{i,t-1} * x_{i,t})\right\} \tag{4.2}$$

The dependent variable equals one if political liberties and civil rights protection is above a threshold and zero otherwise. The set of independent variables includes both time-varying and country-invariant variables. Estimation is performed in all sample countries ("*Always Autocratic*", "*Always Democratic*" and "*Democratization*") in the post war data. The merit of this Markov-switching specification is that it enables the identification of x's impact on both the transition probability (which is captured by the δ vector) and the probability of consolidation (captured by the γ vector). Our results can be interpreted in this framework as well, since in the first part of our empirical analysis we estimated the sum of γ and δ, while in Section 4.5 we estimated the transition probabilities (δ).

The main limitations in this approach are the non-random selection of countries in the beginning of the sample (the richest countries were all democracies) and reverse causation. For example, clearly part of income's or education's large effect on the consolidation of democracy is driven by the group of countries that were democracies throughout the post-Second World War era. In addition, this approach does not distinguish between permanent versus temporary changes and consequently the estimates on the transition probabilities (especially) can be very sensitive to noise and unsuccessful transitions.[35]

[35] Assume, for example, that in a very poor nation the democratization dummy jumps many times. Assume furthermore that the "modernization theory" holds and that all spikes are just temporary. In contrast in a relatively rich nation (e.g. Spain) the democracy variable jumps only once, but this is permanent (because H_1 holds). The first point is that those jumps are more likely to capture instability (noise) rather than a serious attempt to democratize. The second is that the coefficients will not capture the theoretically predicted effects. Transitions are going to be more likely in poor (not in rich) countries!

In Table 4.14 we report estimates of four dynamic probit specifications, estimated only in those countries that were non-democratic in the early sixties. Columns (1a), (2a), (3a) and (4a) report the probabilities of transition (δ), while columns (1b), (2b), (3b) and (4b) give the estimates of γ. The probability that a country experienced successful transition plus consolidation, given that in the beginning of the sample period it was non-democratic is given by the sum of the γ and the δ coefficients in each model. The main results presented so far are unchanged. The modernization theory is supported in both the "*exogenous*" and to a lesser extent the "*endogenous*" version. In line with Przeworski *et al.* (2000) income mainly stabilizes rather than fosters successful transitions. The estimates also clearly illustrate the different effects of income level and growth. In all models growth has a negative impact on the likelihood of transition, validating the previous evidence on the strong association between recessions (and crises) and transitions.

4.8.2 Linear Models

Older studies have relied on much less elaborate techniques, estimating either cross-country regression models on levels of democracy or long-run changes (e.g. Bollen and Jackman (1995); Muller (1995)). In the most complete study Robert Barro (1999) investigates the effect of income, inequality, religion and various other covariates on democracy employing panel-data techniques with five-year averages. Barro's model takes the following form:

$$Democ_{i,t} = a + \beta_1 Democ_{i,t-1} + \beta_2 Democ_{i,t-1} + \gamma_1' x_{i,t} + \gamma_2' z_i + \nu_{i,t} \tag{4.3}$$

This approach utilizes information both from the cross-section and the time dimension. It also enables the estimation of both the short and long-run effects of x's and z's on the level of political freedom (*Democ*) in country i in the five-year period t. As Barro acknowledges, however, estimates are primarily driven by the "between" (rather than the "within") country variation. Moreover, in spite of using lagged values of x's as instruments for their contemporaneous value, this approach does not fully addresses issues of reverse causation. Finally, by construction this approach identifies the correlates (causal or not is not clear) of

long-run levels of democracy and one can not isolate the impact of the independent variables on the transition and consolidation of representative institutions.

The starting point of Acemoglu *et al.* (2004, 2005) is that both the non-linear (2) and linear (3) model may suffer from unobserved country-heterogeneity and omitted variable bias. As a solution to the previous limitations, they propose adding in (3) country fixed effects and also year-fixed effects to control for common global shocks. They thus estimate variants of the following model:

$$Democ_{i,t} = a_i + \eta_t + \beta_1 Democ_{i,t-1} + \beta_2 Democ_{i,t-1} + \gamma_1' x_{i,t} + \gamma_2' z_i + \nu_{i,t} \tag{4.4}$$

They show that income and education becomes insignificant when one adds country intercepts to control for country heterogeneity. In this direction, in Table XV columns (1)-(5) we estimate linear probability models in countries that enter our sample with a non democratically contested government. When we add the country dummies in column (2) income becomes insignificant. The alternative variable proposed by H_1, years of schooling is significant, but with the "*wrong*" sign. The results are thus in accord with Acemoglu *et al.* (2005a, b); But does this imply that income does not causes democracy? Not exactly. The estimates only imply that income growth does not lead to democratization. Yet this is exactly what our results show. GDP growth exerts a negative impact on democratic transitions and currency and banking crisis *positively* influence the likelihood of a democratic transition. Note also that this is not just an empirical coincidence, since there are solid theoretical reasons (Acemoglu and Robinson, 2001) for a strong negative relationship between economic performance and growth in non-democracies. Acemoglu *et al.* (2005) acknowledge this point and then show when they augment the model with time-invariant historical variables (z_i); conditioning on settler mortality and historical controls, income becomes insignificant. Although we have found considerable support for their argument that endowments and initial institutions have crucially influenced both economic and political development, we are not able to rule out the effect of income on democratization. In columns (4) and (5) we augment the linear probability model with many variables proposed by "endowment" theories [H_4]. In spite of the very high correlation between some of these measures with

income, the coefficient on the logarithm of GDP has retained its statistical significance, although its effect has decayed by a third.[36]

4.8.3 Non-linear IV models

Although our methodology by construction aims to address reverse causation and endogeneity, one could remain dubious about the interpretation of our results. In ongoing work, for example, Lopez-Cordova and Meissner (2004) employ an instrumental variables approach to circumvent the endogeneity and assess the effect of trade on democracy. In columns (6)-(10) of Table 4.15 we employ Newey's (1987) technique and report instrumental variables probit models, instrumenting GDP with legal origin dummies and years since independence (in column (6)), latitude, European heritage and a linear time-trend (in columns (7) and (8)). In columns (9) and (10) both income level and trade openness are treated as endogenous. The instrument set includes: latitude, landlocked, years-since-independence, legal origin, and European heritage. We employ various instrumenting sets, since it is almost impossible to meet the exclusion restriction that the exogenous instrument's effect on democratization only goes through income (or trade). For example, according to H_4 the age of a nation exerts an independent effect on political development. The same argument applies for European heritage and to almost every variable conceivable. Thus, although the IV estimates strengthen the main hypotheses, we interpret them solely as supportive to the previous estimates.

To summarize the first main difference between our work and previous studies is that

[36]The linear probability model especially when estimated in the universe of countries has some additional drawbacks. First the estimated probabilities can exceed the $0-1$ interval. This concern is major, especially when there are many observations with zeros and ones (as it is the case in democratization). Second by adding country fixed-effects, the estimation does not utilize both "*Always Autocratic*" and "*Always Democratic*" countries. This is because in these countries there is no variation in the dependent variable. (The same applies if one uses a level approach, since in many countries both the Polity and the Freedom House indicators do not vary). Third, due to the limited "within" variation of the dependent variable, the consistency of the estimates comes with a sizable efficiency loss. Fourth, stands measurement error. Numerous studies, for example, show that the Barro-Lee schooling statistics suffer from measurement error. Since differencing magnifies the noise to signal ratio the insignificant coefficient of education on democracy, reported by Acemoglu *et al.* does not necessarily cancel the modernization theory.

we test democratization theories on a sample of initially non-democratic countries. This is theoretically required to investigate the systematic factors that determine *why* and *when* some countries opt out of autocracy. This enables us to precisely identify the causal effect of the theoretically proposed variables on democratization and avoid capturing reverse effects from democratic institutions to income, human capital, inequality, etc. [Figure 4.1] The second departure from the existing approach is that we investigate permanent regime changes. Since theory models the economic causes and consequences of stable political regimes, this approach is therefore more appropriate, since we avoid measuring instability, coups, etc. Thirdly, we do not limit our analysis to a particular hypothesis, as most papers have done so far (an exception is Barro, 1999; and Przeworski *et al.*, 2000). Ratherwize, try to investigate the various theories during the period with the greatest advancement of representative institutions. So in one aspect our analysis is narrower than studies utilizing long-horizon data (like Acemoglu *et al.*, 2005 and Boix and Stokes, 2003), but more specific, since we test all democratization theories during the Third Wave. Fourthly, we exploit the recent "*quasi-natural*" experiment of the fall of communism to test the predictions of the main democratization theories.

4.9 Summary and Conclusion

Twenty years ago, Samuel Huntington (1984) expressed scepticism and pessimism regarding future democratic progress. In spite of the democratizations of the seventies and early eighties, the globalization of political liberalism was far from secure. Both the First and the Second Wave of Democratization were followed by reversals towards totalitarianism. Improvements in civil liberties and consolidation of democratic rule, should not, therefore, be taken for granted.

In this paper we examined the countries that entered the post-war era with non-representative institutions and identified the factors that led some of them to permanently abandon autocracy. We also exploit the fall of communism and the transition of many centrally planned economies to test the main democratization theories. Our empirical evidence suggests that

economic development, education and trade openness were the key driving forces of recent democratization events. In addition, the evidence show that an abundance of natural resource and religious fragmentation can crucially hinder the democratization process. These factors also helped reinforce democratic powers and prevented a reverse movement towards autocratic rule. We also identified the factors that influence the timing of the democratic transition. Democratizations typically follow severe banking or currency crises, which tend to be quite destabilizing for autocratic regimes. Transitions are also more likely to occur after the end of an international war or jointly with economic reforms. These results appear robust to various perturbations of the basic econometric model. We controlled for the social environment, along with alternative democratization identification methods. The results also hold for a trichotomous division of political systems, distinguishing between perfectly democratic, partially democratic and totally autocratic regimes.

Although we study almost all democratization events of the post-war era, including the liberalization that occurred after the fall of communism in the early nineties, our results simply point out where and when democratization is more likely to emerge and consolidate. Income level, education, trade openness, religion, and initial conditions shape the probabilities for democratic transition. They do not, however, determine the transition nor the consolidation of democratic governance. We believe that no single factor can fully explain the development of democratic institutions in all countries. Democratization in each region and country has always been from a combination of economic, social and political causes. Country-specific characteristics can either amplify or cancel the effect of the driving factors identified. For example, political leadership and cooperation have crucially contributed to the consolidation of democratic institutions in many countries. Assistance from the international community was also an important factor in stabilizing representative government in many countries, since in many cases Western powers appeared unwilling to support pro-democratic movements.

We focused on the systematic component of the recent democratizations and quantified, in a probabilistic framework the effect, of various theoretically proposed factors on the emergence and stabilization of representative government. A successful permanent democratic

transition requires at least two distinct events. First, the removal of an autocratic regime and the emergence of democratic forces and second the consolidation of representative institutions. In our research we tried to identify and isolate which factor affects these two requirements. More theoretical and empirical research is needed to clarify which features drive each of these channels. Finally, additional research is required to show how non-democratic countries can move to a representative government at minimal transition costs (avoiding for example a civil war or a financial crisis) and how newly-established democracies can cope with adverse and destabilizing economic shocks.

Appendix A

Data Filtering and Impulse Responses under Measurement Error in the Regressors

A.1 To difference or not difference

The issue of whether the variables in the VAR need to be differenced exists. We estimate a VAR model in levels as many economists such as Sims (1980) and Doan (1992) recommend against differencing even if the variables contain a unit root. The main argument against differencing is that it *"throws away"* information concerning the comovements in the data and removes any cointegrating relationships. It is also noted that the goal of a VAR analysis is to determine the interrelationships among the variables, not the parameter estimates, as it is the case in this paper. Moreover, Fuller (1976) shows that differencing produces no gain in asymptotic efficiency in an autoregression, even if it is appropriate. He notes that in a VAR, differencing throws information away while it produces almost no gain.

Conventional unit root tests fail to reject the presence of nonstationary components in nominal exchange rates. However, we do know that long-run tests run over a century and more of data can reject a unit root (e.g. Lothian and Taylor (1996)). Taylor (2001) documents that in the short samples that have been used in empirical studies, unit root tests have very low power. The main determinant of the power of a conventional unit root test

is neither the length nor the frequency of a dataset, but its span (e.g. Shiller and Perron (1985)). Moreover, Taylor (2001) shows how the power of a standard unit root test is affected by a variety of spans. With an autoregressive coefficient of 0.846, which is the highest in our sample, the power of the test would be 0.5-0.6 if we had 300 observations! Many concerns have been also expressed for the first difference filter that is usually applied to nonstationary time series, particularly when non-stationarity has been indicated by conventional unit root tests. Baxter (1994) emphasized that differencing of a near unit root process would be inappropriate and would discard valuable information. Although first differencing will remove unit root components, this filter has several drawbacks. It alters timing relationships between variables by inducing a substantial phase shift and it involves a dramatic re-weighting of frequencies – high frequency (noise) components are emphasized at the expense of down-weighting lower frequencies: in particular, much of the *'cyclic'* variation is removed. To understand how application of this filter alters the data consider any filter $H(L)$, and two time series Y_t and X_t such that $Y_t = H(L)X_t$ with population spectrums:

$$\begin{aligned} S_{yy}(\varpi) &= h(e^{-i\varpi})h(e^{-i\varpi})S_{xx}(\varpi) \\ S_{yx}(\varpi) &= h(e^{-i\varpi})S_{xx}(\varpi) \end{aligned} \tag{A.1}$$

where $h(e^{-i\varpi})$ is the *transfer function* or *frequency response function* with gain $R(\varpi) = |h(e^{-i\varpi})|$ and phase $\theta(\varpi) = \arg\left[h(e^{-i\varpi})\right]$, with $0 \leq \varpi \leq \pi$. In the special case of a first difference filter $H(L)$ reads: $H(L) = 1 - L$. If we apply this filter to a stationary process Y_t, this imposes certain gain and phase transformations on the spectrum of the raw series (see also Baxter (1994) and Obstfeld and Taylor (2001)). From (13) we can obtain the transfer function as well as the face and the gain of a first difference filter. When $H(L) = 1 - L$ then (13) reads:

$$h(e^{-i\varpi}) = (1 - \cos\varpi) + i\sin\varpi \tag{A.2}$$

and:

$$R(\varpi) = 2 - 2\cos\varpi \tag{A.3}$$

$$\theta(\varpi) = \frac{\pi}{2} - \frac{\varpi}{2} \tag{A.4}$$

The first difference filter has very low gain at low frequencies such that regressions involving the filtered data tend to pick up correlations between two variables only at the high end of the frequency spectrum . The main concern that is related to our analysis is that, with monthly data, the differenced exchange rates, when regressed with capital flows data, might lead to a correlation that is greatly biased towards high frequencies. To put it differently, the relationship between capital flows and exchange rates might be tighter at lower frequencies but it could remain obscured since this range of the spectrum is deeply attenuated when a first difference filter is applied.

A.2 Impulse responses with measurement error in the regressors

This section gives a detailed description of the proposed methodology to assess the significance of impulse responses under errors in the regressors. Recall the simultaneous equation setup of an unrestricted VAR(p). If $z_t = (x_{t-1}, x_{t-2}, \ldots : y_t)$ and $\Gamma = (D_1', D_2', \ldots, a)'$ is a $(q + pN) * N$ matrix then:

$$x_t = z_t\Gamma + e_t \tag{A.5}$$

where $e_t \backsim (0, \Sigma_e)$. It is known that the distribution of the likelihood function of a VAR(p) is the product of a normal density, conditional on the OLS estimates, and an inverted Wishart distribution with $(T - \xi)$ degrees of freedom, T denoting the number of observations, and ξ denoting the number of estimated coefficients in each equation. More formally:

$$L(\gamma, \Sigma_e) \propto (Normal \times inverted\ Wishart) \tag{A.6}$$

where γ are the OLS estimates of $D's$. If we assume a flat prior for $\gamma, \Sigma, : (\gamma, \Sigma_e) \propto |\Sigma_e|^{\frac{1(N+1)}{2}}$ then the posterior is equal to the likelihood multiplied by the prior and reads:

$$post(\gamma, \Sigma_e) = L(\gamma, \Sigma_e) \times flatprior(\gamma, \Sigma_e) \tag{A.7}$$

Note also that: $post(\gamma, \Sigma_e) = post(\gamma \mid \Sigma_e) \times post(\Sigma_e)$, where the posterior of Σ_e is distributed as an inverted Wishart with $(T - \xi)$ degrees of freedom and the posterior of $(\gamma \mid \Sigma_e)$ can be obtained from $x^* \backsim N(\gamma_{OLS}, var(\gamma_{OLS}))$. Even if the estimated $\widehat{D}'s$ are biased, the distribution will be exact.

Next, consider the following logical algorithm. Generate $T - \xi$ i.i.d. draws for e_t from the inverse $N(0, (X - Z\Gamma_{OLS})'(X - Z\Gamma_{OLS}))$. Form the second moments by taking $\Psi = \frac{1}{T-\xi}\sum_i (e_i - \frac{1}{T-\xi}e_i)^2$. Then set $\Sigma^h = \Psi^{-1}$. Next construct $\gamma^h = \gamma_{OLS} + \mu_t$ by drawing μ_t from the $N(0, \Sigma^h)$. Now compute the h^{th} impulse response at step k for each draw h. If we repeat this algorithm for $H = 1000$ times we can generate the theoretical distribution of impulse responses from the asymptotic distribution of the estimated coefficients. The resulting 2 standard error band shows where the impulse response lives for any bias in the estimated coefficients due to measurement and sampling error in equity flows.

Appendix B

A New Democratization Data-Set

B.1 Methodology

This Appendix describes in detail the methodology in identifying and timing permanent democratic transitions. It also provides a brief description of the democratic transition. We start by analyzing the evolution of the two most widely used democracy measures: the Polity democracy index and the Freedom House (FH) political rights and civil rights indicators.

The Center for International Development and Conflict Management at the University of Maryland produces various quantitative measures of political characterizations in its Polity project (edition IV). We focus on the composite democracy index (Polity 2). The index ranges from -10, indicating the full absence of representative institutions and civil rights to $+10$ indicating the perfect protection of political freedom. The index is constructed by subtracting a 0 to 10 autocracy measure (AUTOC) from a similar 0 to 10 institutionalized democracy (DEMOC) indicator. The Polity index reflects the degree of competitiveness in political participation, the openness in the selection of the executive and legislative branch, and the constitutional constraints on the executive. It also incorporates subjective information on the absence of checks and balances to executive powers, the degree of restrictions in electoral participation, and to which extent the political participation is regulated.

The Non-Governmental Organization Freedom of the World in its Freedom House product

reports a score of political rights and civil liberties. Both measures range from 1 to 7 with lower values indicating a higher level of protection. Data are reported on an annual basis, starting in 1972 and cover almost all independent states. FH also designates to each country a trichotomous polity status characterization of either "free" (F), "partly free" (PF), or "not free" (NF). Countries whose combined average score of political rights and civil liberties is less than 2.5 are assigned "free", while those with a greater than 5.5 score are designated as "not free"; countries with an average score of between 2.5 and 5.5 are designated "partly free".

First we identified a sudden and permanent jump (fall in the FH measures) of either indicator. Since we are interested in identifying the nature and the timing of a permanent regime change, we recorded an event change when: 1) the Polity index jumped from a negative to a positive value and remained positive for at least five years, 2) the FH status jumped from "NF" to "PF" or "F" and remained at the improved democracy status for at least five years (changing this requirement to seven, six, four or three years does not alter the main results). In South Korea, for example, "free and fair elections" were held on December 1987. The new democratic constitution that established a multi-party representation, however, came into effect the following year. We therefore use 1988 as the democratization year for South Korea. In most cases the two indicators yielded similar results with minor changes in the exact timing. For example, in Guyana the Polity indicator jumped from -6 to $+7$ in 1992 (when the first free post-independence elections were held), while the FH characterization jumped to "F" in 1993. For those countries that democratized after 1998 and are included in our sample, we validated that they fulfill the stability requirement beyond the year 2000 (e.g. Djibuti democratized in 1999 according to both indicators. Until 2003 neither of the two ratings has fallen back to "autocratic" status).

Second, we delved into numerous historical and political science resources. This was done to fully address the documented conceptual and measurement problems of the democracy variables and to precisely identify the nature of each event. Our main resources were: 1) the detailed Freedom House and Polity Project country reports, 2) the Country Studies/Area Handbook Series of the Federal Research Division of the United States Library of Congress,

3) the Central Intelligence Agency (CIA) World Factbook, 4) the U.S. Department of State "Background Country Notes" and 5) the various country releases of the non-governmental organization "Human Rights Watch". We identify a democratization episode at the timing of the first internationally deemed "free and fair" elections after a prolonged period of autocratic rule. Data on elections were taken from 1) Adam Carr's Election archive and "Elections around the World" online data-set. and 2) Election Results Archive, collected by the Center on Democratic Performance at Binghamton University.

The criterion for dating a successful democratic transition is either at the timing of "free and fair" elections or at the adoption of a democratic constitution after a prolonged period of autocratic rule. In most cases these two events coincide; When they do not, we use the latter date. In South Korea, for example, internationally deemed "free and fair elections" were held on December 1987. The new democratic constitution that established a multi-party representation, however, came into effect the following year. We therefore use 1988 as the democratization year for South Korea.

Although this method of identifying the date of a stable regime change is consistent with the score of the Polity and the FH indicators for almost all countries, in some countries democratic transition was followed by political turmoil and struggles. Since we want to control for the transition, we allow for the stability of democratic institutions. We therefore identify permanent democratization episodes when there is a de facto transfer of power to a democratically elected government. In Bolivia, for example, the military did not initially recognize the outcome of the free elections of 1980. Yet, they were forced to hand over power to the democratically elected President in 1982. So, 1982 marks the transition to democracy (in line with our methodological approach both the Polity and the FH indicators jumped in 1982). In Guatemala, likewise, although the first "free and fair" elections after the military junta occurred in 1986, several coups and civil conflicts followed until 1996, when a U.N. supported accord was signed with leftist guerillas and new elections were conducted. In the example of Guatemala we therefore use 1996 as our event date (the Polity indicator jumped in 1983 whereas the FH indicator jumped in 1996).

In some countries, however, one of the two sources does not report a democracy score. The Polity database, for example, excludes countries with a population less than half a million (excluding mainly some small Caribbean nations), while the FH indictors start in 1972-1973 period. In these cases we relied on either of the two indexes and to country specific sources. An open question is furthermore how to treat the newly established countries that followed the collapse of the Soviet Union, Yugoslavia and Czechoslovakia. Since all three of these former countries were classified as nondemocracies, we include in the democratization sample those nations that not only became independent, but also managed to consolidate representative government. Consequently the Baltic republics, for example, as classified as countries experiencing a democratization event. The results are robust to the exclusion of the countries, where democratization occurred alongside independence (e.g. Estonia, Latvia, Slovak Republic, etc.). A related problem was how to handle some African countries, which during the first post-independence years (in the early sixties) had democratic constitutions, but immediately (by mid-sixties) reversed to non-democartic status (Benin, for example, was classified as democracy during the 1960-1962, but after the 1963 military coup that ousted President Hubet Maga turned to autocratic status; other similar examples include Congo, Kenya, Lesotho, Nigeria, Uganda, etc.). We decided to follow a strict definition of democracy and considered these small intervals as non-successful democratic reforms.

Third, since not all democratizations yielded the same level of political freedom, we distinguished between "full" and "partial" democratizations. A "full" democratization event is identified when a country experienced a democratic transition that led to an almost perfect protection level of political rights and civil liberties. To classify a country as having experienced a "full" democratization, we require both a FH status designation of "free" and a Polity score greater than +7. Examples of "full" democratization include Spain after Franco's death, Argentina's democratization after the Falkland's War, or Chile after the stepping-down of Pinochet. In "partial democratization" incidents, in contrast, although representative institutions have been established and free and fair elections have taken place, the level of political liberties and civil rights has not reached an almost perfect score. In "partial democratization" episodes, the two democracy measures have, consequently, not reached a

maximum score. Examples of partial democratizations include Nigeria, Russia, and many African countries that have recently moved towards representative institutions. We further identify 5 borderline cases of "partial democratization" episodes, namely Iran (1997), Jordan (1993), Niger (1999), Pakistan (1988), and the Central African Republic (1993), where one could reasonably argue that in spite of some democratic progress, political participation and civil freedoms are still at a very low level. Since, however, both our methodology, the the FH and the Polity index, indicate that considerable progress towards representative government has occurred, we do include them in our dataset. We also note that one could also add to the borderline cases Guinea-Bissau. Yet although some reforms took place in 1994, there was recently a significant reversal to autocracy.

We also identify the timing and the event depending solely on the Polity and Freedom House indicators. By doing so, we also drop all countries that emerged from the collapse of Czechoslovakia U.S.S.R. and Yugoslavia. These 22 countries are: Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina , Croatia, Czech Republic, Estonia, Georgia, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, FYROM, Moldova, Russia, Slovakia, Slovenia, Tajikistan, Turkmenistan, Ukraine, and Uzbekistan.

We identify a Polity permanent regime change when the Polity index jumps from a negative to a positive value and remains there for five years. When there are consecutive jumps in two or three years, we use the later year. For example in Philippines the Polity index jumps from -6 to $+1$ in 1986 and further to $+8$ in 1987. So we use 1987 as the democratization year for the Philippines. In most episodes there are only minor differences with our chronology mainly in the exact timing of the event. For example in Albania democratization according to our methodology occurred in March 1992 at the time of the General assembly elections, while according to the Polity index democratization occurred in 1990, when the communist system collapsed. Likewise in Chile the Polity index jumped in 1989, while we use the 1990 Presidential election as the timing of transition. Since the Polity index is not reported for some small nations, so Cape Verde, Grenada, São Tome and Principe, and Suriname are excluded, while Guinea-Bissau (1994) is included in the democratization countries sample. For robustness we also considered a smaller threshold of democratic transitions, of -3. Ac-

cording to this less strict definition: Angola (1992), Algeria (1995), Guinea (1995), Kenya (1997) and Chad (1996) are included in the reforming group. In addition for Guatemala now the date of transition is 1986, for Thailand in 1978 and for Tanzania in 2000.

We identify a FH permanent regime change when the Freedom House characterization jumps from "Not Free" to either "Free" or "Partially Free". When there are consecutive jumps from NF to PF and then to F (like in Argentina, Romania, and Benin), we use the latter date. There are still some minor discrepancies with our (or with the Polity index) methodology at the exact timing. For example according to both our and to the Polity classification democratization occurred in Argentina in 1983, while according to the FH democratization took place in 1984. Yet the FH classification has some noteworthy differences with our methodology. When we use the Freedom House database Cyprus (1981) and Guinea Bissau (1991) are now included in the democratization sample. In addition according to FH no democratization has taken place in: Bangladesh, Iran, Madagascar, Nicaragua, Paraguay, Senegal, Turkey, Zambia or even Indonesia and Lesotho. Other noticeable differences are the timing of democratization in Nepal (in 1979), in Guatemala (in 1984), in Ghana (in 1992), and in Mexico (in 2000).

Panel *A* of this Appendix gives a detailed chronology of the democratic transition in countries that experienced a "full" democratization. Panel *B* gives the chronology of "partial democratization", while Panel *C* gives that of "borderline" democratization events. In Panel *D*, we report the incidents of "adverse" reversals, i.e. transition form a stable democracy to autocracy. Below each episode we report (in italics): First the evolution of the 21-scale Polity index. Second, the change of the FH regime classification. Third the regime characterization of Przeworski, Alvarez, Cheibub, and Limongi (2000), which, however, does not cover the nineties (1950-1990 period). There are two non-democartic regimes, "bureaucracies", defined as "institutionalized regimes that promulgated laws" and "autocracies", defined as "regimes without any proclaimed rules"; the democratic regimes are further classified as "parliamentary", "presidential" or "mixed" (Przeworski *et al.*, p. 48-49). Fourth we document Persson's (2005) characterization of regime transition in the 1962-1998 period. Although Persson's event identification employes the Polity index, using a similar to

ours cutoff value, his chronology contains information on the form of democratic government (whether the democratic transition established a presidential or a parliamentary system) and the form of the electoral system (majoritarian or proportional). In Panel E we report for completeness the non-reforming countries, "permanent democarcies" and "permanent stable autocracies". Finally we present analytical country graphs with the evolution of growth in each of the transition countries as a supplementary appendix after the end of all chapter Tables.

Appendix C

Data Sources and Variable Definitions

In this Appendix I provide detailed variable definitions and sources for each of the four Thesis Chapters.

C.1 Chapter 1: A model of real exchange rates and real consumption spending with time varying discount factors

Quarterly consumption spending: End of period, seasonaly adjusted total household consumption spending in local currency. Source EcoWin.

CPI: End of period, seasonally adjusted consumer price index. Source EcoWin.

Exchange rates. End of period rates. Source EcoWin.

C.2 Chapter 2: Capital Flows and Exchange Rates

Capital Flows: The data series is based on submissions of monthly TIC Form S, 'Purchases and Sales of Long-Term Securities by Foreigners' millions of US dollars. These reports are mandatory and are filed by banks, securities dealers, investors, and other entities in the U.S. who deal directly with foreign residents in purchases and sales of long-term securities

(equities and debt issues with an original maturity of more than one year) issued by U.S. or foreign-based firms. The data series are revised for up to 24 months after the initial 'as of' reporting date.

The data reflect only those transactions between U.S. residents and counterparts located outside the United States. The data cover transactions in six classifications of securities: There are four domestic types of securities, which include U.S. Treasury bonds and notes, bonds of U.S. government corporations and federally-sponsored agencies, U.S. corporate and other bonds, and U.S. corporate and other stocks; and two foreign types of securities, namely foreign bonds and foreign stocks.

Please note that the geographical breakdown of Form securities transactions indicates country of location of the foreign buyers and sellers who deal directly with entities resident in the U.S. (i.e., reporting institutions). The data do not necessarily indicate the country of beneficial owner or issuer, or the currency of denomination of securities. For instance, a U.S. purchaser's order for Japanese securities may be placed directly with an intermediary in London. In this instance, the transaction for Form S reporting purposes would be recorded opposite the U.K. and not opposite Japan. Similarly, purchases and sales of U.S. securities for the account of an Italian resident may be placed, for example, in the Swiss market. In such an arrangement, the trades would be reported opposite Switzerland and not opposite Italy

(Available at http://www.treas.gov/tic)

Exchange Rates: Monthly averages of nominal exchange rates obtained from IFS, CD-Rom. We could also use end of period rates. Results are dentical with the ones reported here.

Stock returns indices: Germany: CDAX Composite Price Index, Japan: Nikkei 225 Stock Average, UK: FT-Actuaries All-Share Index, Switzerland: Swiss Market Index and S&P 500 Composite index. All in lcal currencies obtained from EcoWin.

Stock market capitalization: monthly averages in millions of US dollars obtained

from Global Financial Data, Inc.

Interest rates: Monthly averages of short term money market rates obtained from IFS, CD-Rom. We could also use end of period rates. Results are dentical with the ones reported here.

C.3 Chapter 3: Democartization and Growth

Ln. GDP Level: Natural logarithm of real per capita GDP. GDP per capita based on purchasing power parity (PPP). PPP GDP is gross domestic product converted to international dollars using purchasing power parity rates. GDP is the sum of gross value added by all resident producers in the economy plus any product taxes minus any subsidies not included in the value of the products. Data are in current international dollars. *Source: World Bank World Development Indicators CD-ROM (2002 Edition).*

GDP growth: Real per capita GDP growth is defined as the annual logarithmic change of real per capita GDP. *Source: World Bank World Development Indicators CD-ROM (2002 Edition).*

Investment: Gross capital formation relatively to GDP. Gross capital formation consists of outlays on additions to the fixed assets of the economy plus net changes in the level of inventories. *Source: World Bank World Development Indicators CD-ROM (2002 Edition).*

Government Consumption: General government final consumption expenditure (general government consumption) measured as a share of GDP. It includes all government current expenditures for purchases of goods and services (including compensation of employees). It also includes most expenditures on national defense and security, but excludes government military expenditures that are part of government capital formation. *Source: World Bank World Development Indicators CD-ROM (2002 Edition).*

Trade: The sum of exports and imports of goods and services measured as a share of gross domestic product. *Source: World Bank World Development Indicators CD-ROM*

(2002 Edition).

Population Growth: Annual logarithmic change in population. *Source: World Bank World Development Indicators CD-ROM (2002 Edition).*

Schooling: Average years of schooling in the population aged 15 and above. *Source: Barro and Lee (2001)*

High and Low School: Countries that democratized and had above median schooling are: Argentina, Bulgaria, Chile, Ecuador, Spain, Guyana, Hungary, Indonesia, South Korea, Lesotho, Mexico, Panama, Peru, Philippines, Poland, Paraguay, Romania, Russia, Thailand, Uruguay, South Africa, Zambia. Countries that had below median average years of schooling are: Benin, Bangladesh, Ghana, Guatemala, Honduras, Haiti, Mali, Mozambique, Malawi, Nepal, and Senegal. The following countries democratized when average years of schooling were below median, but soon after the transition their schooling level increased above the median value: Bolivia, Brazil, the Dominican Republic, Nicaragua, Portugal, El Salvador, and Turkey.

Life Expectancy: Life expectancy at birth indicates the number of years a newborn infant would live if prevailing patterns of mortality at the time of its birth were to stay the same throughout its life. The data has some arbitrary gaps. A linear interpolation is used to fill in these gaps. At the regressions the variable is entered as the logarithm of the interpolated series. *Source: World Bank World Development Indicators CD-ROM (2002 Edition).*

Trade Openness: A zero-one variable for trade openness based on five individual dummies for specific trade-related policies. A country was classified as closed if it displayed at least one of the following characteristics: (1) Average tariff rates of 40% or more (2) Non-tariff barriers covering 40% or more of trade. (3) A black market exchange rate that is depreciated by 20% or more relative to the official exchange rate, on average, during the 1970s or 1980s (4) A state monopoly on major export. (5) A socialist economic system. *Source: Wacziarg and Welch (2003).*

Independence period: Years, since independence, normalized $0-1$. For countries that became independent before 1800, we use the 1800 as independence date. *Source: CIA Factbook, 2003 edition.*

Religious/Ethnic Fractionalization: Index of religious/ethnic heterogeneity. Constructed as one minus the Herfindahl index of the share of the largest religious/ethnic groups. It reflects the probability that two randomly selected individuals follow different religious/ethnic beliefs. Specifically, $RFract_i = 1 - \sum_{j=1}^{j} s_{i,j}$ where $s_{i,j}$ is the relative size of religion j in country i. *Source: Alesina et al. (2003)*

Muslim: Share of population that follows the Muslim religion. Estimated as the sum of shares of the Muslim, Sunni Muslim, Shii Muslim and Ibadiyah Muslim. *Source: Alesina et al. (2003).*

Confucian: Share of the population that follows the Confucian-Buddist religion. Estimated as the sum of shares of Buddhist, Lamaistic Buddhist, Confucian, Chondogyo, and Shintoist. *Source: Alesina et al. (2003).*

Oil: Indicator variable that equals one if the country is a member of the OPEC (Organization of the Petroleum Exporting Countries) or is classified by the IMF as a fuel exporting country. *Source: IMF World Economic Outlook 2003 (Statistical Appendix); OPEC web site http://www.opec.org/*

C.4 Chapter 4: What Determines Democratization?

C.4.1 Panel A: Variables used in benchmark analysis

GDP: Real per capita GDP. In the regressions the natural logarithm is used. *Source: World Bank World Development Indicators (2004 edition)*

Schooling: Average years of schooling in total population. *Source: Barro and Lee (2001).*

GDP Ratio: Current per capita GDP expressed relative to the United Sates ($US = 100$) in each year. *Source: Penn World Tables 6.1 edition.*

Growth: Annual logarithmic change of real per capita GDP. *Source: World Bank World Development Indicators (2004 edition)*

Urbanization: Share of the total population living in areas defined as urban in each country. *Source: World Bank World Development Indicators (2004 edition)*

Radios: Radios per 1000 people. Radios refer to radio receivers in use for broadcasts to the general public. The data has some arbitrary gaps and a linear interpolation has been applied. *Source: World Bank World Development Indicators (2004 edition)*

Literacy: Youth literacy rate is the percentage of people ages $15-24$ who can, with understanding, read and write a short, simple statement on their everyday life. *Source: World Bank World Development Indicators (2004 edition)*

Independence: Years since independence, normalized $0-1$. For countries that became independent before 1800, we use the 1800 as independence date. *Source: CIA Factbook, 2003 edition.*

Trade Openness: A zero-one variable for trade openness based on five individual dummies for specific trade-related policies. A country was classified as closed if it displayed at least one of the following characteristics: 1) Average tariff rates of 40% or more; 2) Non-tariff barriers covering 40% or more of trade; 3) A black market exchange rate that is depreciated by 20% or more relative to the official exchange rate, on average, during the 1970s or 1980s; 4) A state monopoly on major export; 5) A socialist economic system. *Source: Wacziarg and Welch (2003), who update and extend Sachs and Warner (1995) variable.*

Trade Liberalization: An indicator variable that takes on the value one if a permanent trade liberalization has occurred during the current or the past three years. *Source: Wacziarg and Welch (2003)*

Trade/GDP: Sum of exports and imports of goods and services measured as a share of

GDP. *Source: Penn World Tables 6.1 edition.*

Oil: Fuel exporting countries dummy variable, as defined by the IMF. It takes on the value of one in the following countries: Angola, United Arab Emirates, Bahrain, Brunei, Congo, Rep., Algeria, Gabon, Iran, Iraq, Libya, Nigeria, Oman, Qatar, Saudi Arabia, Turkmenistan, Trinidad and Tobago, Venezuela. For robustness we also defined an OPEC (Organization of the Petroleum Exporting Countries) member countries dummy. OPEC member countries are: United Arab Emirates, Algeria, Indonesia, Iran, Iraq, Kuwait, Libya, Nigeria, Qatar, Saudi Arabia, Venezuela. *Source: IMF World Economic Outlook 2003 (Statistical Appendix); OPEC web site http://www.opec.org/*

Religious Fragmentation: Index of religious heterogeneity. Constructed as one minus the Herfindahl index of the share of the largest religious groups. It reflects the probability that two randomly selected individuals follow different religious beliefs. Specifically, $RFract_i = 1 - \sum_{j=1}^{j} s_{i,j}$ where $s_{i,j}$ is the relative size of religion j in country i. *Source: Alesina et al. (2003)*

Ethnic Fragmentation: Index of ethnic heterogeneity. Constructed as one minus the Herfindahl index of the share of the largest ethnical groups. It reflects the probability that two randomly selected individuals belong to different ethnical group. Specifically, $EFract_i = 1 - \sum_{j=1}^{j} s_{i,j}$ where $s_{i,j}$ is the relative size of group j in country i. *Source: Alesina et al. (2003)*

Armed Conflict Ending: An indicator variable that takes on the value one if a major armed conflict has ended during the current or during the past three years. A major armed conflict is defined as at least 25 battle-related deaths per year and an accumulated total of at least 1,000 deaths (originally defined as "*intermediate armed conflict*") or at least 1,000 battle-related deaths per year (defined as "war"). For robustness we also used an alternative indicator variable that takes on the value one if a war has ended during the current or during the past three years. *Source: Armed Conflict Dataset (2003), International Peace Research Institute, Oslo (PRIO)*

Currency Crisis: A dummy variable that takes on the value one if a currency crisis

has occurred in the current or during the last three years. *Source: Kraay (2003)*

Banking Crisis: A dummy variable that takes on the value one if a systematic or a minor banking crisis is in place. *Source: Caprio and Klingebiel (2003)*

Settler Mortality: Mortality rates faced by potential European settlers around 1800 – 1900. *Source: Acemoglu, Johnson, and Robinson (2001).*

Common Law and other Legal origin: Identifies the legal origin of the Company Law or Commercial Code of each country. There are five categories: (1) Common law; (2) French civil law; (3) German civil law; (4) Scandinavian civil law; (5) Socialist/Communist law. *Source: La Porta et al. (1999).*

Latitude: Absolute distance from the equator. *Source: Dollar and Kraay (2003)*

European Heritage: Percentage of the population speaking English or a major European Language. For robustness we also used the fraction of the population that speak English. *Source: Dollar and Kraay (2003), who update and extend Hall and Jones (1999) measure.*

Gini: The variable ranges from 0 till 1 with higher values indicating higher income inequality. The data is available for only 71 countries and has many gaps. In the regression we use the last available value. For robustness we also used five and a ten year moving average as well as linearly interpolated values. We also adjusted the income inequality data following Dollar and Kraay (2003). *Source: World Bank's Inequality Around the World Database; combines data from Deininger-Squire, Dollar and Kraay, UN-WIDER World Income Inequality Database,and World Income Distribution.*

C.4.2 Panel B: Variables used in the cross-sectional estimates on former centrally-planned economies

Fertility: Births per woman. Total fertility rate represents the number of children that would be born to a woman if she were to live to the end of her childbearing years and bear

children in accordance with prevailing age-specific fertility rates. *Source: World Bank World Development Indicators (2004 edition)*

GDP: Real per capita PPP-adjusted GDP in 1989 US dollars. *Source: de Melo, Denizer, Gelb and Tenev (2001)*

Industrialization: Share of industry in GDP in 1990, estimated at current prices. *Source: de Melo, Denizer, Gelb and Tenev (2001)*

Central Planning: Years that the country was under central planning in 1990. *Source: de Melo, Denizer, Gelb and Tenev (2001)*

Mean Growth: Average real GDP growth during 1985-1989. *Source: de Melo, Denizer, Gelb and Tenev (2001)*

"Rich" Natural Resources: Indicator variable that equals one if a country had relatively to the other transition countries rich natural resources (mainly natural gas and oil). The variable equals one for Kazakhstan, Russia and Turkmenistan and zero otherwise. *Source: de Melo, Denizer, Gelb and Tenev (2001)*

"Moderately Rich" Natural Resources: Indicator variable that equals one if a country had relatively to the other transition countries rich natural resources, but does not belong to the "rich natural resources" category. The variable equals one for Poland, Rumania, Georgia, Ukraine, Uzbekistan, Mongolia, China, and Vietnam and zero otherwise. and zero otherwise. *Source: de Melo, Denizer, Gelb and Tenev (2001)*

Natural Trade Openness: This variable measures a country's geography predicted trade. It is taken from Dollar and Kraay (2003) and estimated as follows: (*i*) Estimate gravity equations using bilateral trade shares. (*ii*) Retrieve predicted bilateral trade by applying coefficient estimates to data on RHS variables for all countries. (*iii*) Sum across all trading partners to get predicted bilateral trade. (*iv*) Take logs. *Source: Dollar and Kraay (2003), who update and extend Frankel and Romer (1999) measure* alance as a Percentage of GDP (30%).

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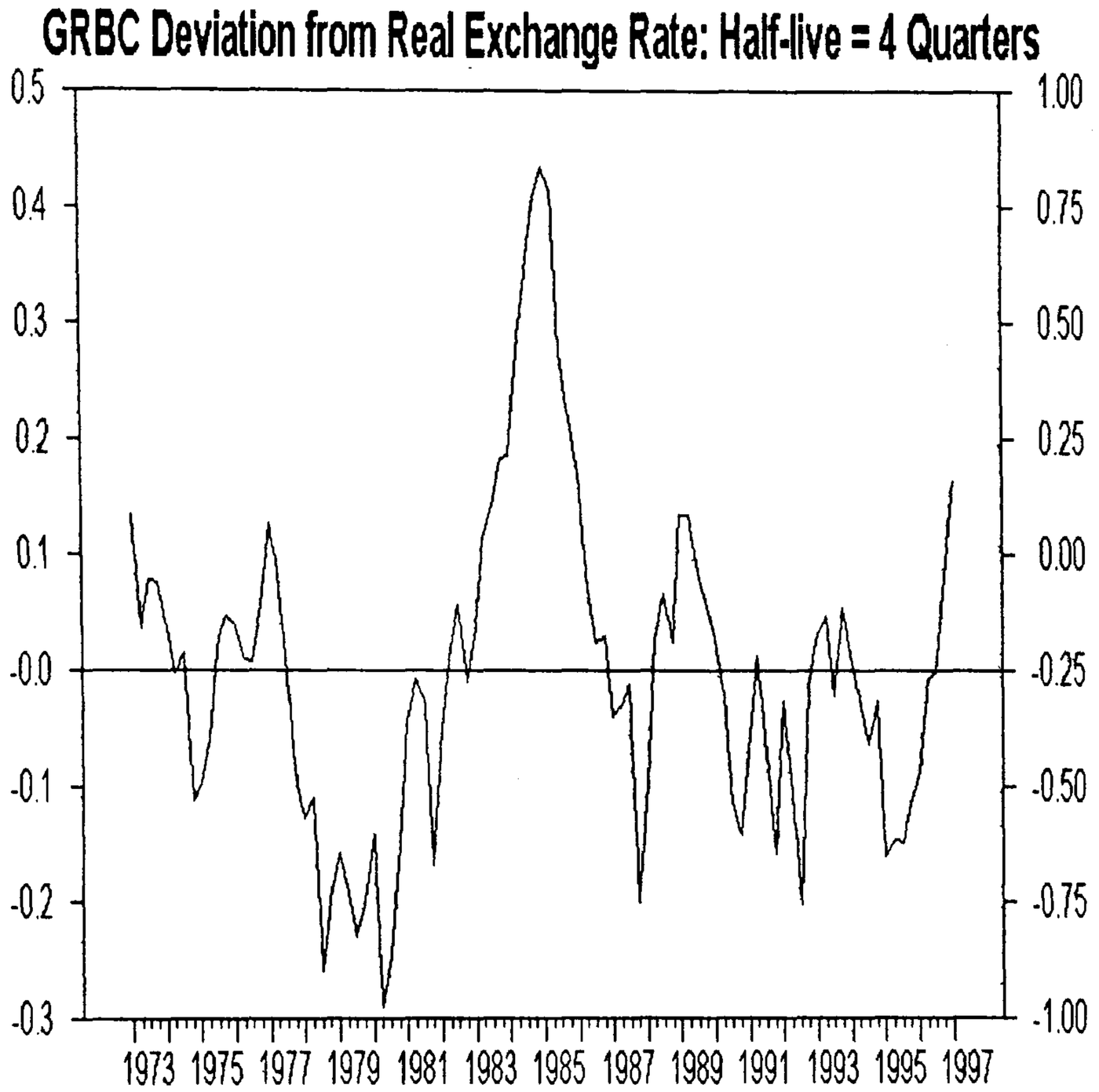
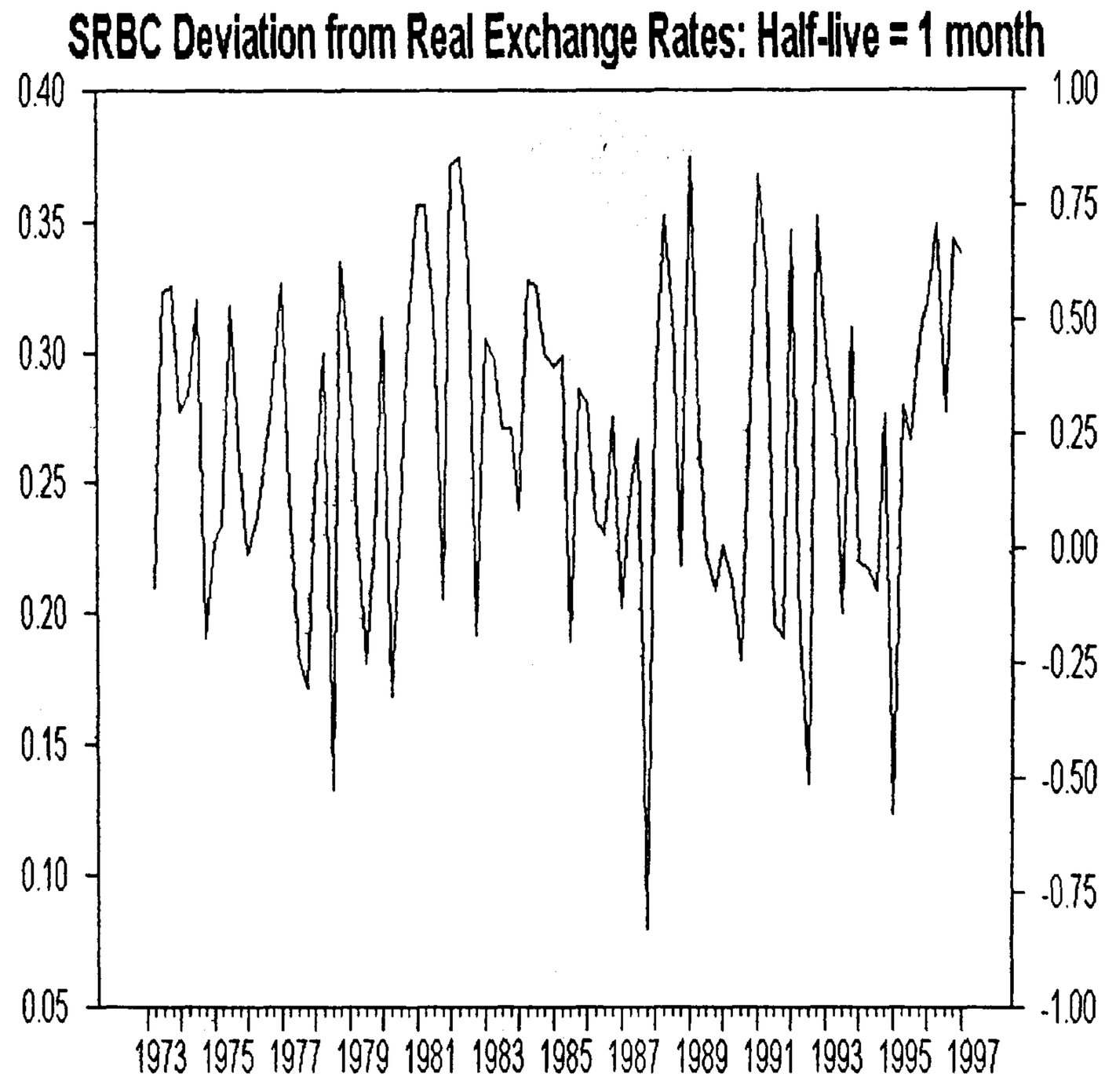
Figures

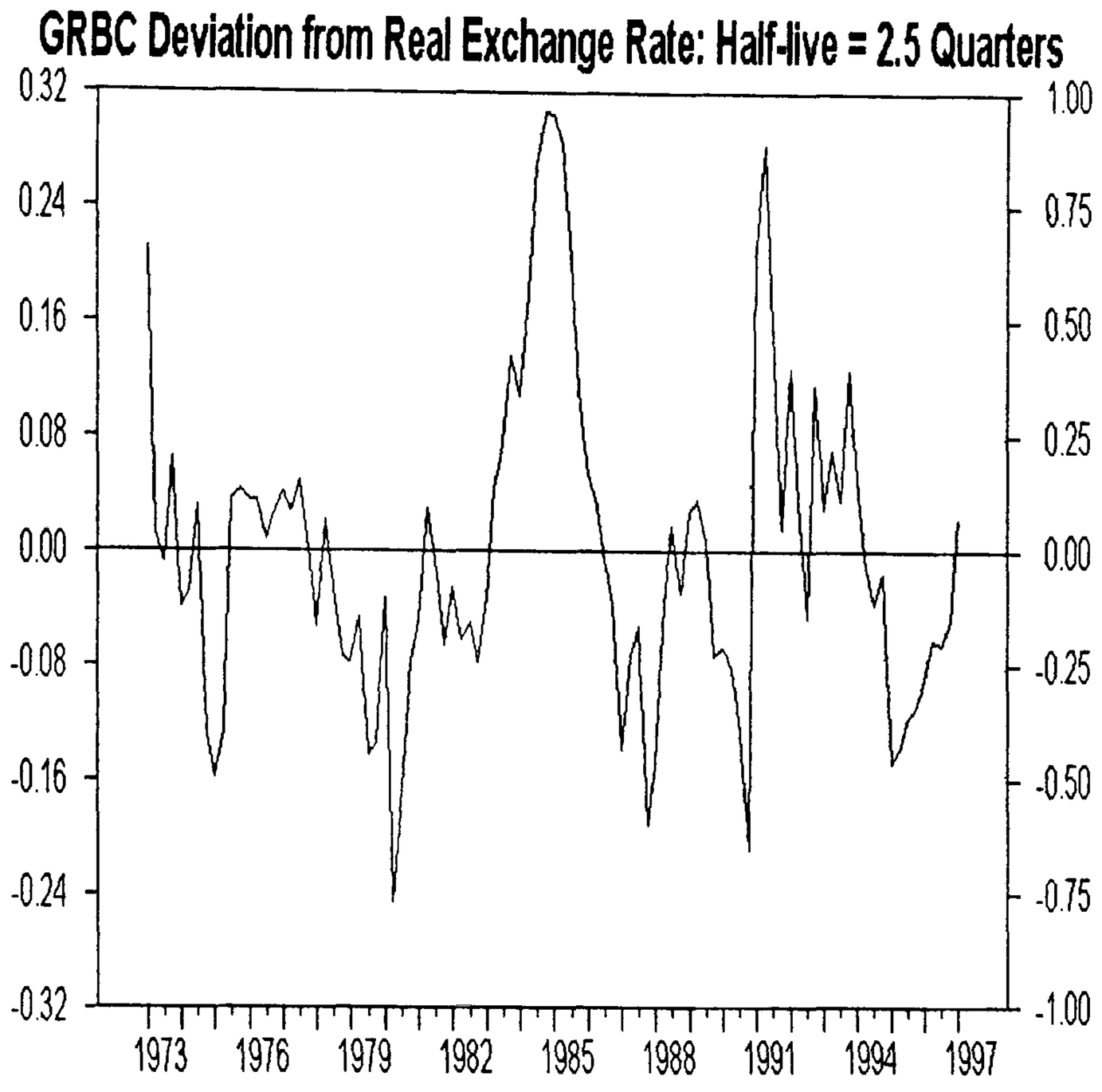
Figure 1.1: Estimated Deviations from Real Exchange Rates from the Generalized Real Business Cycle Model (GRBC) and the Sinusoidal Real Business Cycle (SRBC)

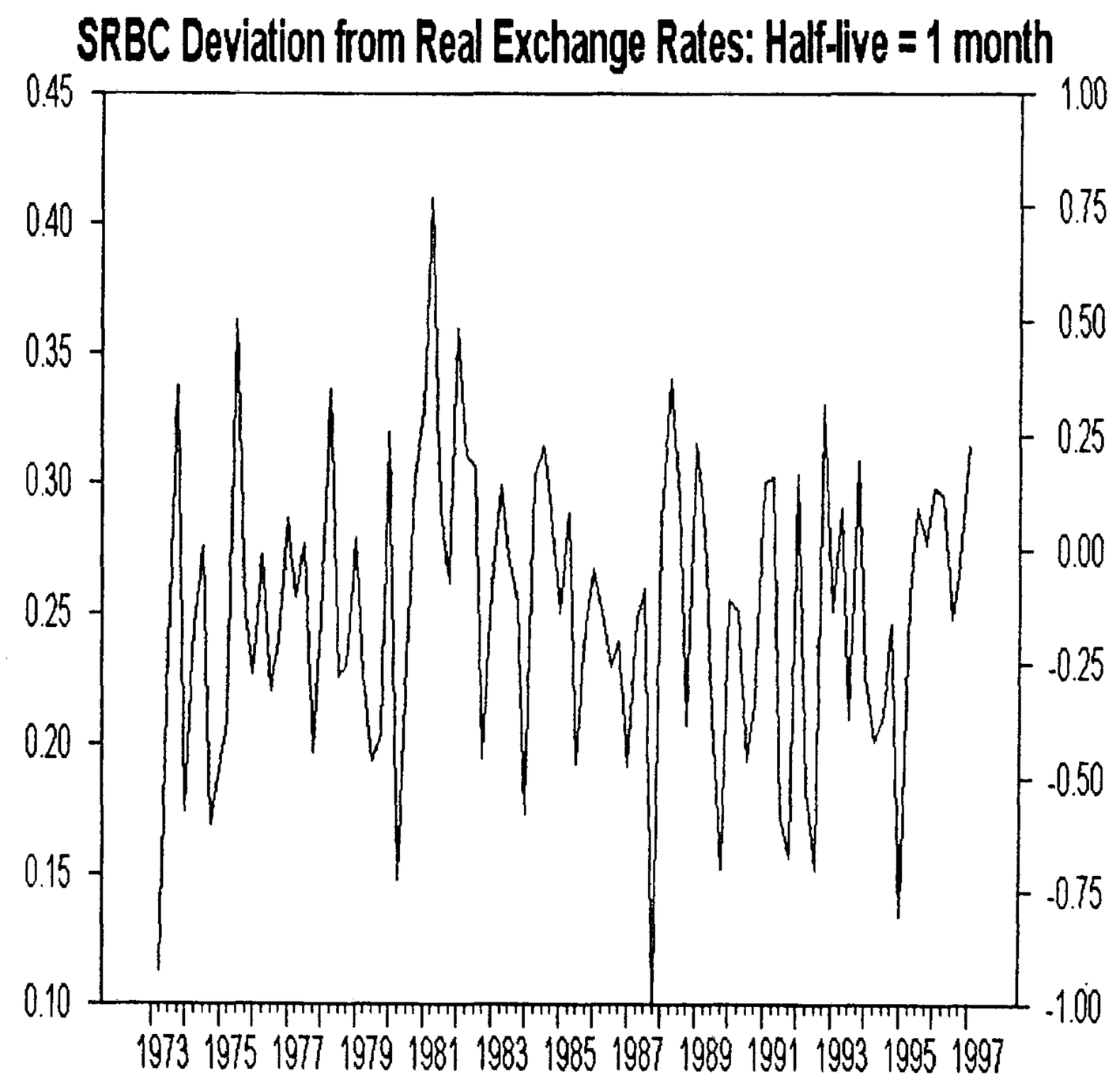
Switzerland



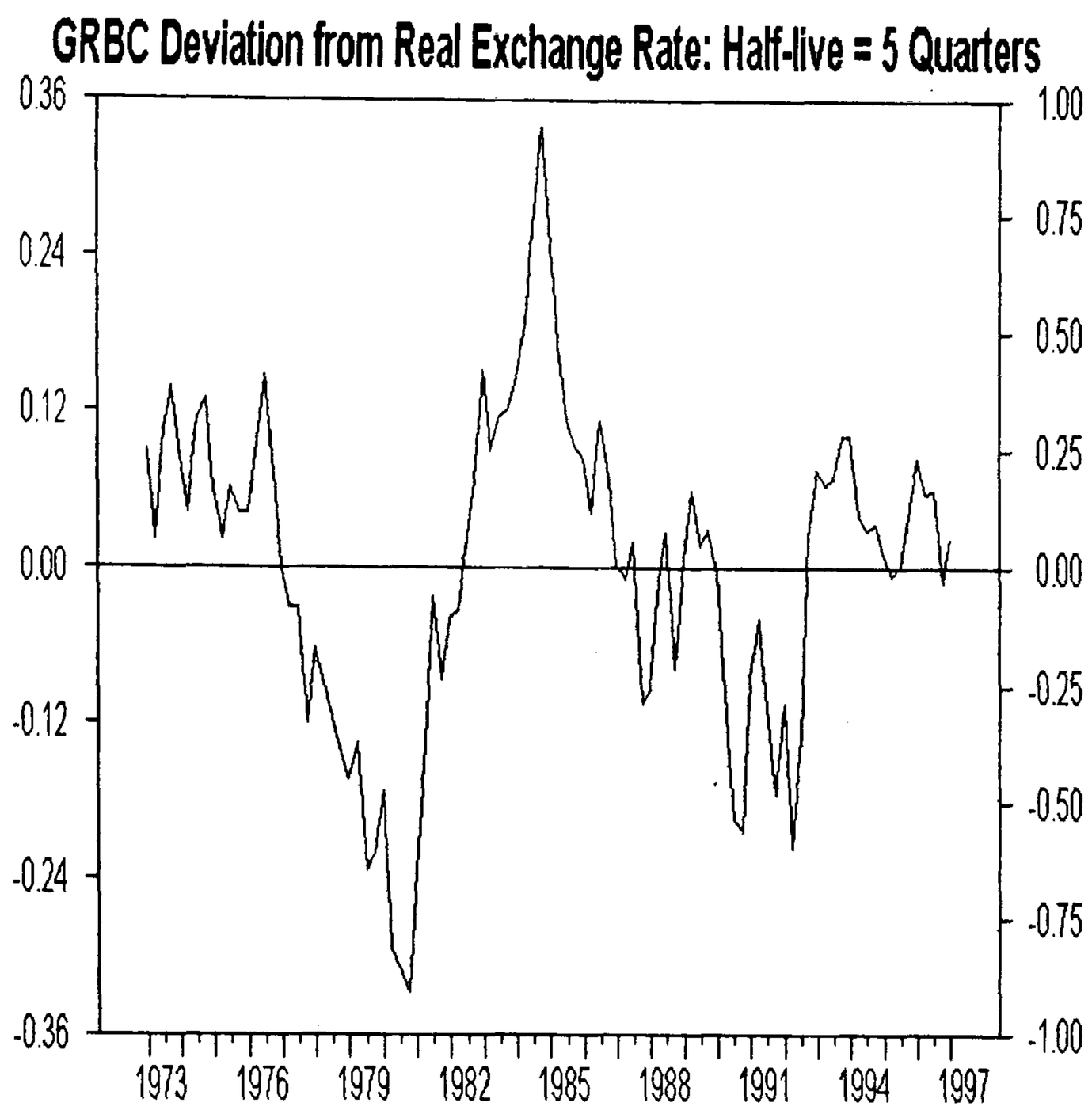


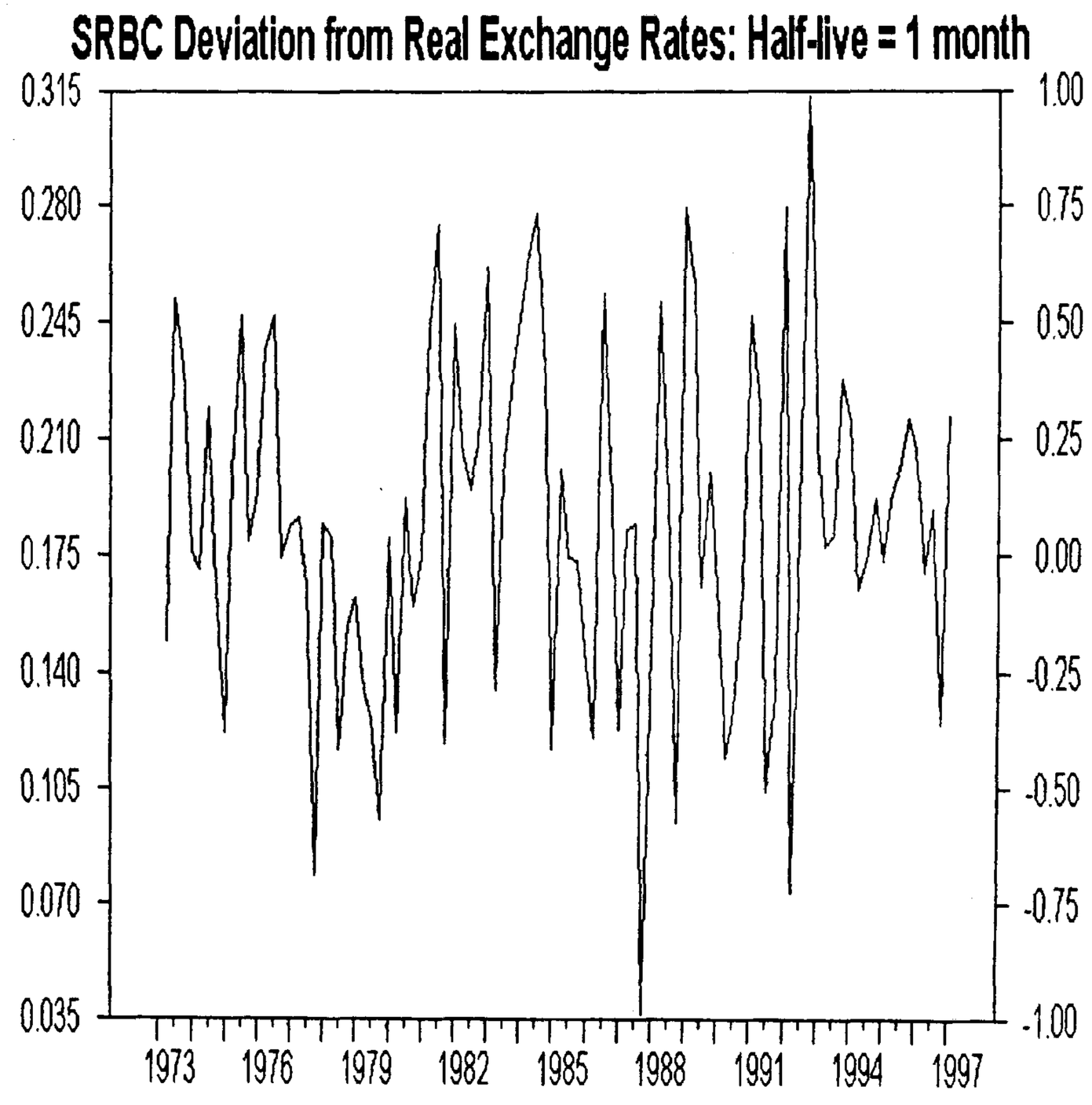
Germany


GRBC Deviation from Real Exchange Rate: Half-live = 2.5 Quarters
0.32
0.24
0.16
0.08
0.00
-0.08
-0.16
-0.24
-0.32
1.00
0.75
0.50
0.25
0.00
-0.25
-0.50
-0.75
-1.00
1973
1976
1979
1982
1985
1988
1991
1994
1997

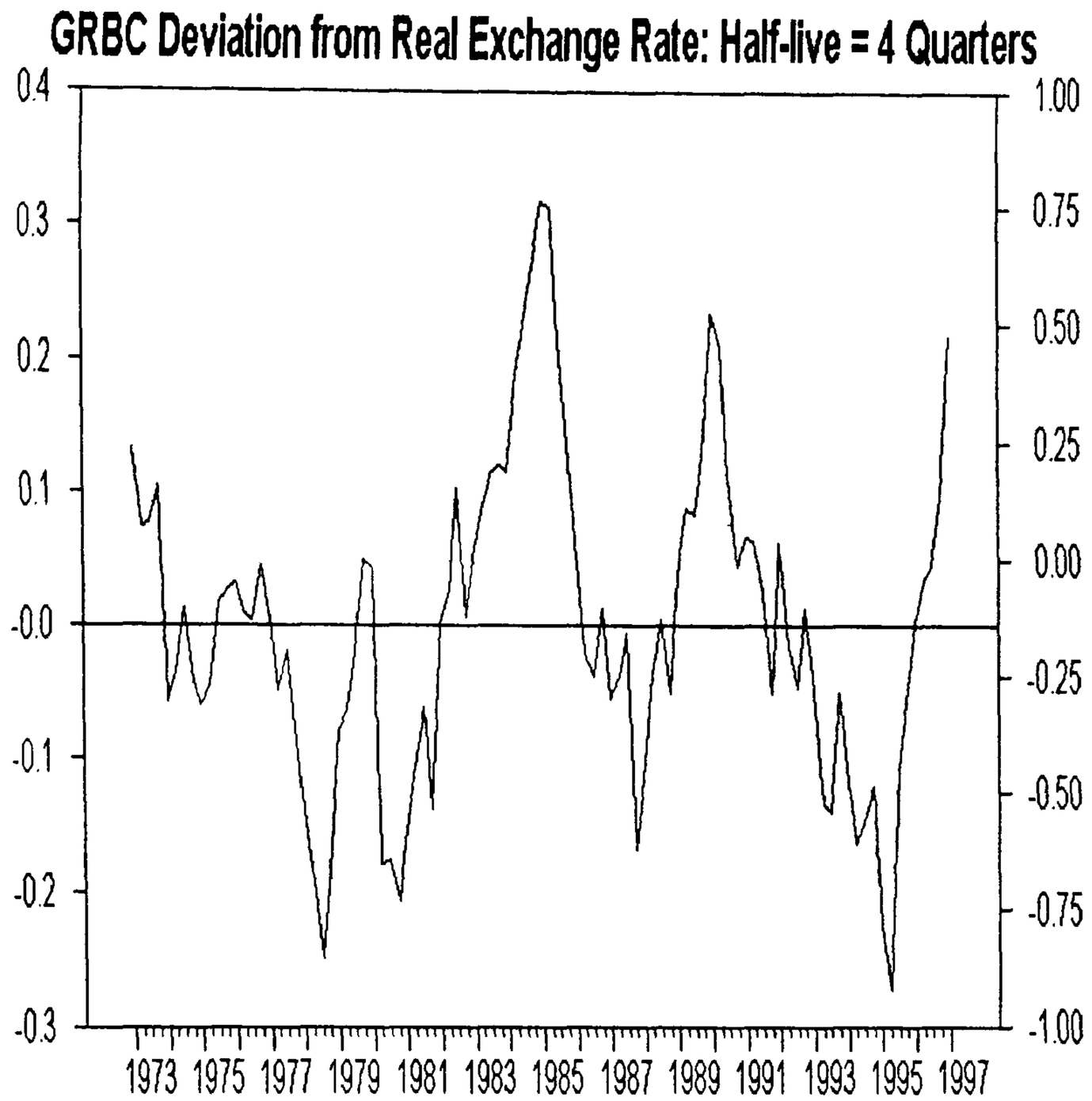

SRBC Deviation from Real Exchange Rates: Half-live = 1 month
0.45
0.40
0.35
0.30
0.25
0.20
0.15
0.10
1.00
0.75
0.50
0.25
0.00
-0.25
-0.50
-0.75
-1.00
1973
1975
1977
1979
1981
1983
1985
1987
1989
1991
1993
1995
1997

UK


GRBC Deviation from Real Exchange Rate: Half-live = 5 Quarters
0.36
0.24
0.12
0.00
-0.12
-0.24
-0.36
1.00
0.75
0.50
0.25
0.00
-0.25
-0.50
-0.75
-1.00
1973
1976
1979
1982
1985
1988
1991
1994
1997


SRBC Deviation from Real Exchange Rates: Half-live = 1 month
0.315
0.280
0.245
0.210
0.175
0.140
0.105
0.070
0.035
1.00
0.75
0.50
0.25
0.00
-0.25
-0.50
-0.75
-1.00
1973
1976
1979
1982
1985
1988
1991
1994
1997

Japan


GRBC Deviation from Real Exchange Rate: Half-live = 4 Quarters
0.4
0.3
0.2
0.1
-0.0
-0.1
-0.2
-0.3
1.00
0.75
0.50
0.25
0.00
-0.25
-0.50
-0.75
-1.00
1973 1975 1977 1979 1981 1983 1985 1987 1989 1991 1993 1995 1997

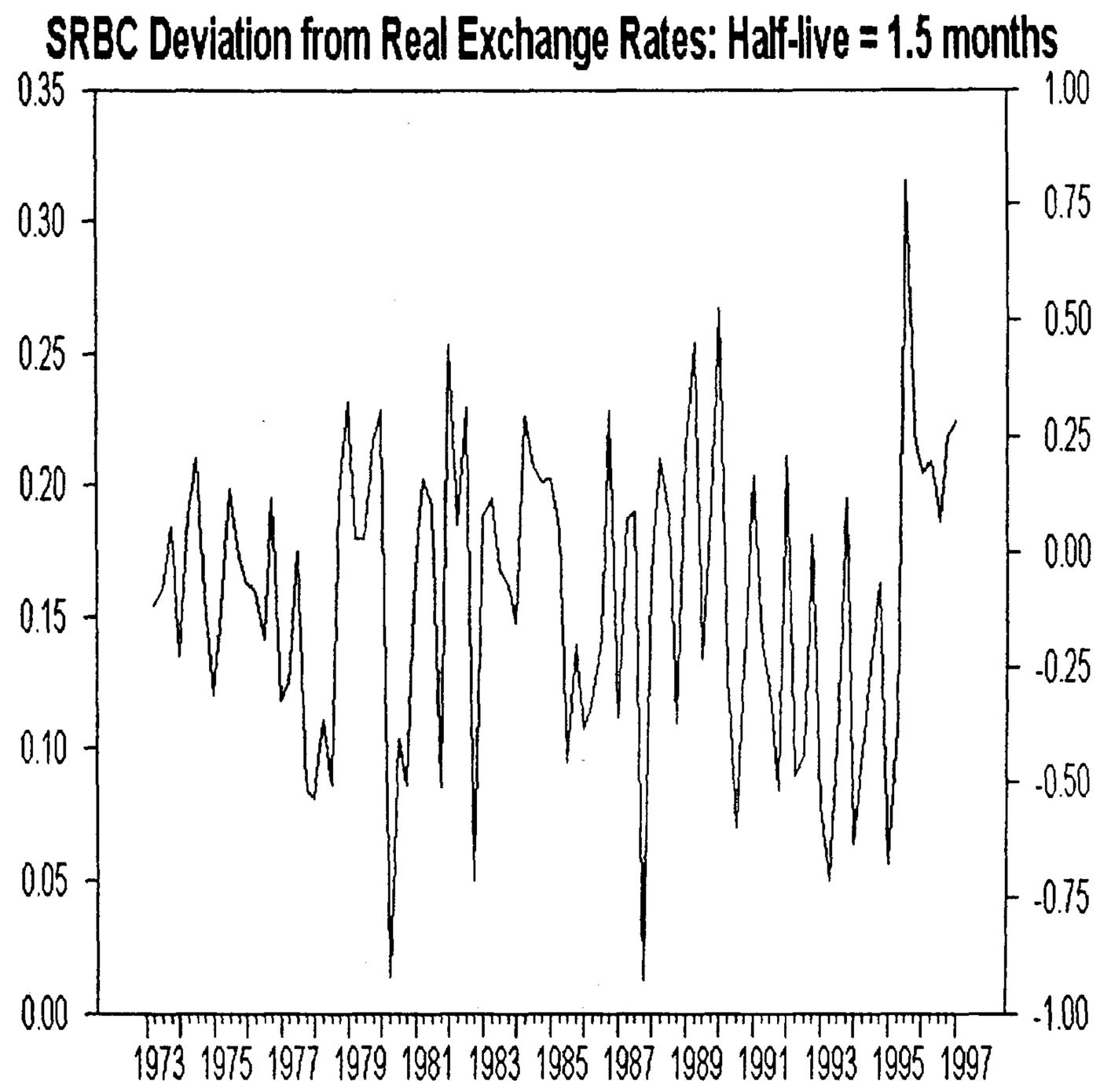
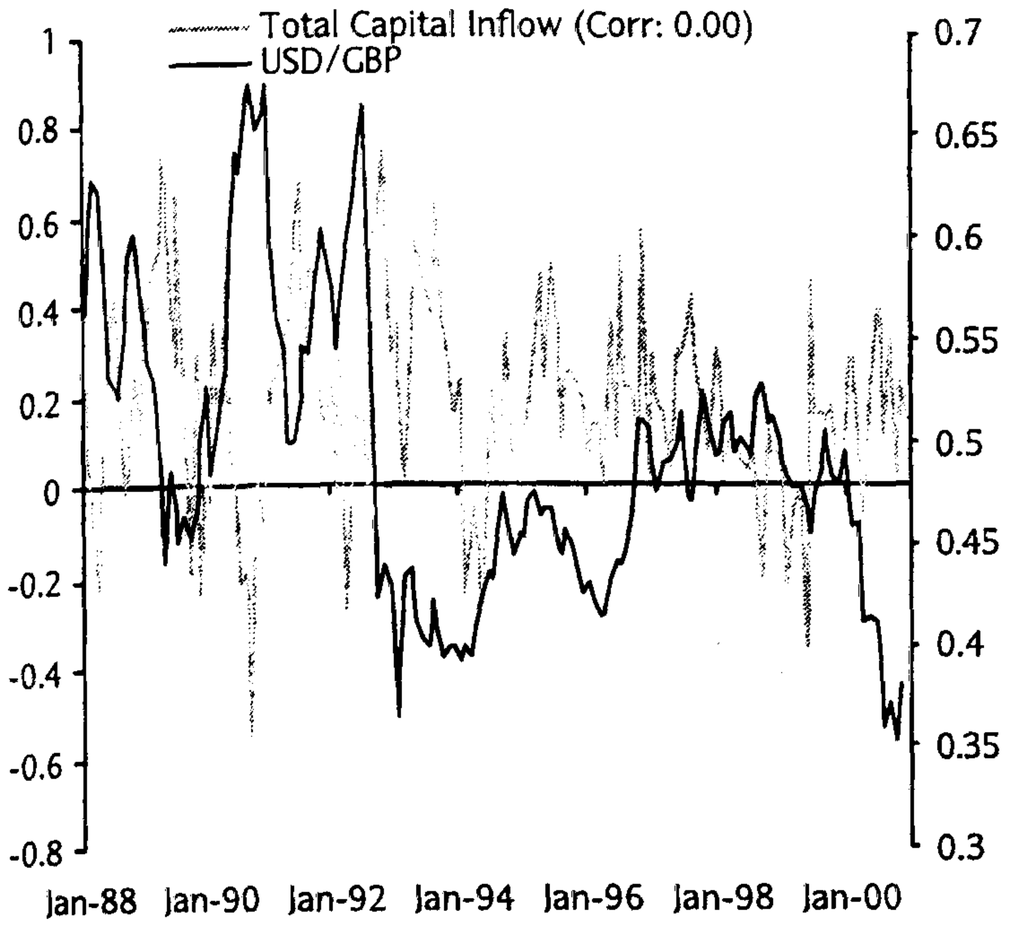
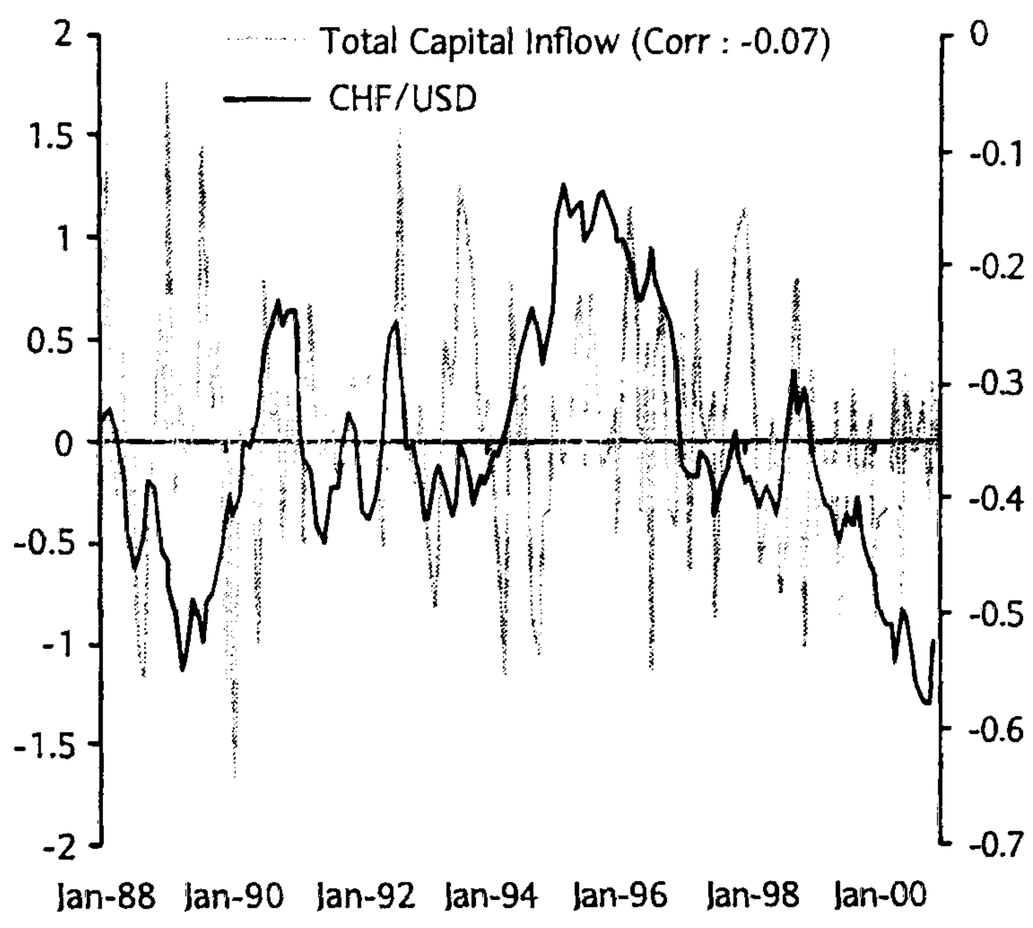
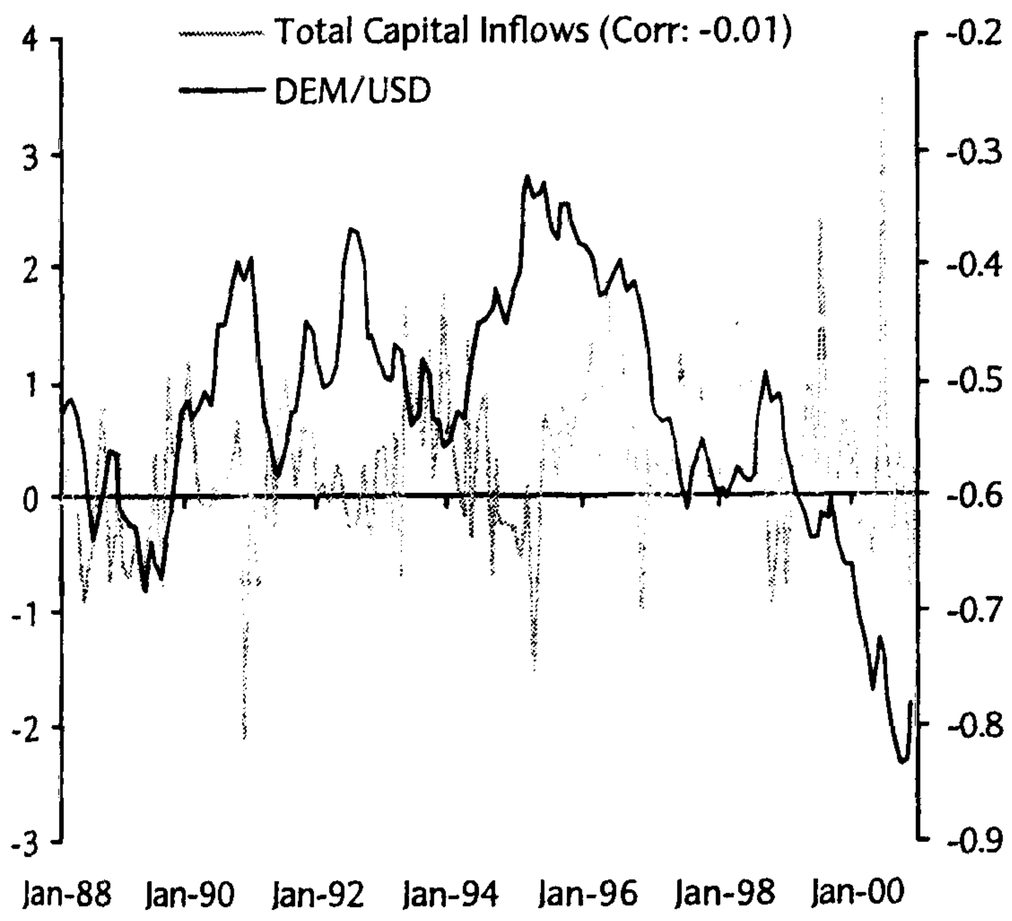

SRBC Deviation from Real Exchange Rates: Half-live = 1.5 months
0.35
0.30
0.25
0.20
0.15
0.10
0.05
0.00
1.00
0.75
0.50
0.25
0.00
-0.25
-0.50
-0.75
-1.00
1973 1975 1977 1979 1981 1983 1985 1987 1989 1991 1993 1995 1997

Figure 2.1: Exchange Rates (in logs) and Net Total Capital inflow (Equities + Bonds) in the U.S. (unconditional correlations in parenthesis)







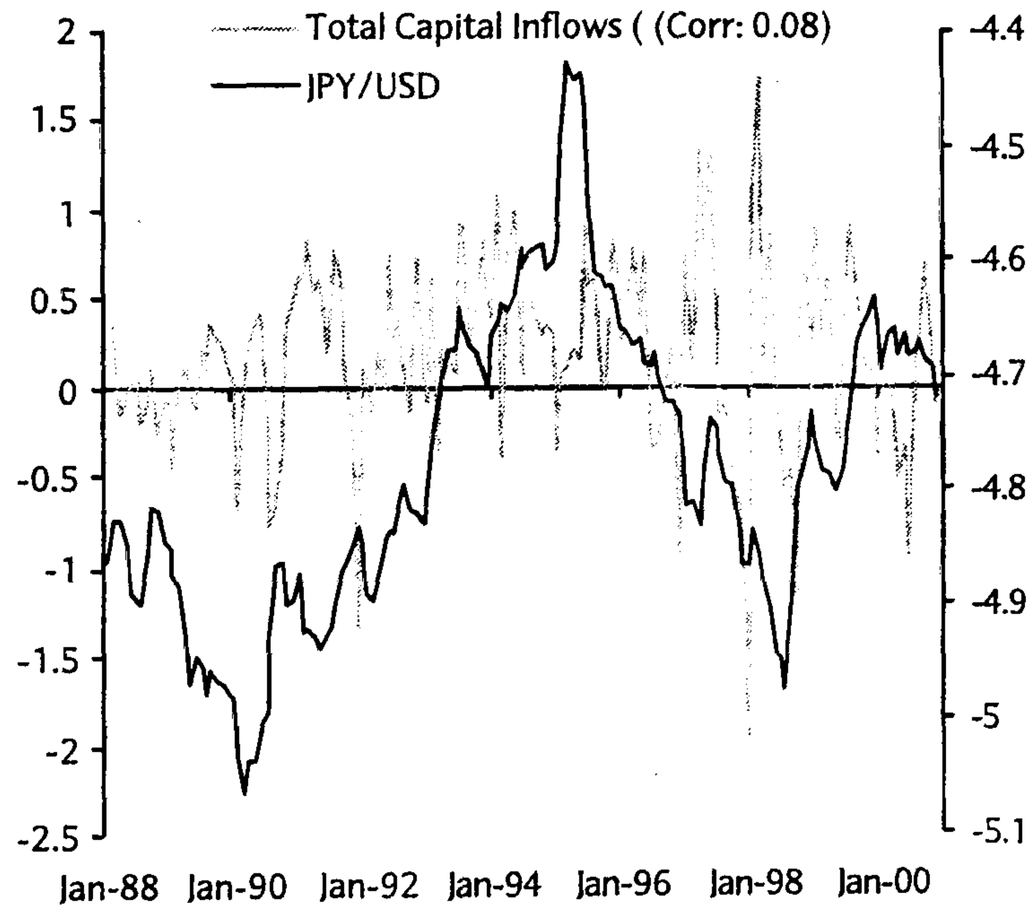
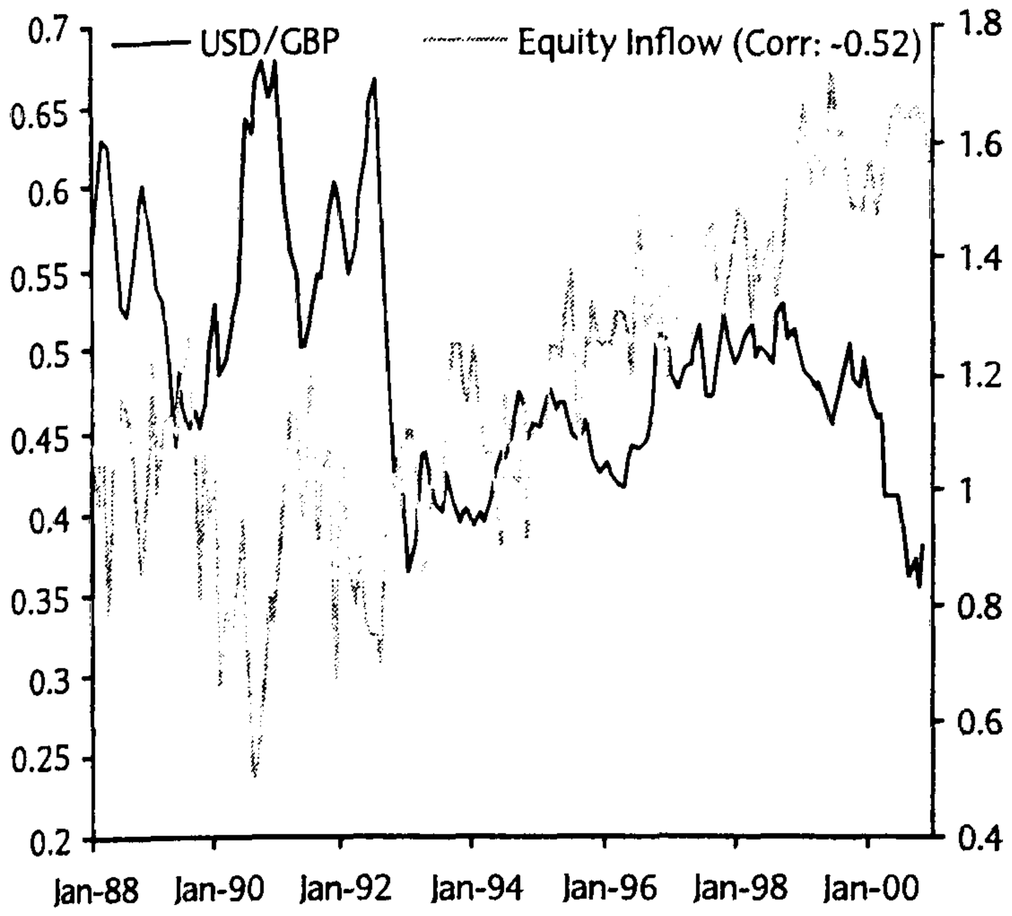
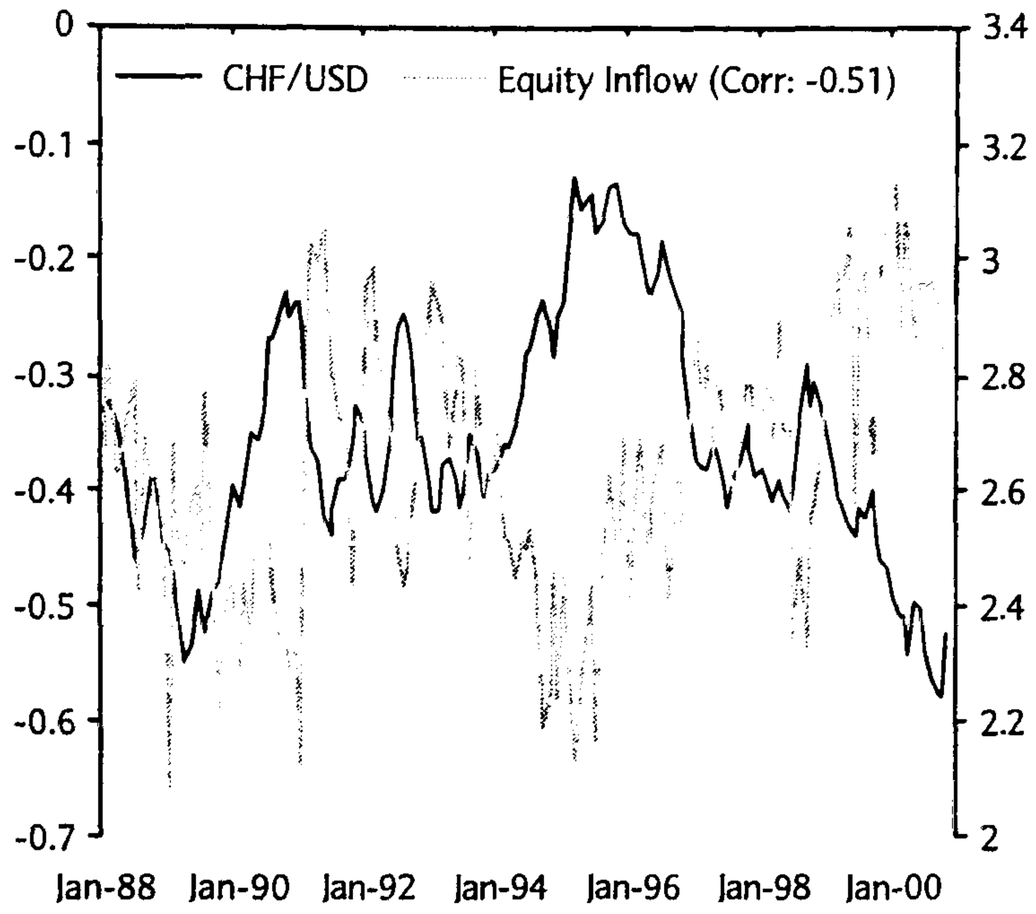
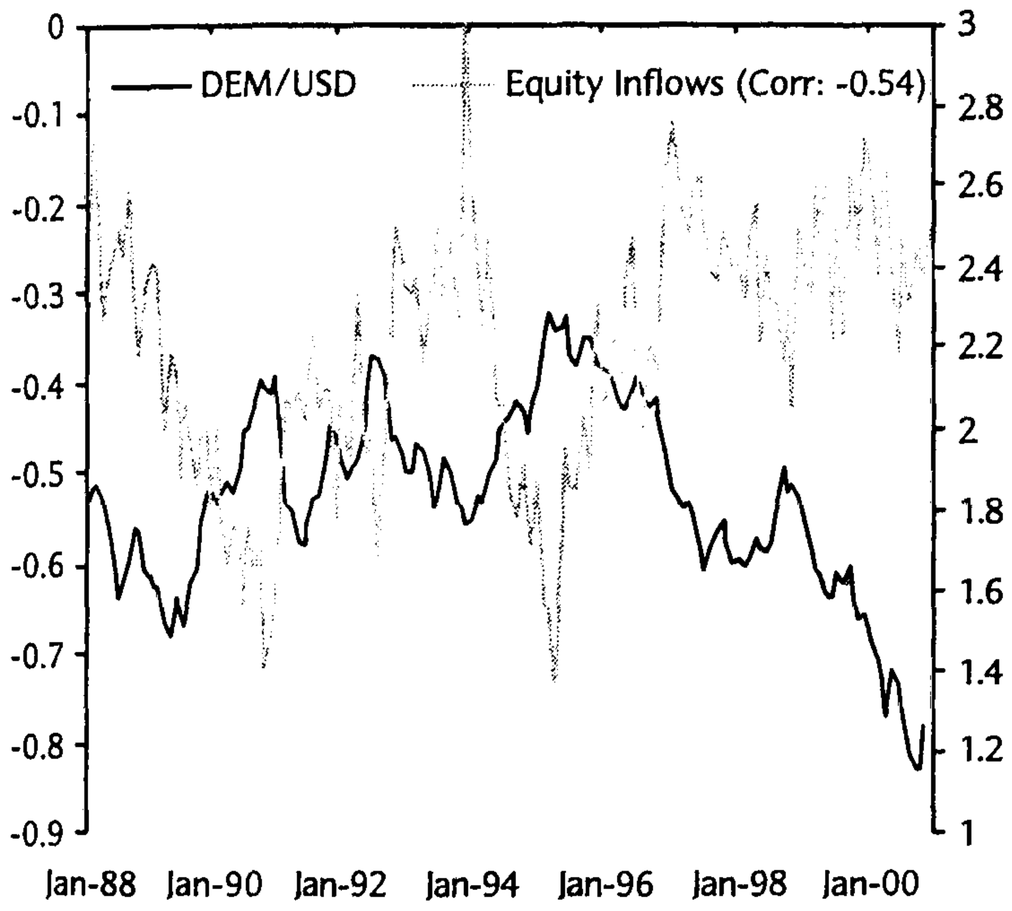


Figure 2.2: Exchange Rates (in logs) and Net Equity inflow in the U.S. (unconditional correlations in parenthesis)







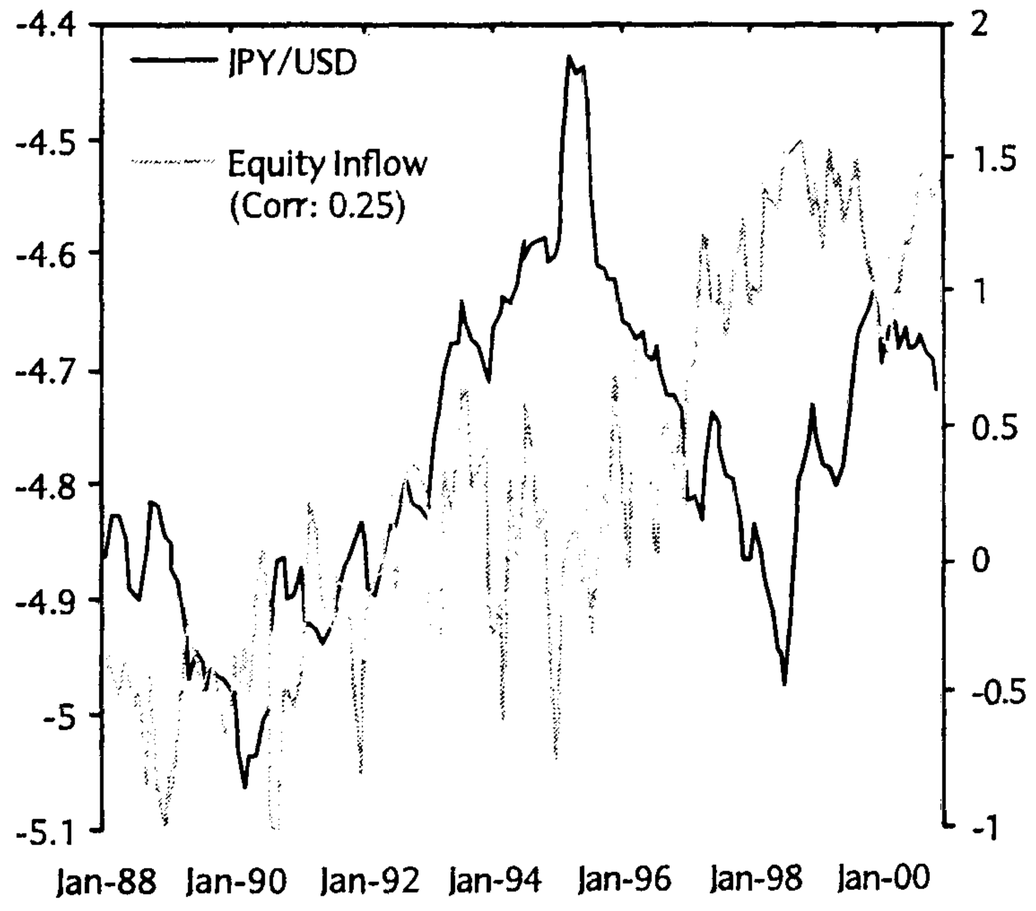
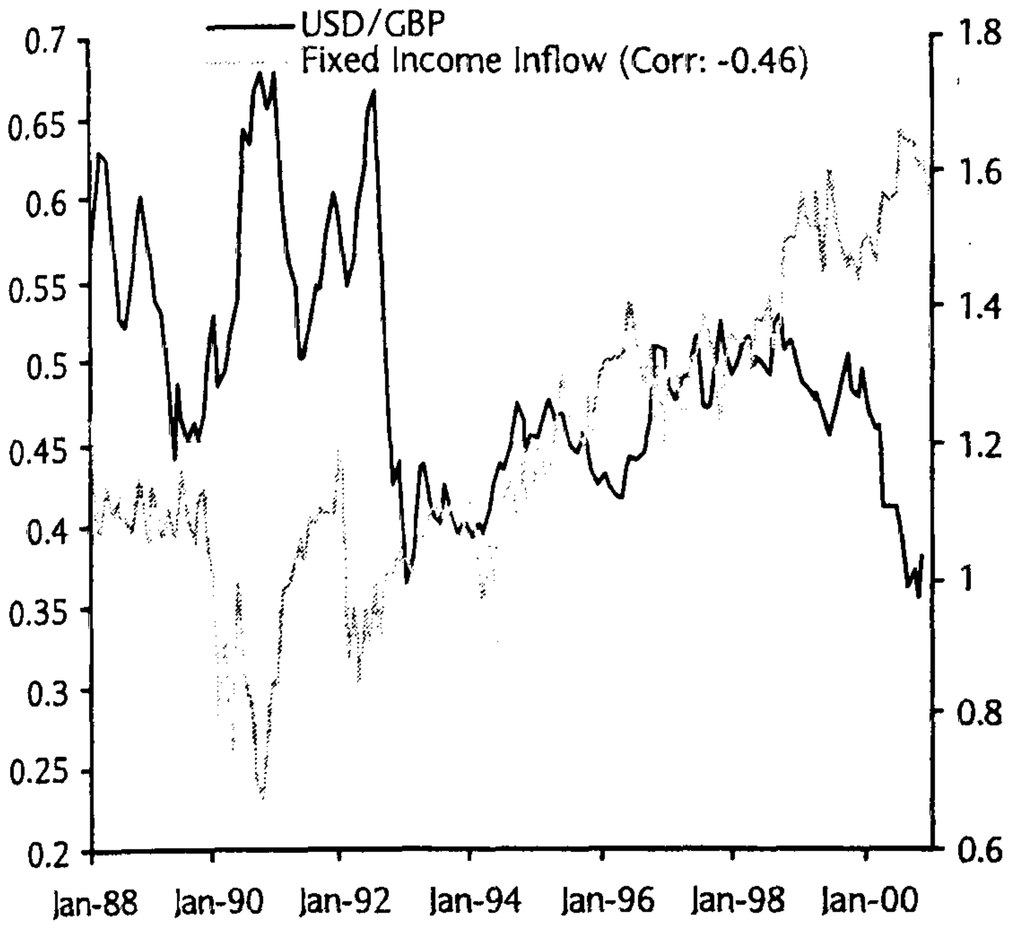
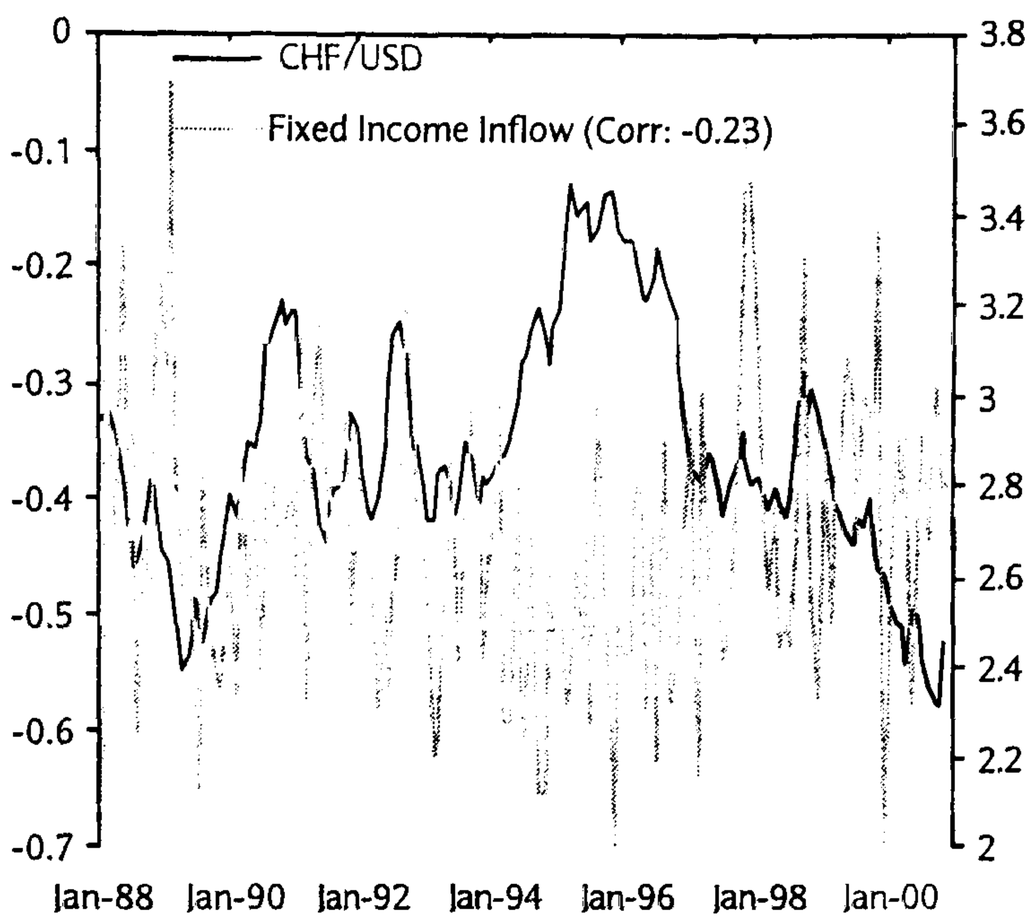
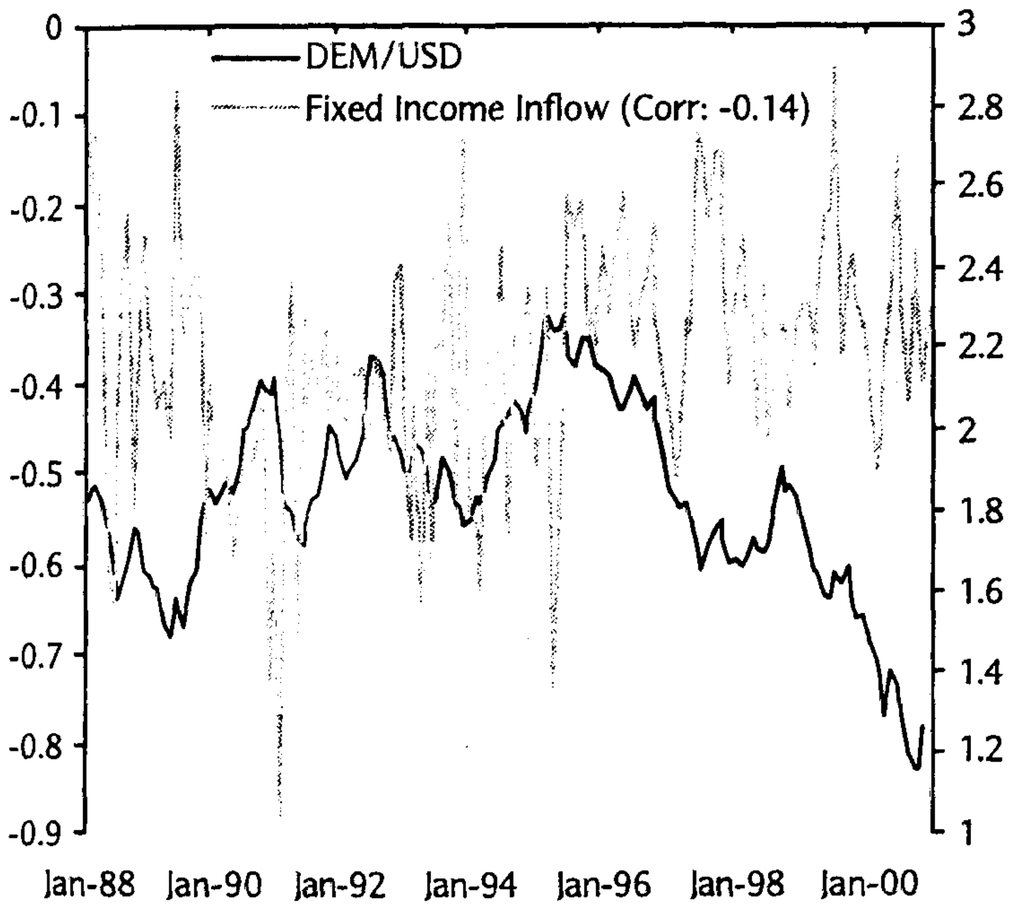


Figure 2.3: Exchange Rates (in logs) and Net Fixed Income inflow in the U.S. (unconditional correlations in parenthesis)







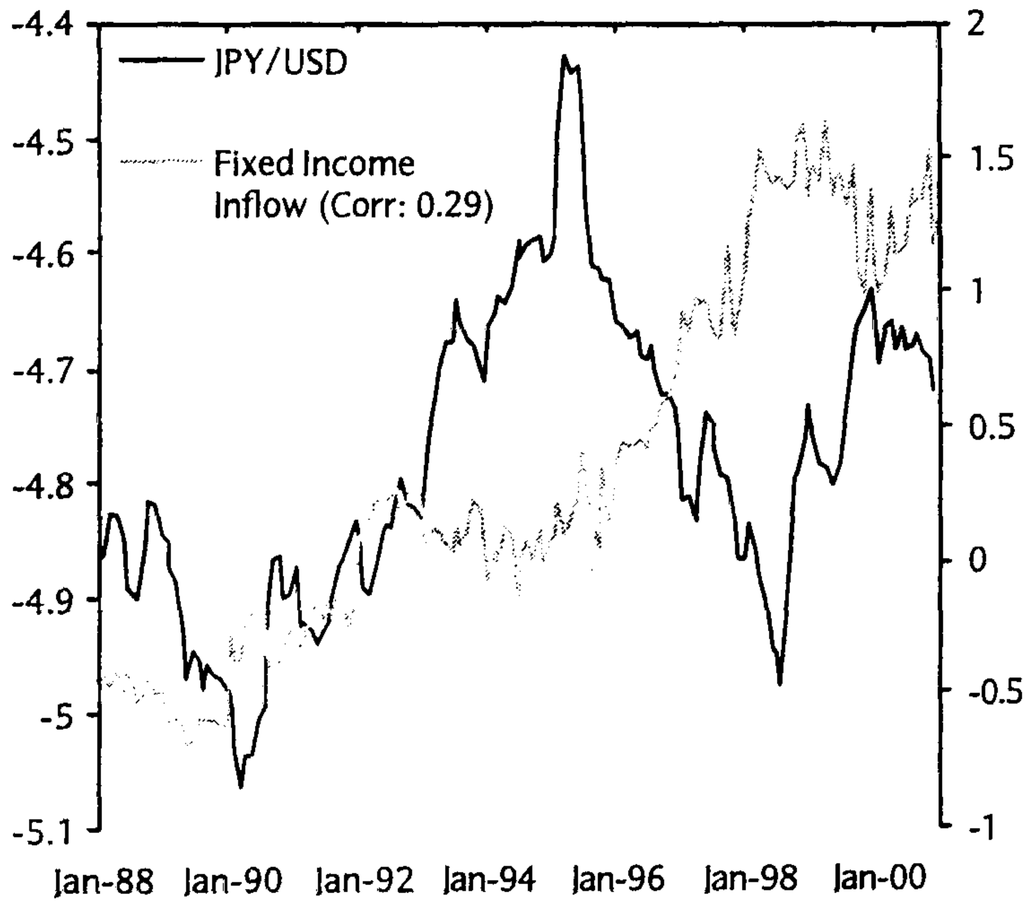


Figure 2.4: Schematic representation of the likelihood ratio tests to identify the capital flow that correlates the strongest with exchange rates.

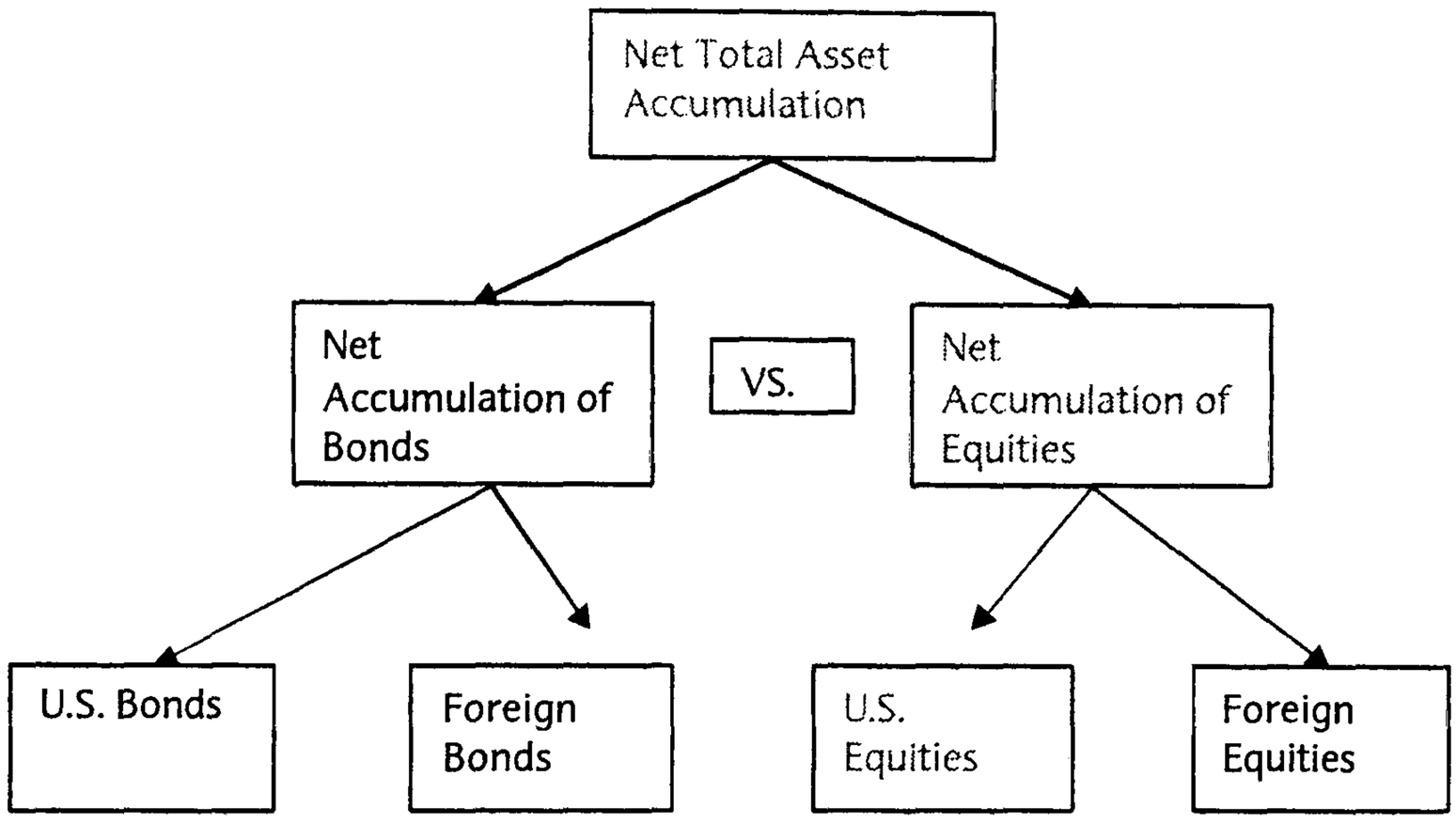
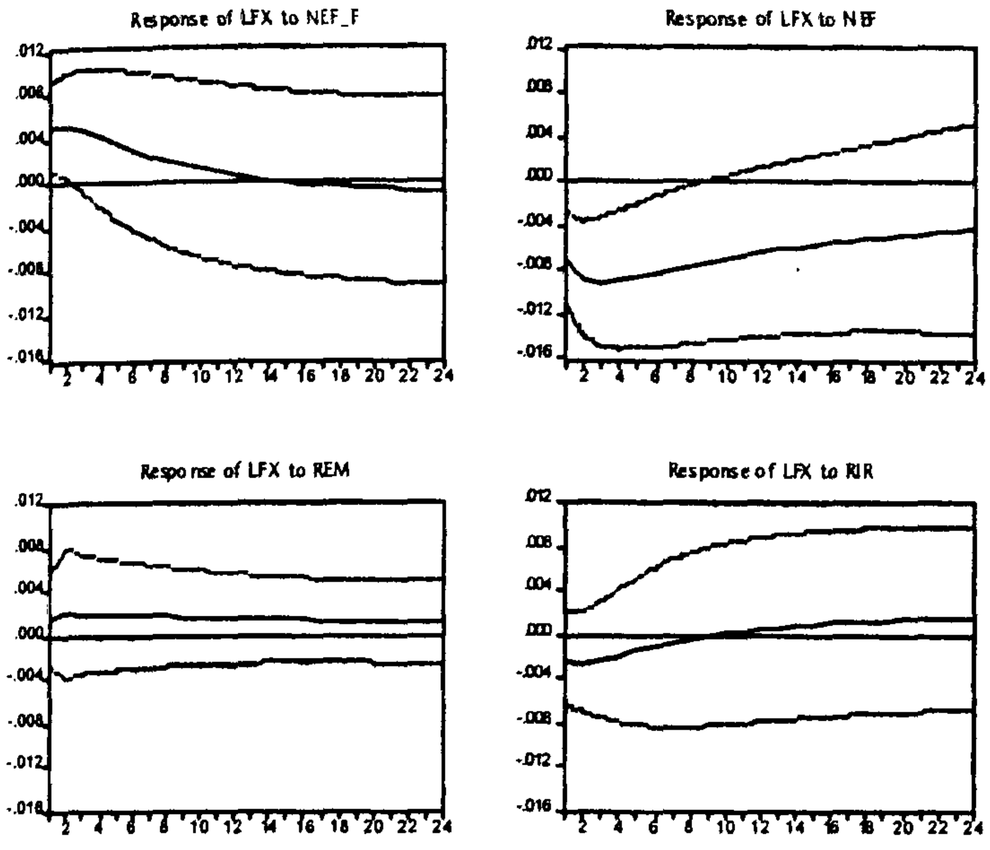
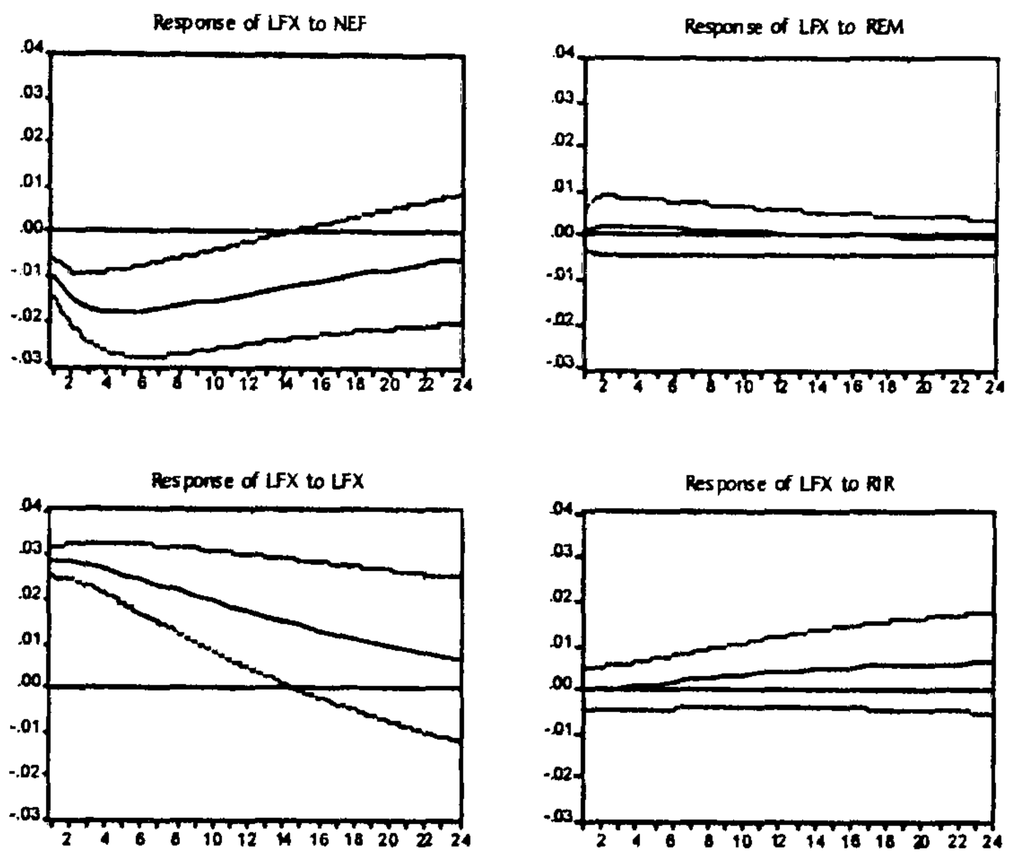


Figure 2.5: Generalized Impulse Responses of exchange rates (LFX) to one standard deviations shocks.

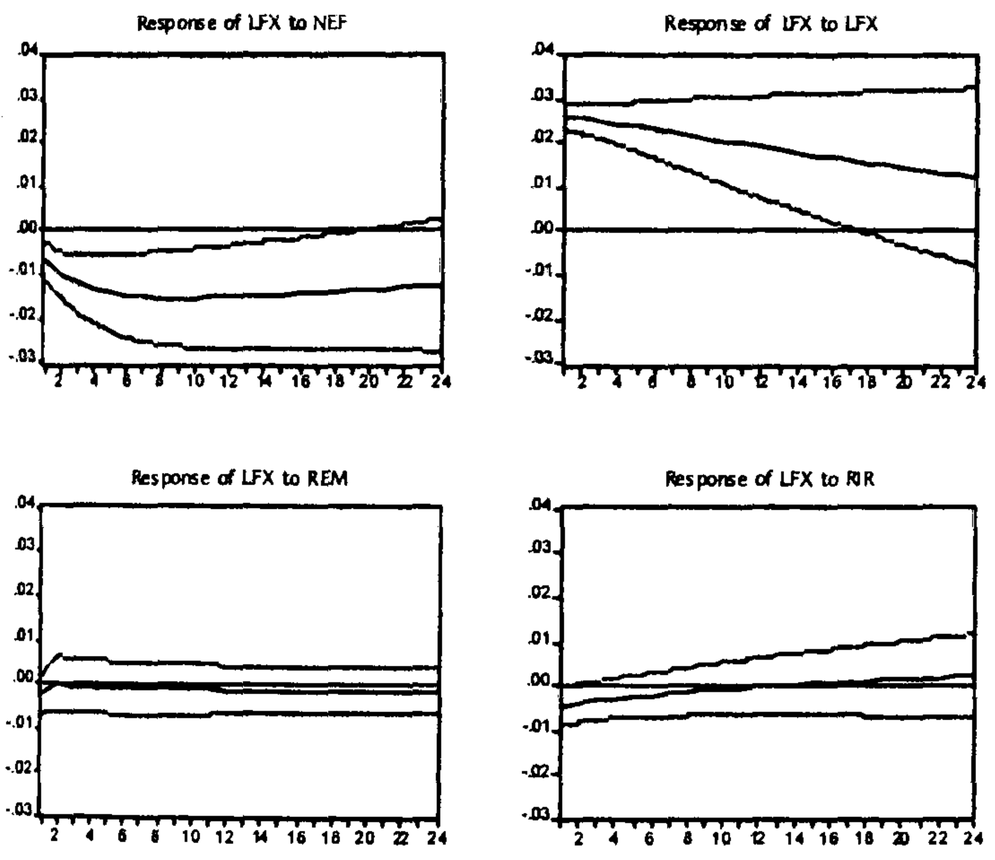
VAR System for U.K.



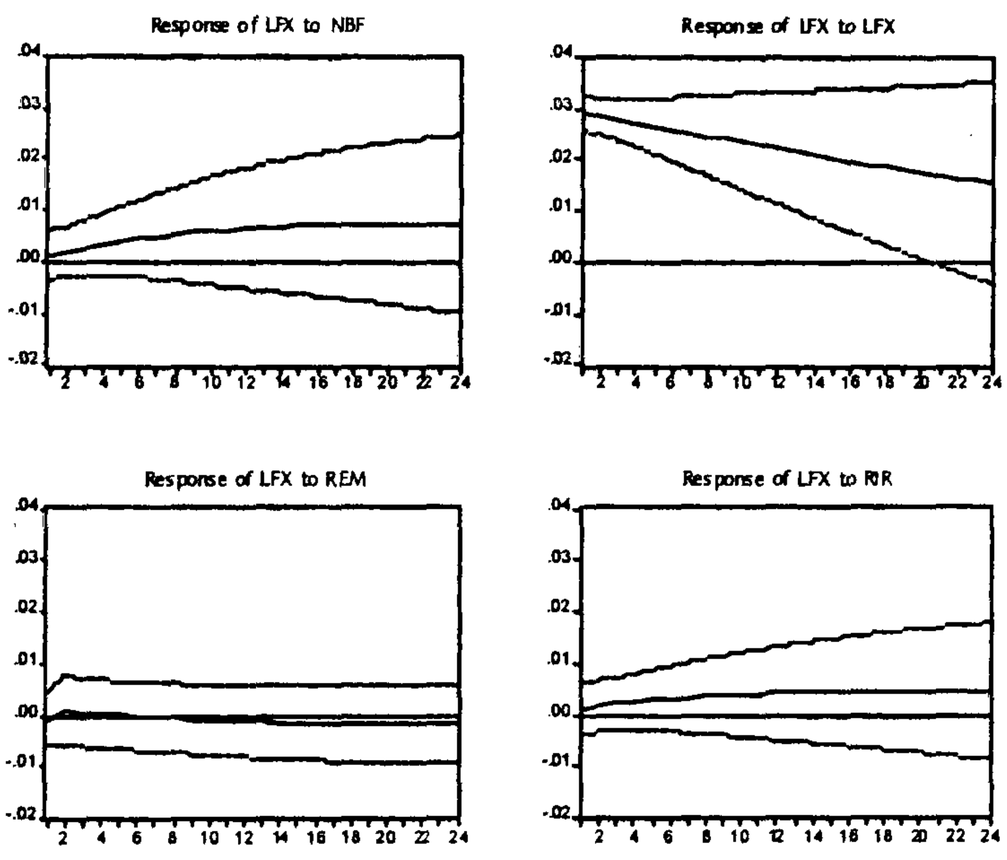
VAR System for Switzerland



VAR Sustem for Germany

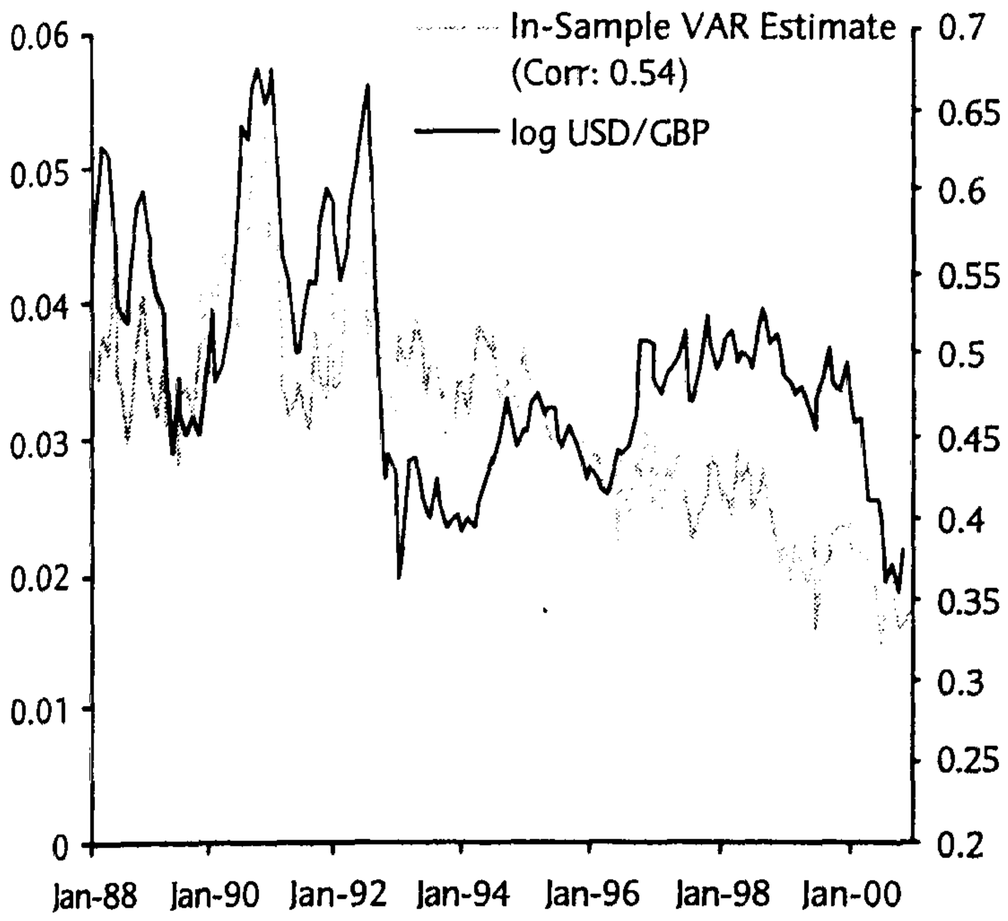


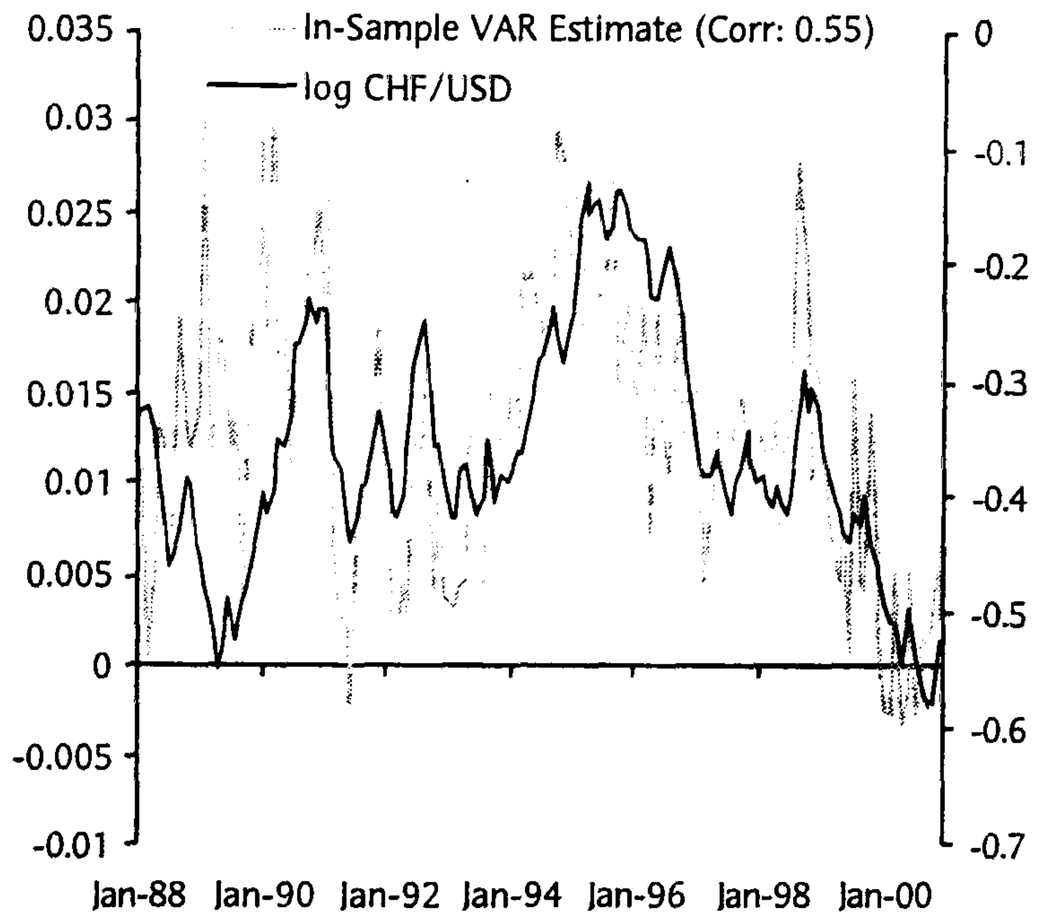
VAR System for Japan

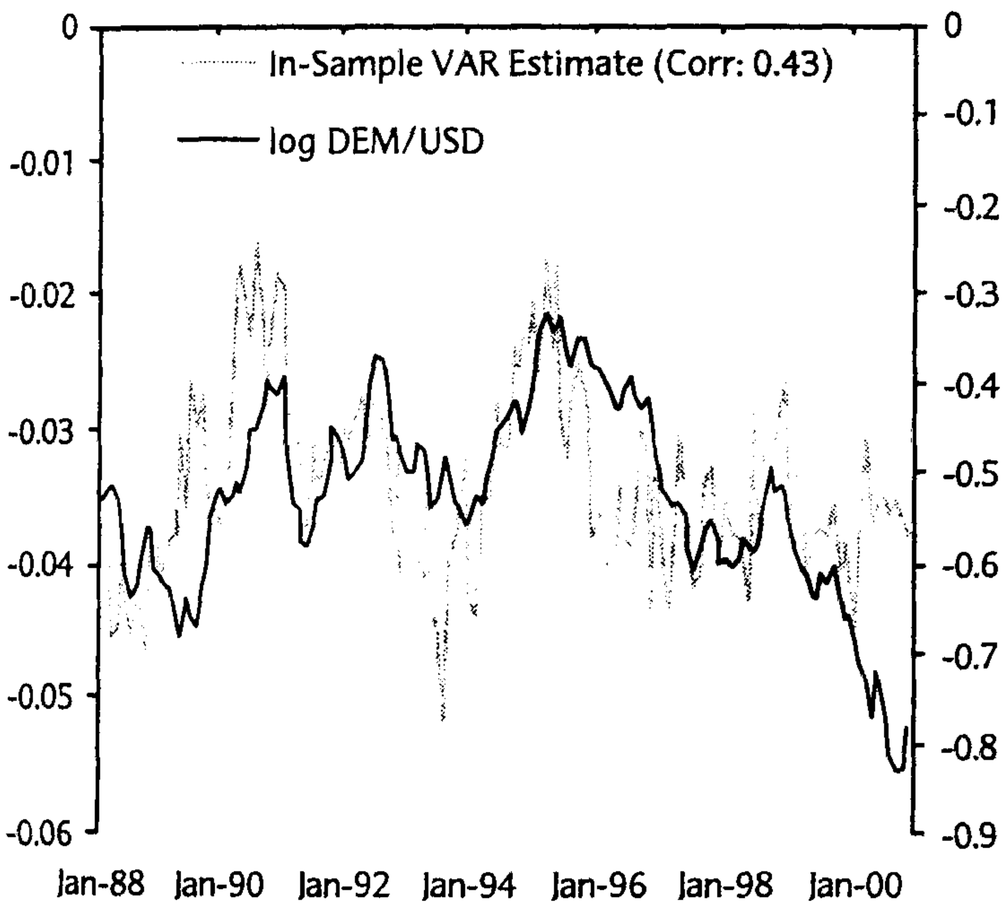


Note: Generalized Impulses as described by Pesaran and Shin (1998). Depending on the VAR system, the variables used are: log foreign exchange (LFX), net purchases of U.S. equities (NEF), net purchase of foreign equities (NEF_F), net purchases of bonds (NBF), relative equity market returns (REM) and relative interest rates (RIR). Sample period : 1998 :01 – 2000 :12. For variable definitions please see data Appendix.
Generalized Impulses as described by Pesaran and Shin (1998) construct an orthogonal set of innovations that does not depend on the VAR ordering. The generalized impulse responses from an innovation to the j-th variable are derived by applying a variable specific Cholesky factor computed with the j-th variable at the top of the Cholesky ordering.

Figure 2.6: Counterfactual analysis: In-sample prediction for FX based on equity flows.







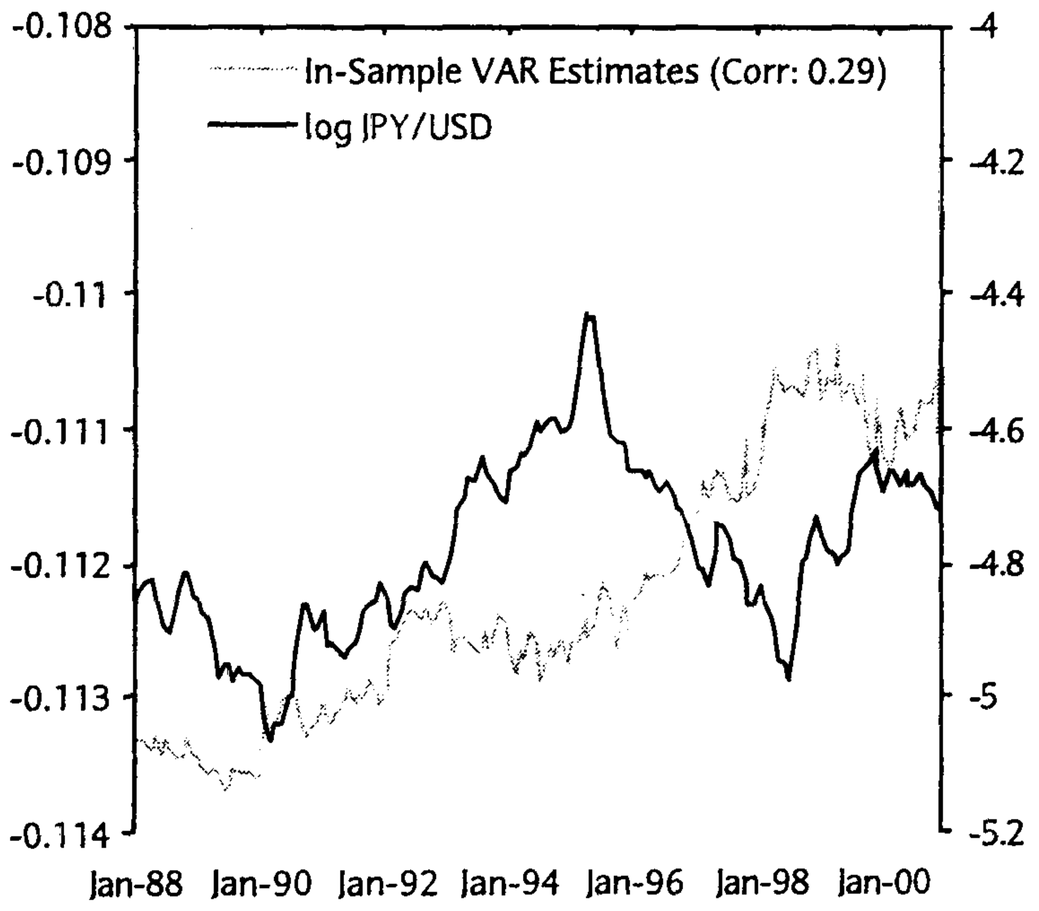
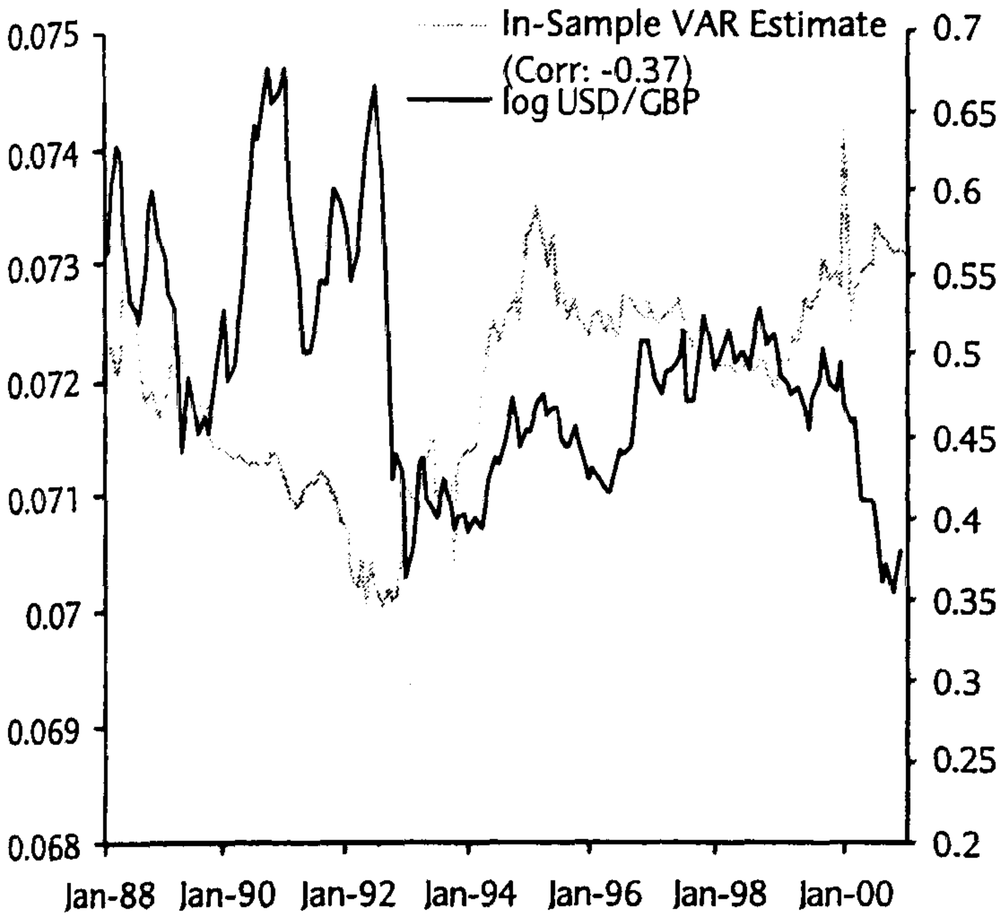
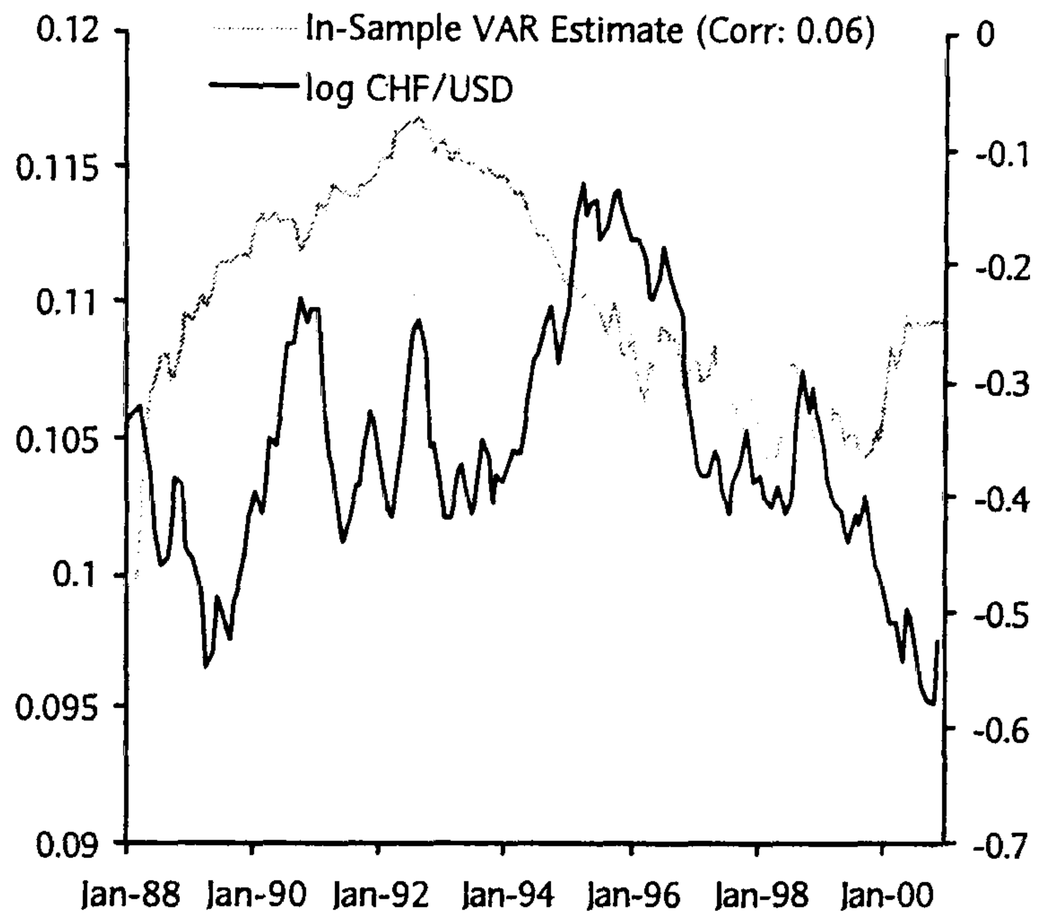
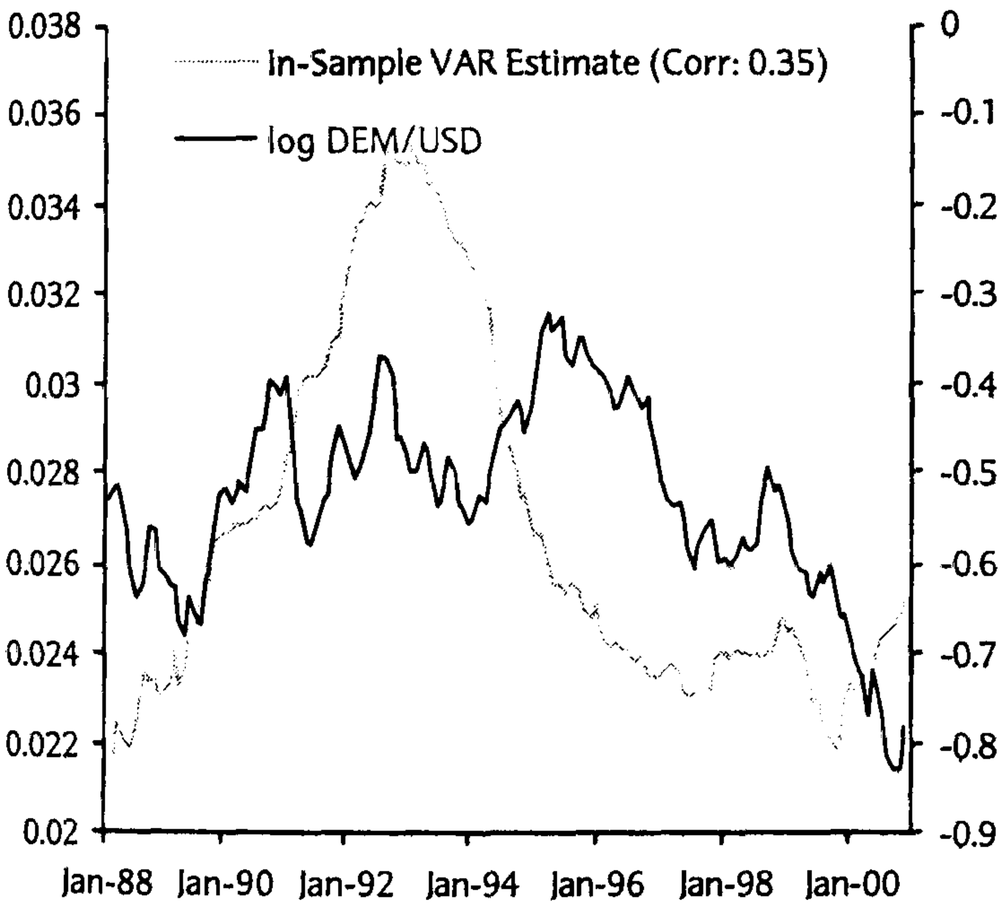


Figure 2.7: Counterfactual Analysis: In-sample prediction for FX based on interest rate differentials.







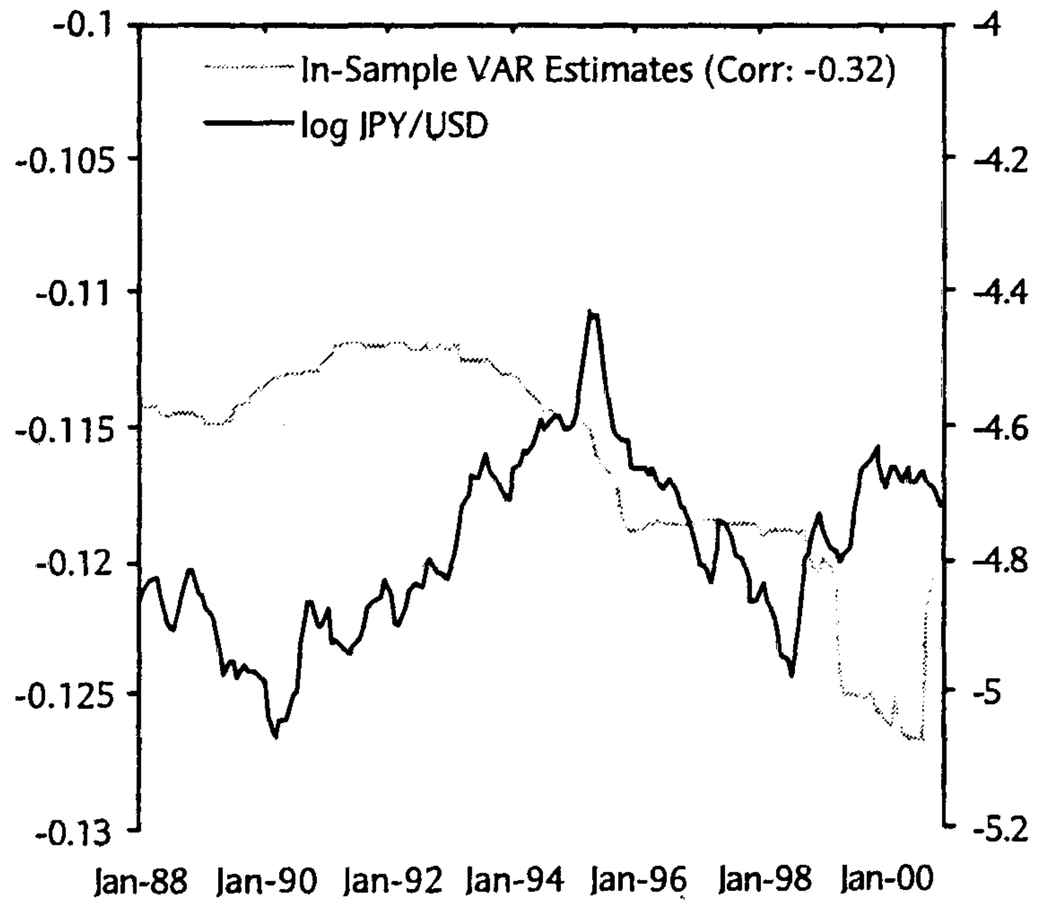
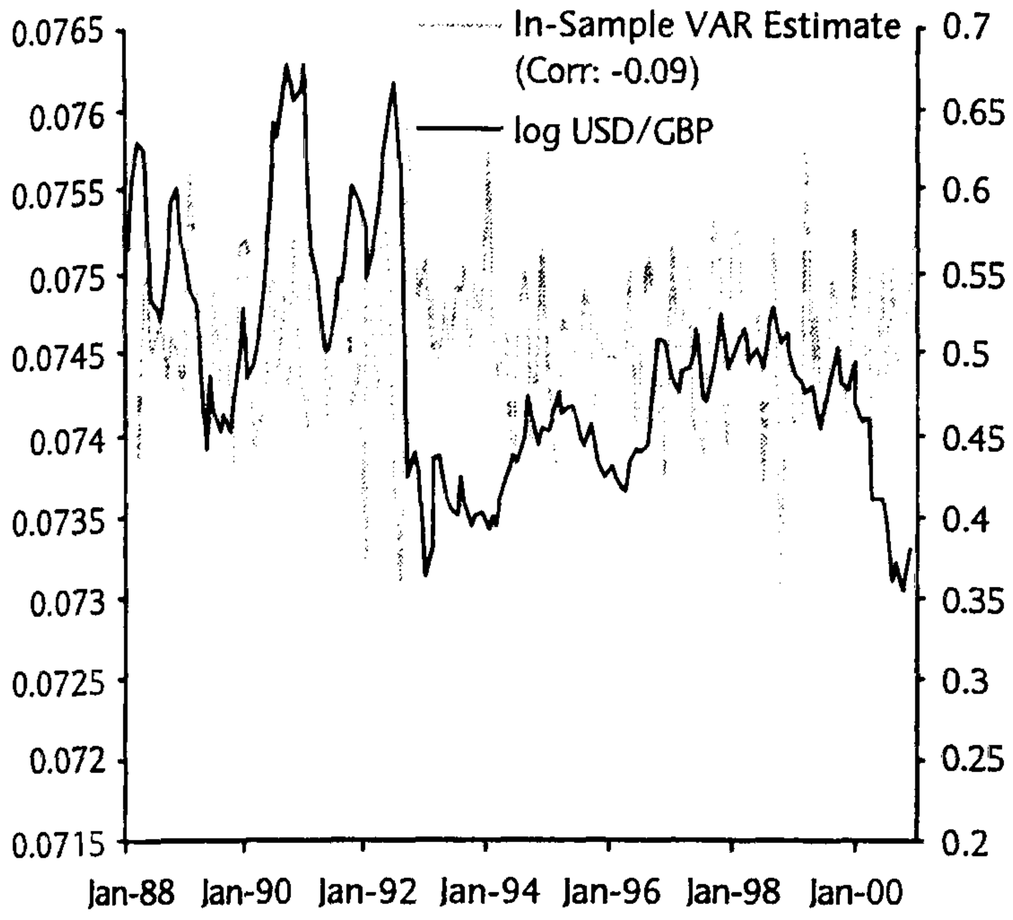
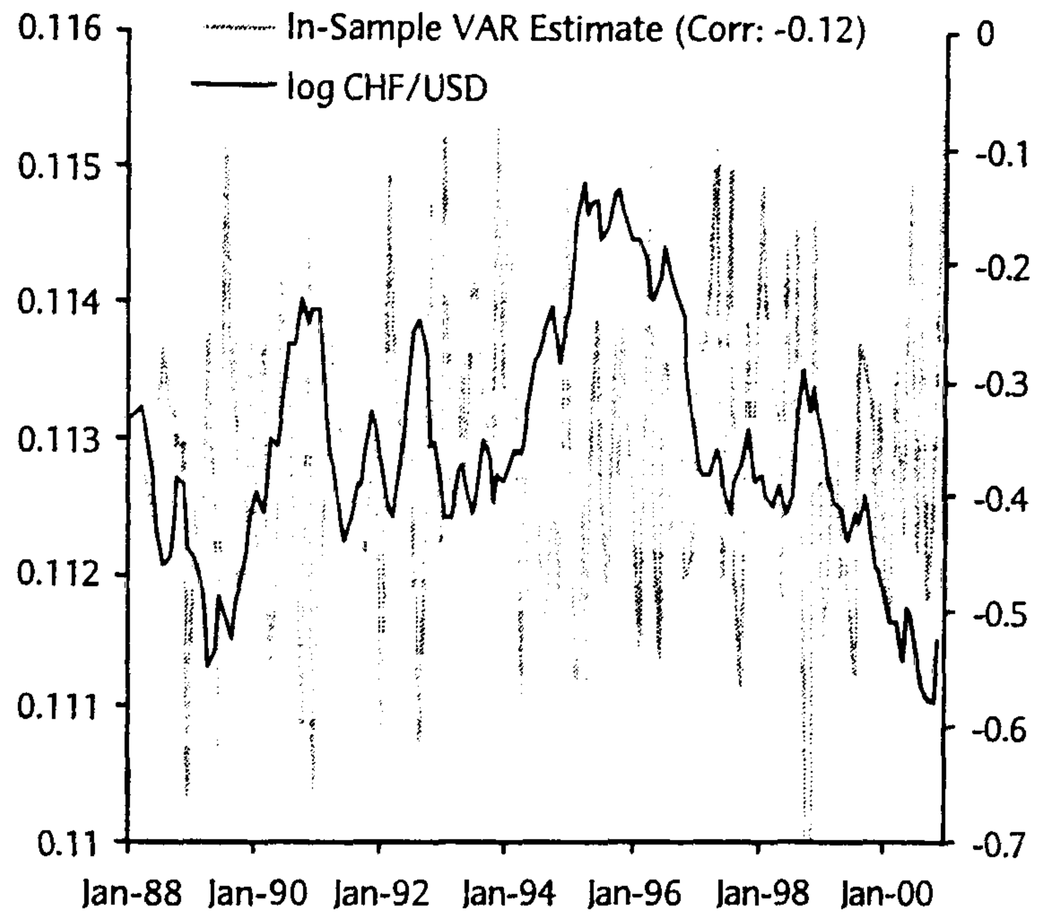
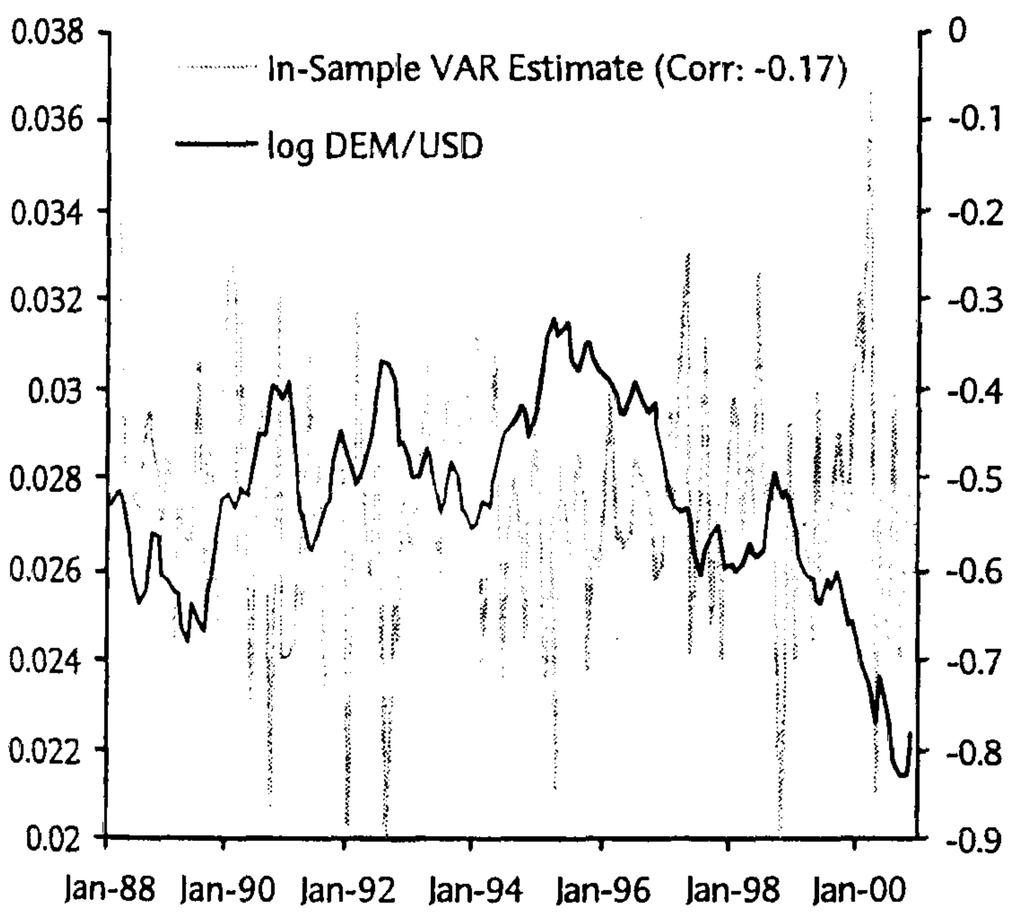


Figure 2.8: Counterfactual Analysis: In-Sample prediction for FX based on relative equity markets performance.







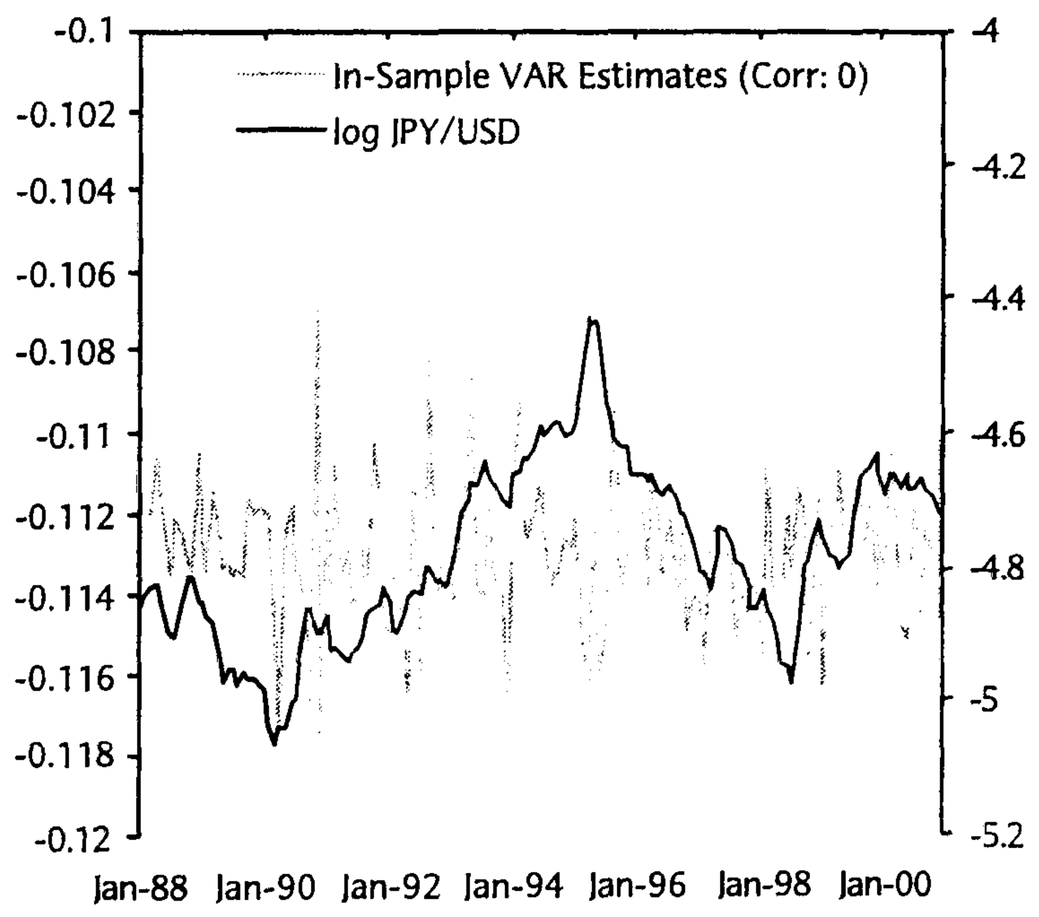
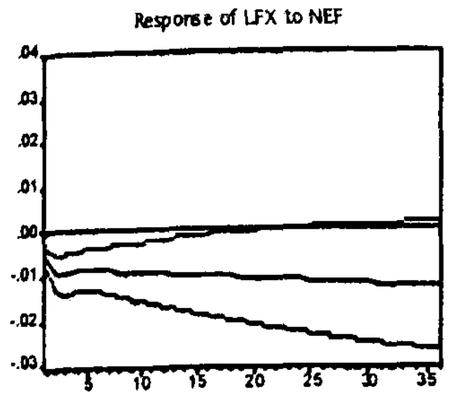
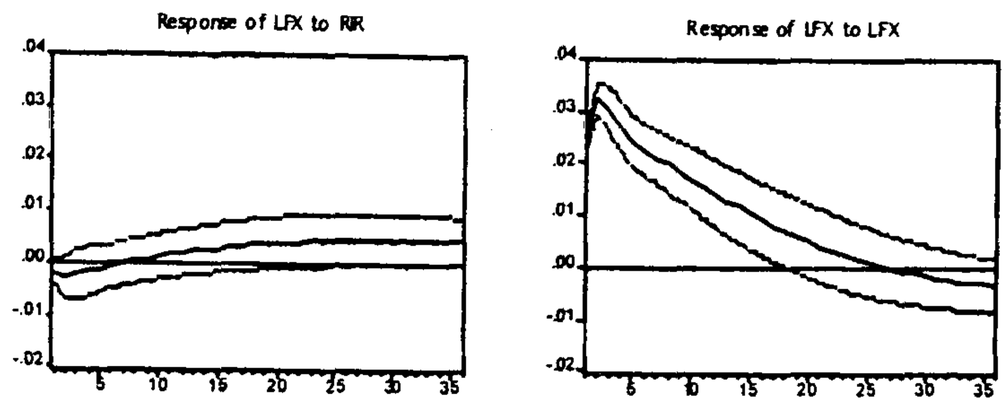


Figure 2.9: Generalized Impulse Responses pooled data

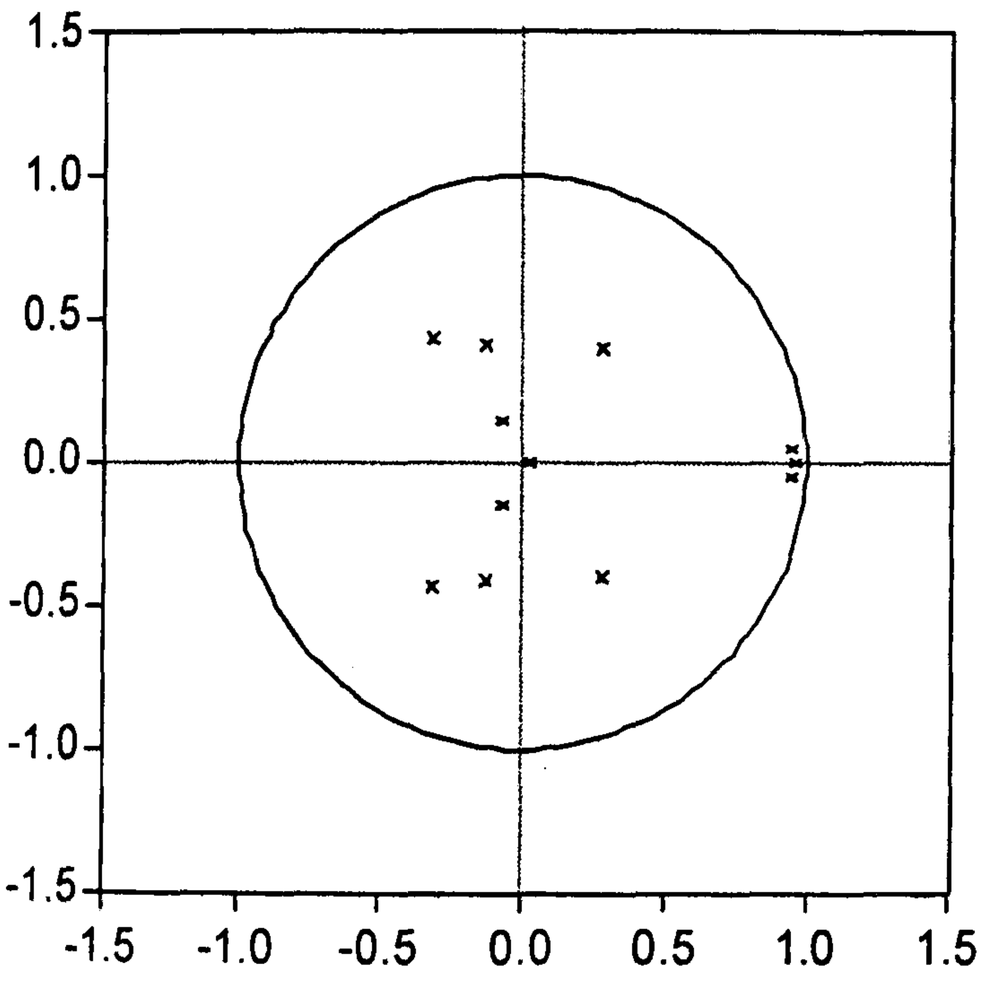
VAR(3) System for pooled data



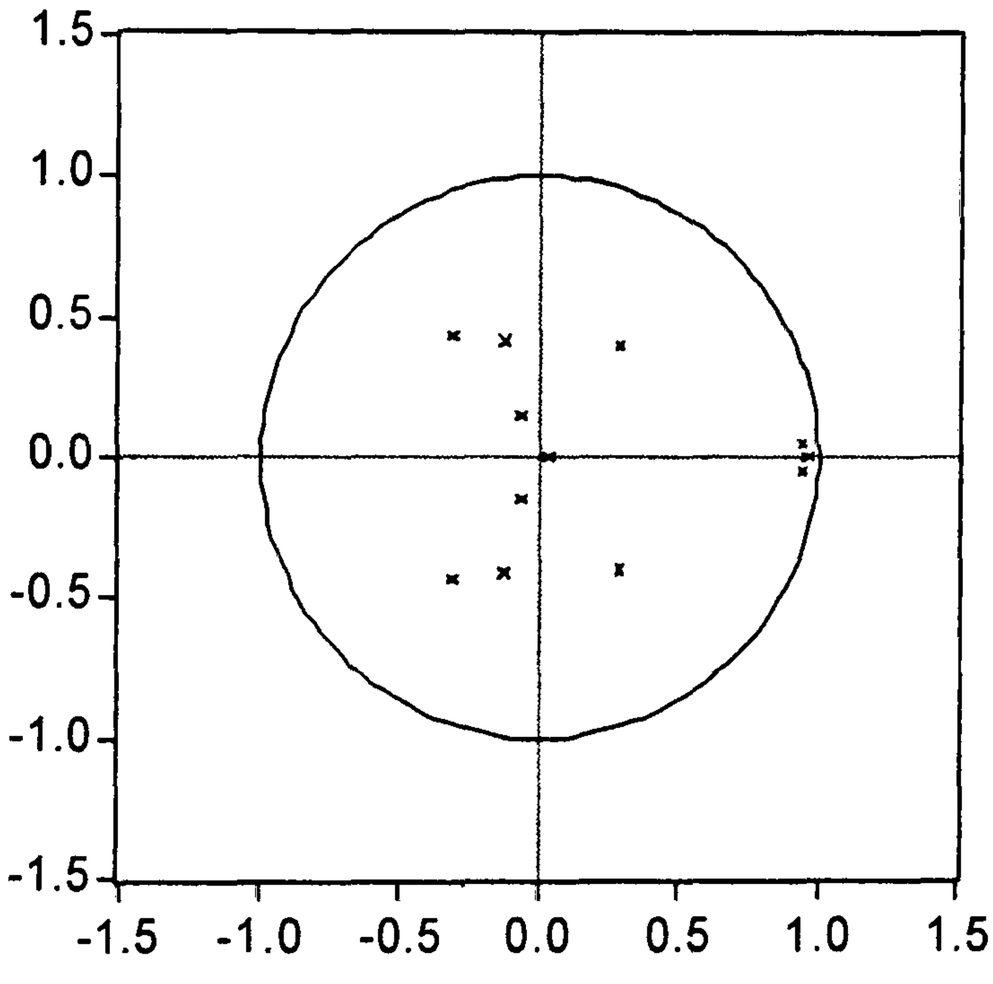
VAR(3) System for pooled data: reversed order of variables



VAR(3) System for pooled data

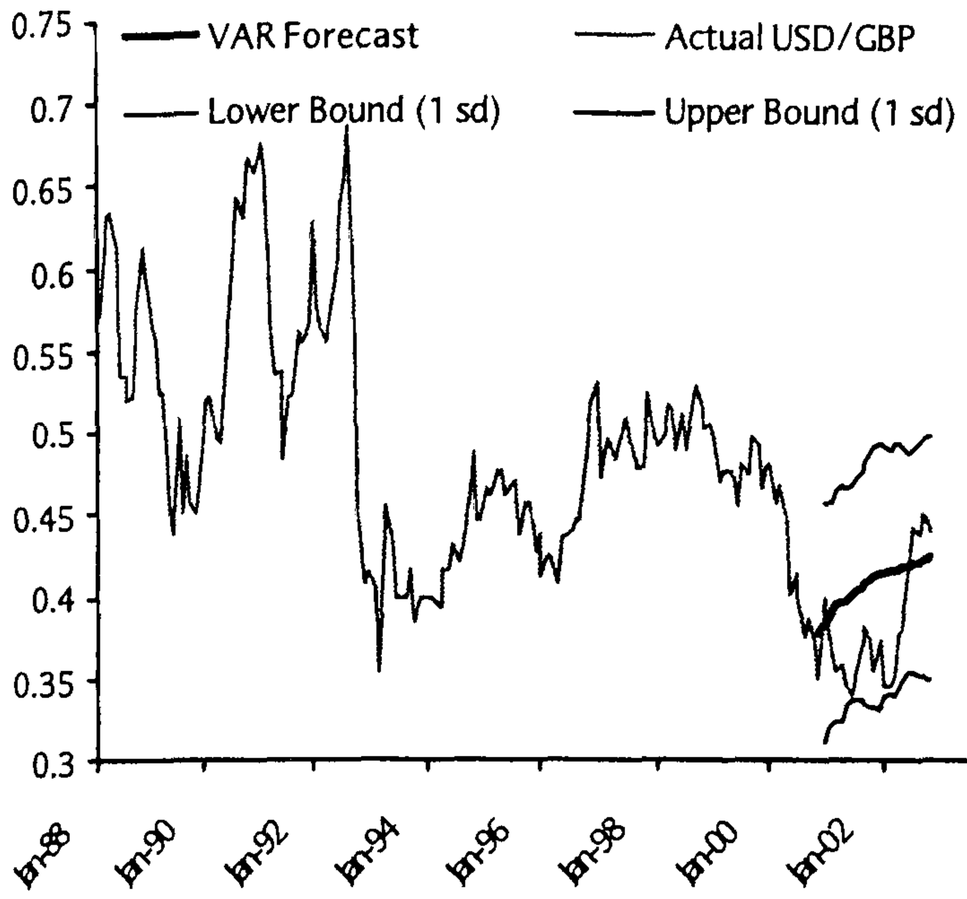


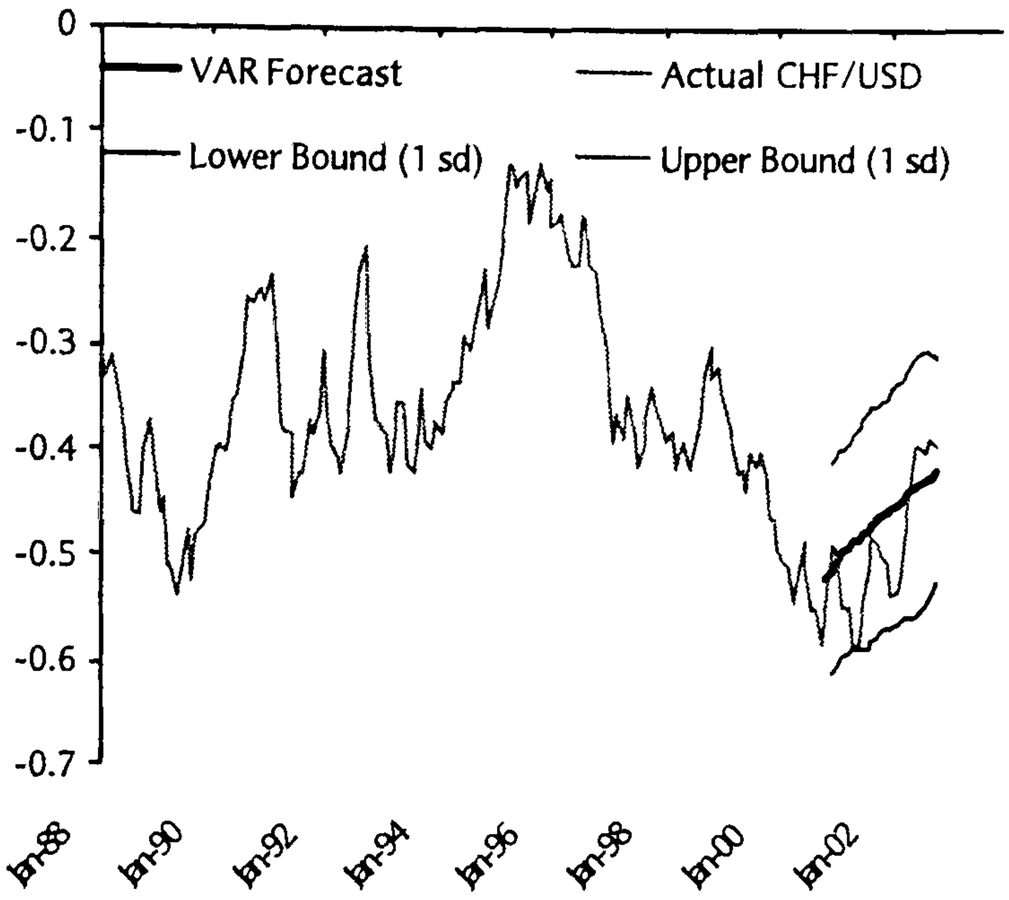
VAR(3) System for pooled data: reversed order

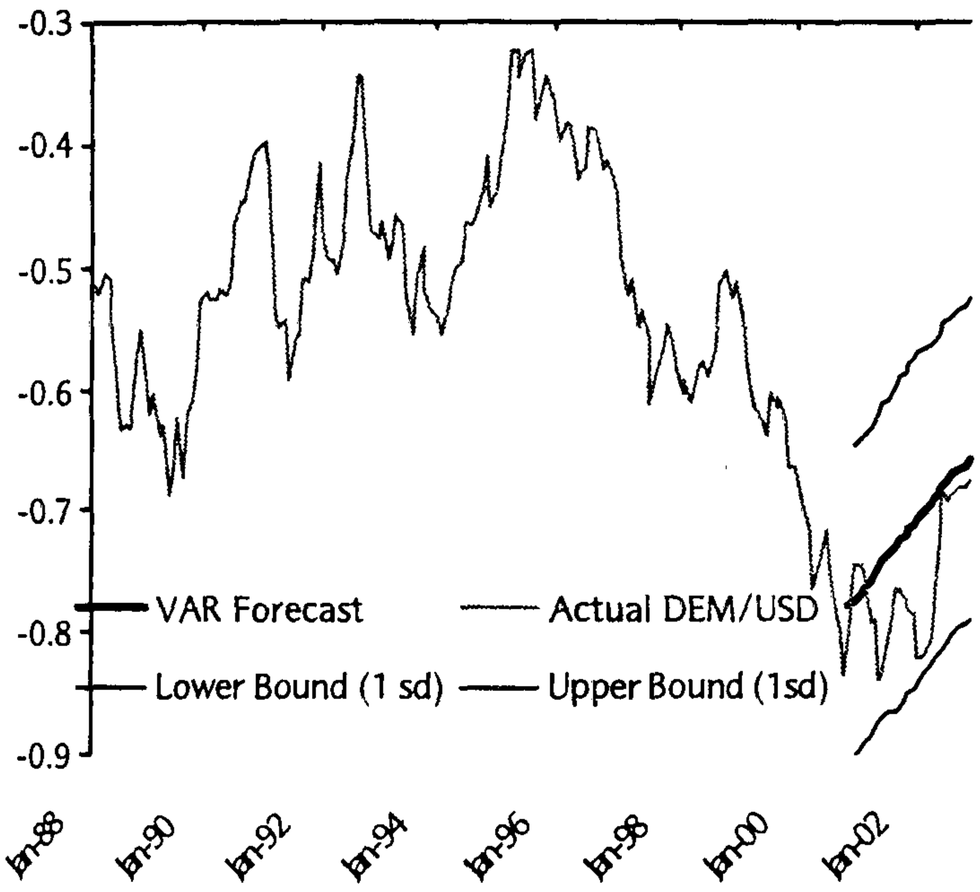


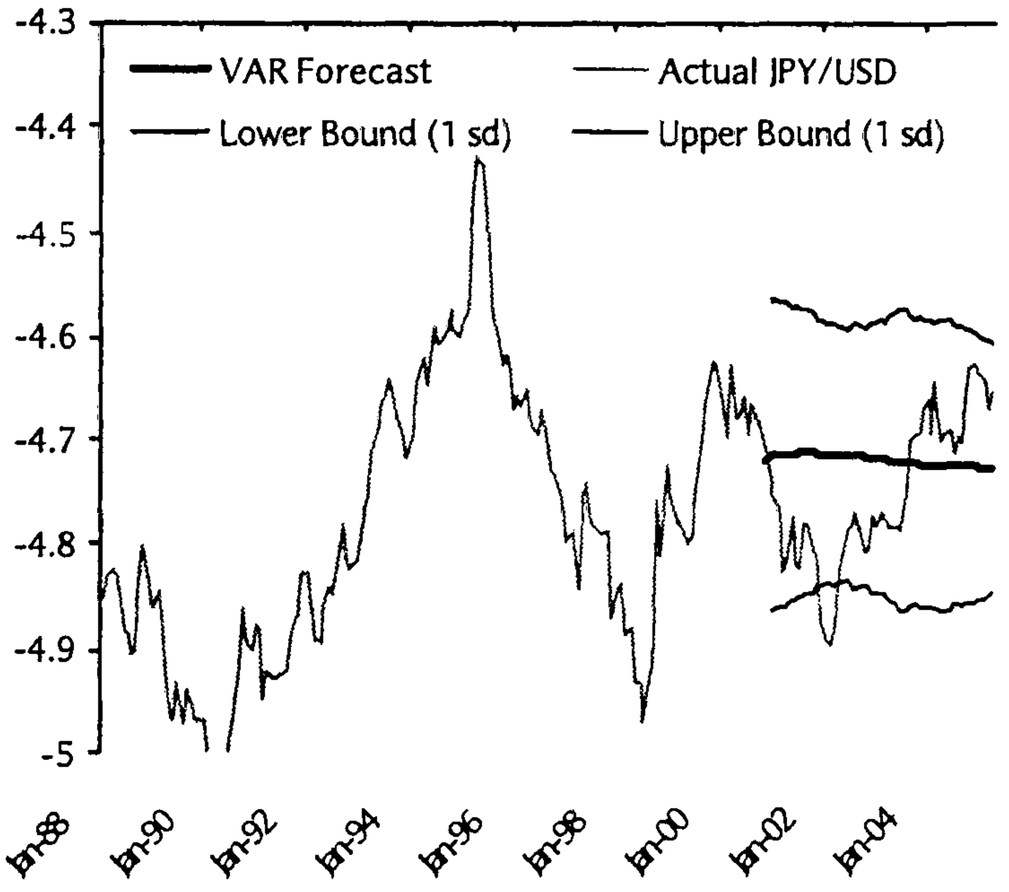
Note : Generalized Impulses as described by Pesaran and Shin (1998) and roots of AR characteristic polynomials. The variables used are: log foreign exchange (LFX), net purchases of U.S. equities (NEF), relative equity market returns (REM) and relative interest rates (RIR). Sample period : 1998 :01 – 2000 :12. For variable definitions please see data appendix.

Figure 2.10: Forecasting Nominal Exchange Rates: Meese-Rogoff revisited









2.11:

Figure ~~9.~~ Impulses response of exchange rates from shocks to net equity flows in U.S. assets using the Canova-DeNicolo method of sign restrictions. Pooled data. Fixed effects included.

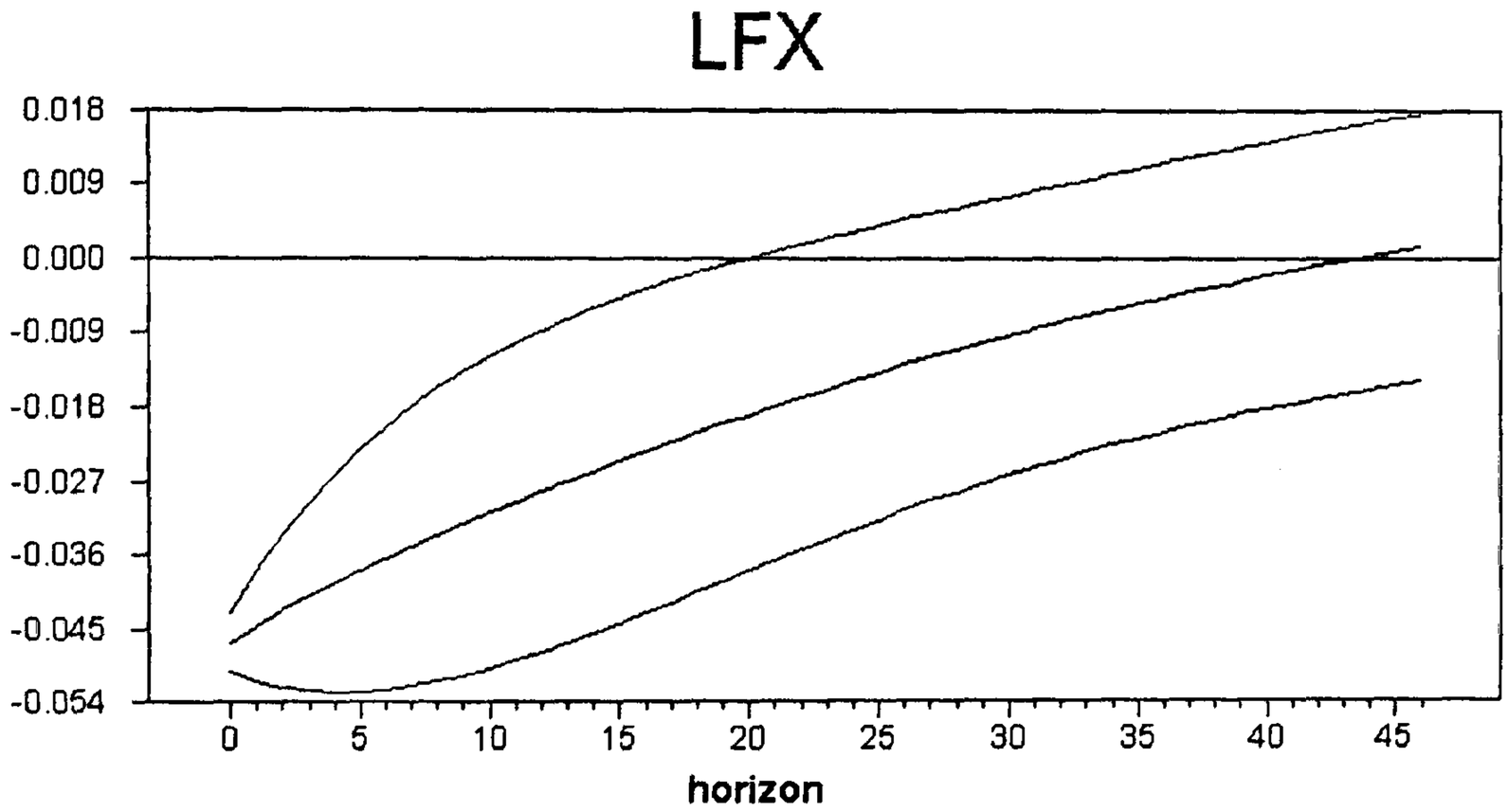
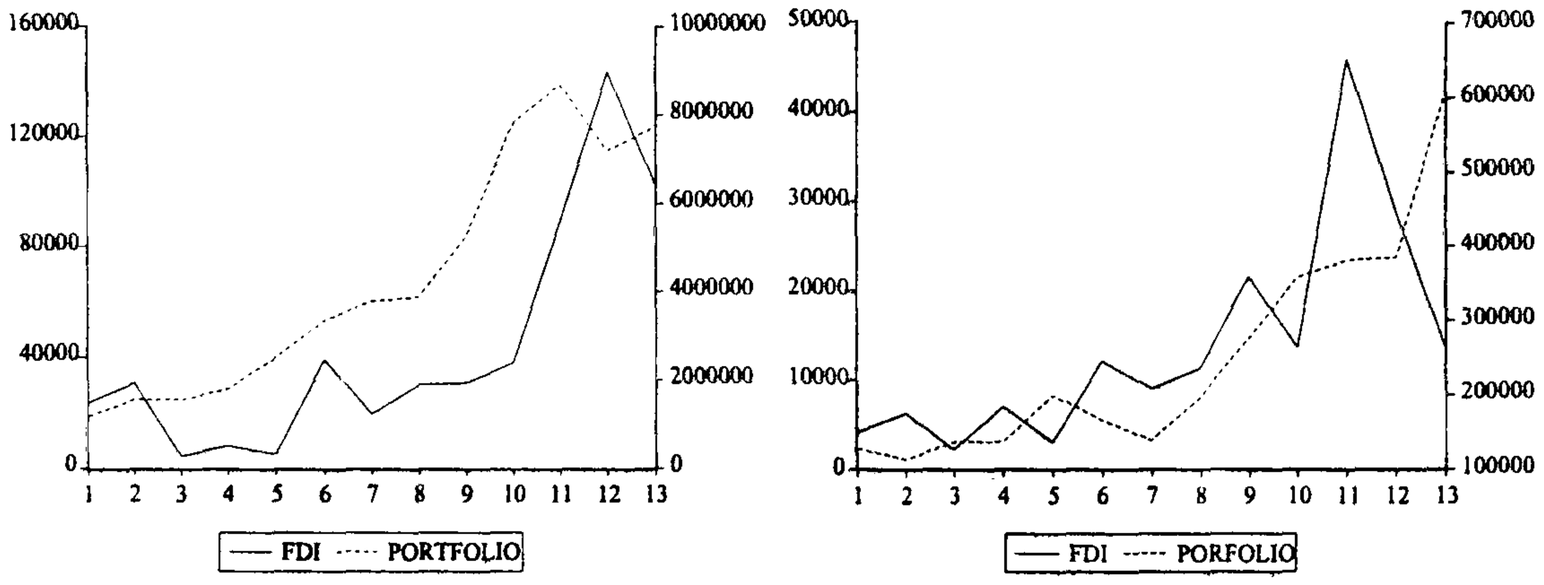
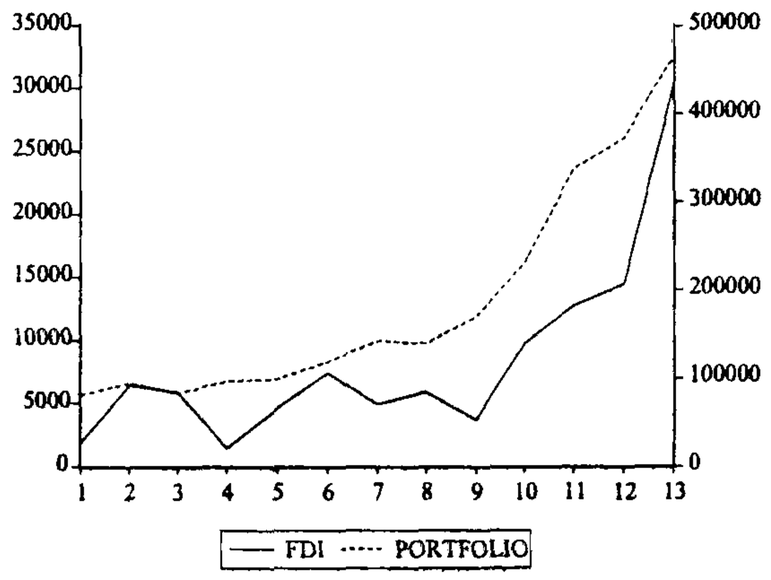


Figure 2.12: ~~10.~~ Gross cross border portfolio flows (right scale) v.s. gross cross border FDI's (left scale) between US and UK, Germany, Switzerland and Japan (in million \$). Sample: 1988-2000. (Correlations in parenthesis).

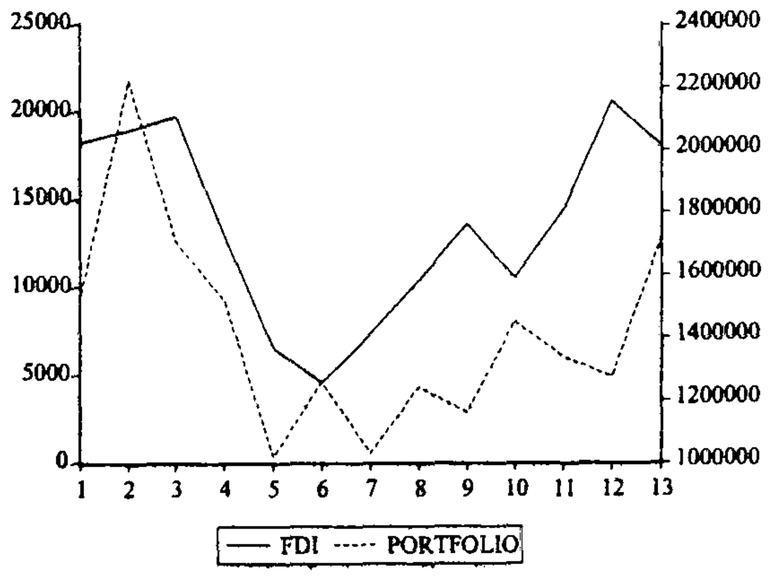


UK ($\rho = 0.76$)

Germany ($\rho = 0.55$)

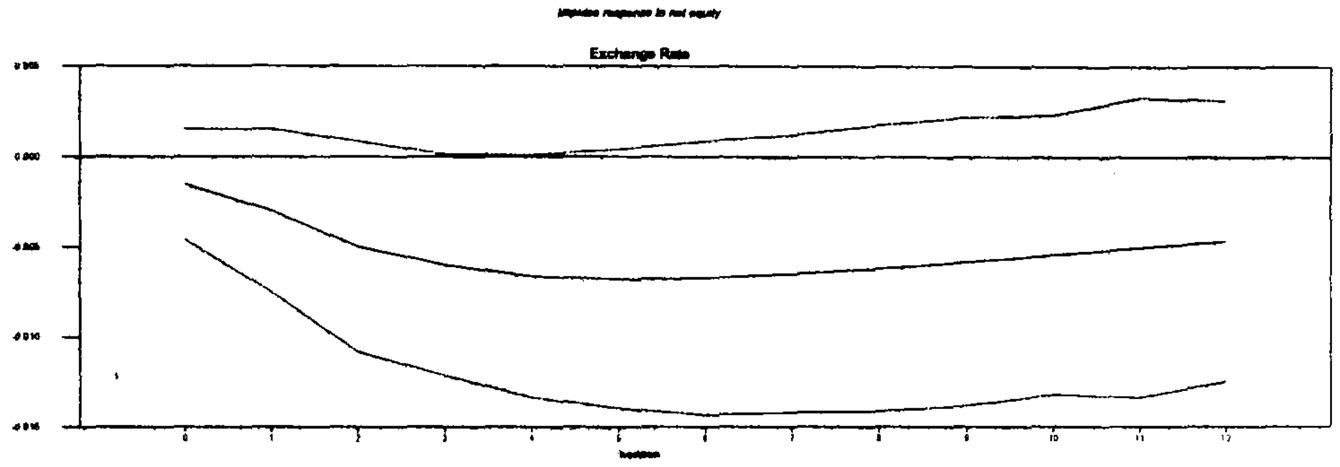


Switzerland ($\rho = 0.90$)

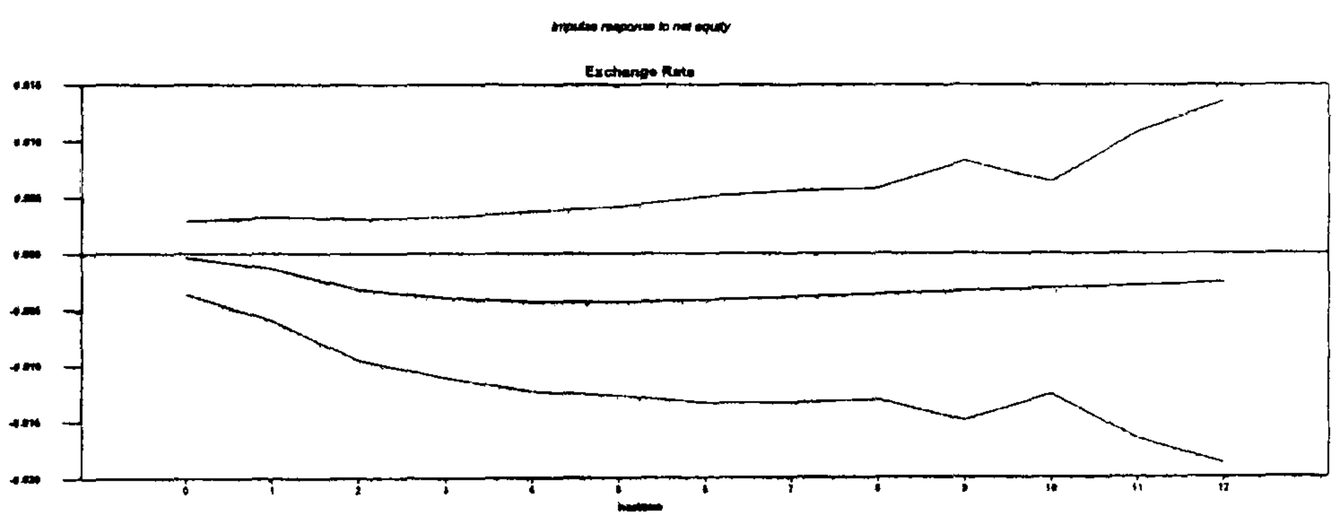


Japan ($\rho = 0.65$)

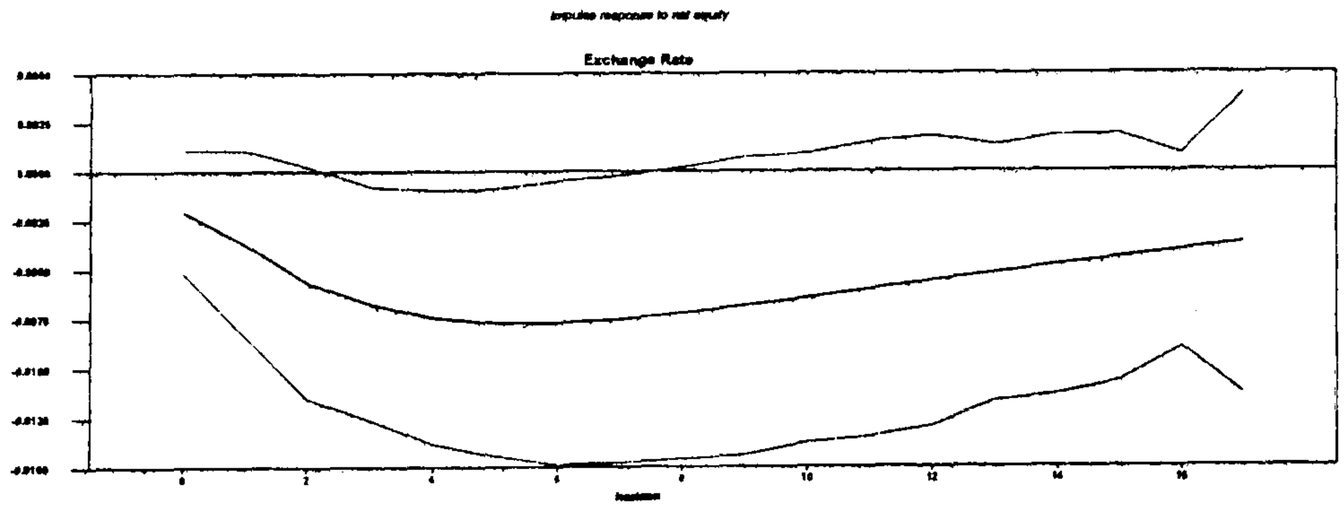
Figure ~~II.~~ 2.13: Impulses responses of exchange rates from shocks to net equity flows in U.S. assets with three different iid measurement errors. Pooled data. Fixed effects included. "Sigma" is the standard deviation of the measurement error.



sigma = 0.25



sigma = 0.4



sigma = 0.2

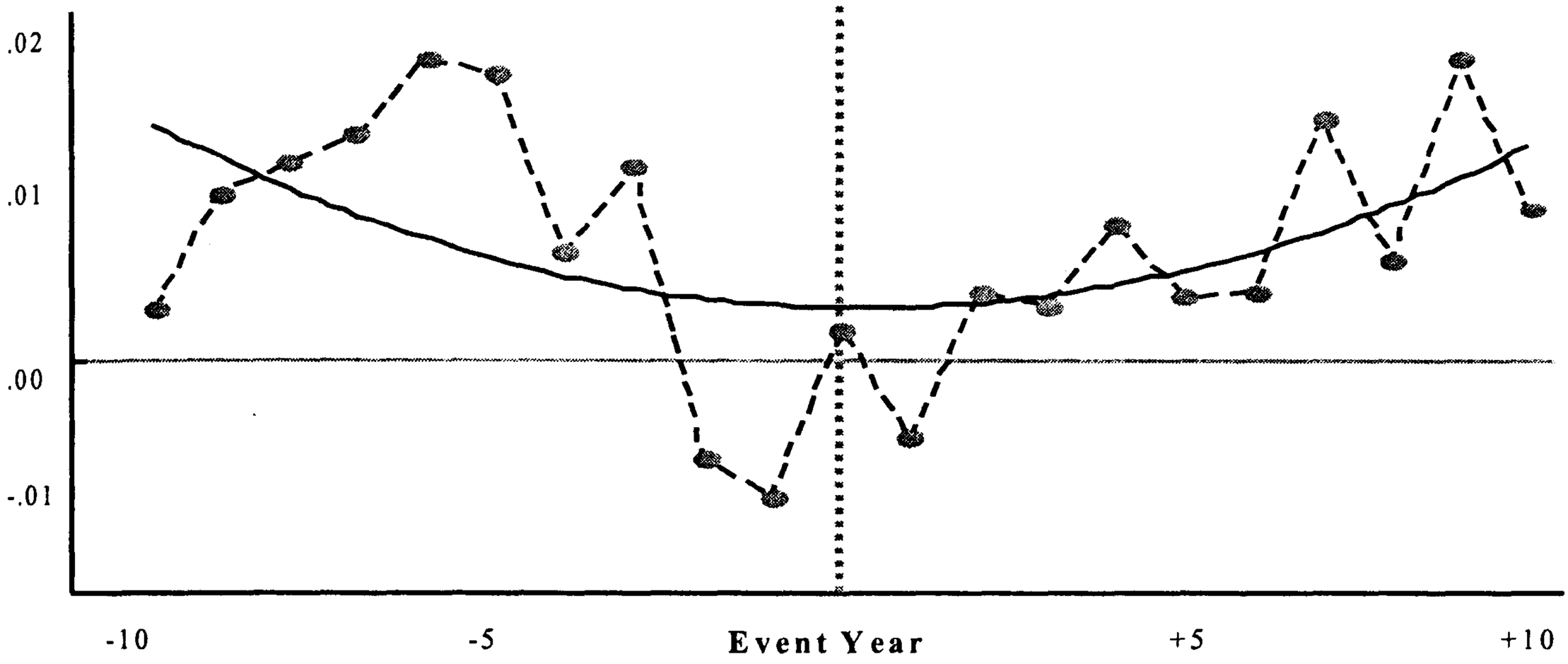


FIGURE 3.1:
MEAN ECONOMIC GROWTH BEFORE AND AFTER A PERMANENT DEMOCRATIZATION

The figure describes the evolution of the cross-country mean growth in the ten years before and after a permanent democratic transition. The mean is estimated in all 67 countries that experienced either a "full" either a "partial" or a "borderline partial" democratization episode in the 1960-2000 period. The bold line gives a quadratic fit. Table 1 lists the democratization countries, the year of transition and provides a small event description. *The Supplementary Democratization Dataset Appendix* gives a more detailed description of the democratization events. For details on our event identification methodology see Section III.

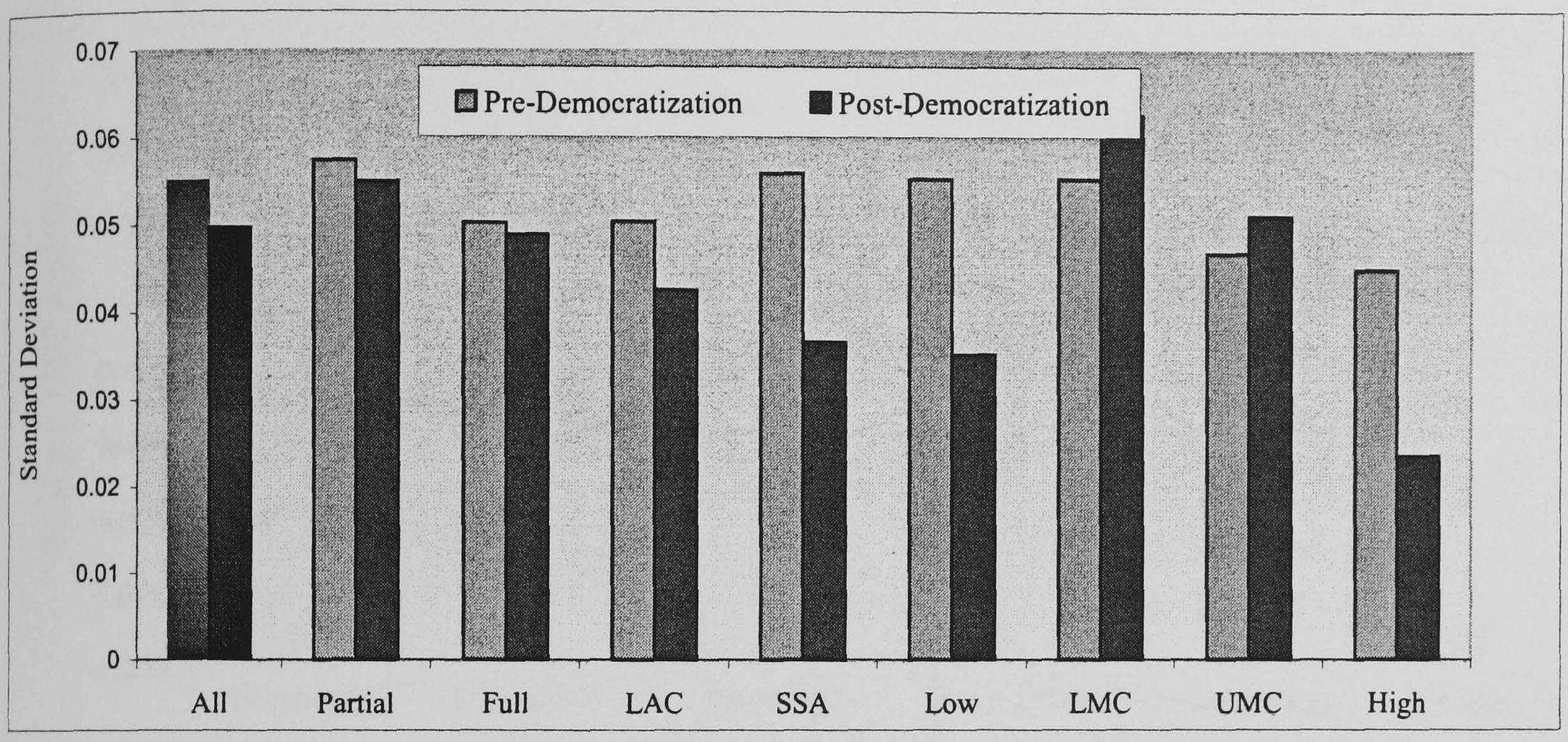


FIGURE 3.2:
STANDARD DEVIATION OF GROWTH BEFORE AND AFTER A PERMANENT DEMOCRATIZATION

The figure gives the standard deviation of real per capital GDP growth before and after a permanent democratic transition. "All" corresponds to the full sample of permanent democratization countries (N=67). "Partial" corresponds to the sample of partial & "borderline" democratization countries (N=29); "Full" corresponds to the sample of "full" democratization countries (N=38). "LAC" corresponds to Latin America and Caribbean countries (N=19); "SSA" to sub-Saharan democratization countries (N=19); Low corresponds to low income countries that democratized (N=24); "LMC" corresponds to "lower-middle-income countries" that democratized; "UMC" corresponds to "upper-middle-income countries that democratized (N=15); High corresponds to "high-income countries" that democratized (N=4). The classification is taken from the World Bank. Table 1 lists the democratization countries, the year of transition and provides a small event description. The *Supplementary Democratization Dataset Appendix* gives a more detailed description of the democratization events. For details on our event identification methodology see Section.

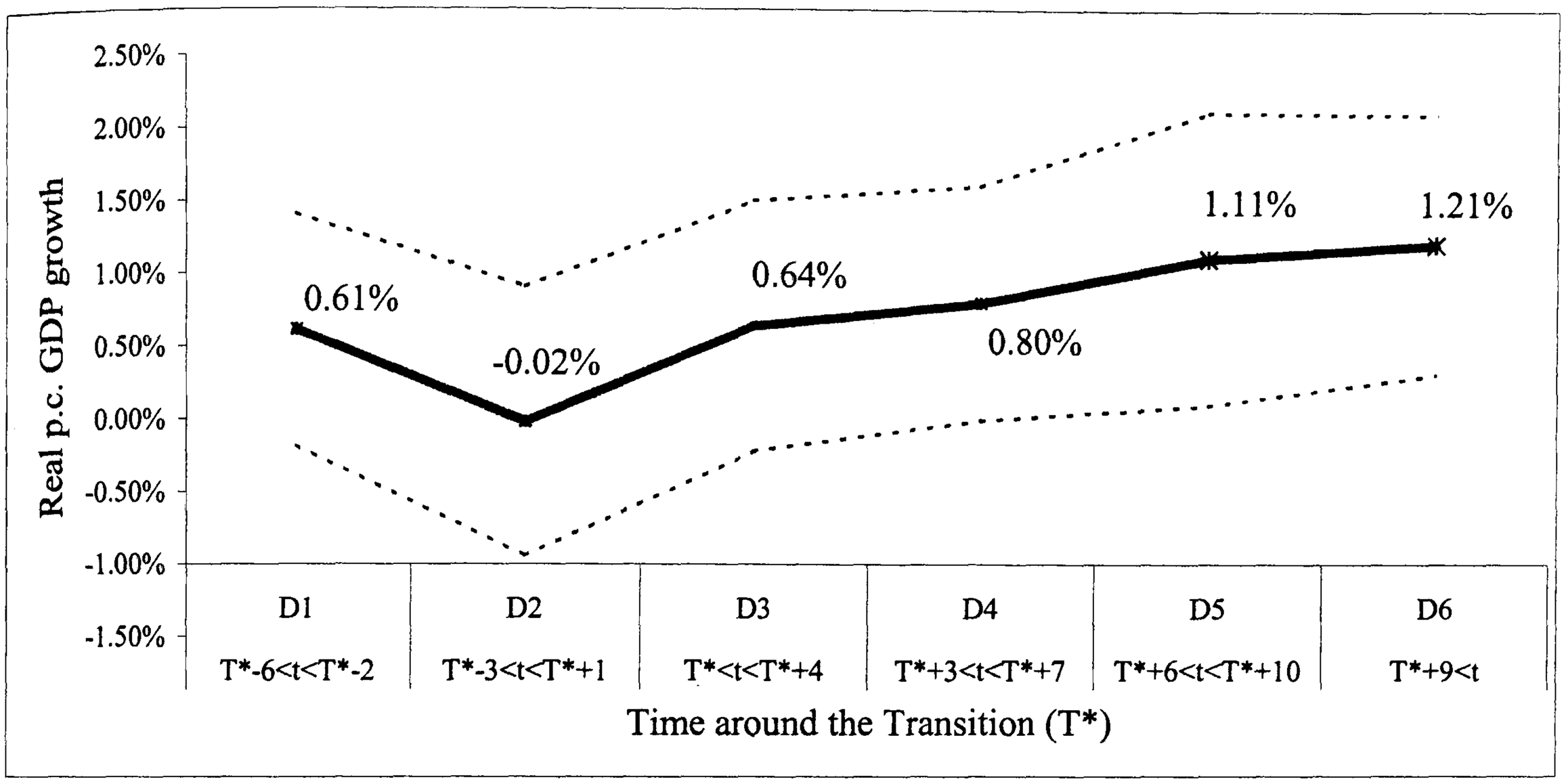
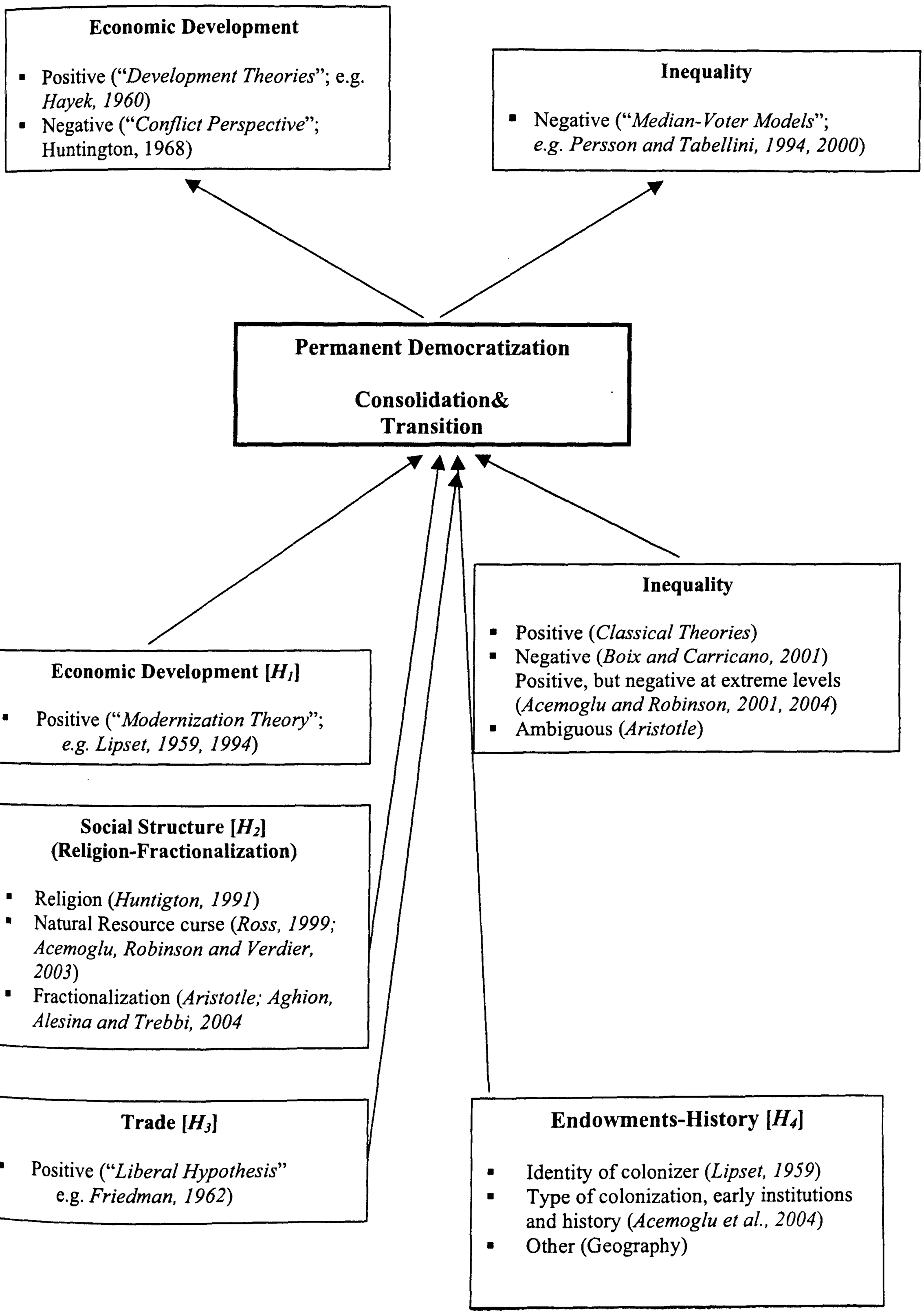


FIGURE 3.3:
DYNAMIC EFFECT OF A PERMANENT DEMOCRATIZATION/ FIXED EFFECT ESTIMATES

The figure plots the coefficient (and 95% confidence intervals) of 6 indicator variables that capture the (average) dynamic impact of a permanent democratization. The dependent variable is real per capita GDP growth. D^1 is a dummy variable that takes on the value one in the fifth, fourth, and third pre democratization year and zero otherwise. D^2 is a dummy variable that takes on the value one in the second, first pre-democratization year and on the transition year and zero otherwise. D^3 takes on the value one in the first, second and third post democratization year and zero otherwise. D^4 takes the value one in the fourth, fifth, and sixth post democratization year and zero otherwise. D^5 takes on the value one in the seventh, eighth, and ninth post democratization year and zero otherwise. D^6 takes the value one in the tenth and all subsequent (eleventh, twelfth, etc.) post democratization year and zero otherwise. The growth regression includes the following control variables: 2 lags of GDP growth (the dependent variable), 3 lags of investment, lag (4) of the natural logarithm of GDP, lag (1) of the log of life expectancy, lag (1) of trade openness, lag (1) of government consumption, and lag (1) of population growth. The specification also includes a full set of country fixed effects and is estimated with OLS. To control for common global shocks all variables enter the specification as deviations from period means. To achieve (almost) unbiased coefficients, the specification excludes all countries with less than 20 years of observations.

Figure 4.1:
Schematic Theoretical Overview Of the Driving Forces And the Consequences Of a Permanent Democratization



Tables

This Section gives all Tables for each of the 4 Thesis Chapters.

Table 1.1: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Consumption Spending - US	126	3705.47	2125.39	827.70	8153.80
Consumption Spending - Germany	126	359.01	173.34	121.80	689.28
Consumption Spending - UK	126	82.06	53.20	10.45	190.10
Consumption Spendin - Japan	126	202272.40	75843.71	55753.00	292688.00
Consumption Spending - Switzerland	126	42.92	15.39	18.27	67.22
CPI - US (2000 base)	126	74.06	22.69	31.02	108.09
CPI - Germany (2000 base)	126	77.77	19.40	43.00	104.90
CPI - UK (2000 base)	126	184.84	39.72	127.79	267.45
CPI - Japan (2000 base)	126	89.82	15.24	45.86	104.93
CPI - Switzerland (2000 base)	126	79.69	19.57	43.56	104.38
USD/GBP	126	0.59	0.09	0.39	0.87
USD/DEM	126	2.03	0.42	1.39	3.19
USD/JPY	126	175.62	67.60	84.48	303.32
USD/CHF	126	1.80	0.51	1.15	3.33

Note: Sample starts at first quarter of 1973 and end at second quarter of 2004 for a total of 126 observations. Data is retrieved from EcoWin. All consumption spending variables are in local currencies. Please refer to the text and the Data Appendix for more information.

Table 1.2: Descriptive Statistics for all variables used in regression analysis

Variable	Obs	Mean	Std. Dev.	Min	Max
Log RATE – Germany (LRG)	126	1.89	0.37	1.32	3.16
Log RATE – Japan (LRJ)	126	4.87	0.24	4.31	5.33
Log RATE – UK (LRUK)	126	-1.49	0.32	-2.38	-0.98
Log RATE – Switzerland (LRSW)	126	0.46	0.20	0.06	0.93
Log C – Germany (LCG)	126	4.35	1.12	2.82	6.57
Log C – Japan (LCJ)	126	2172.06	561.76	1155.57	3076.68
Log C- UK (LCUK)	126	0.40	0.20	0.08	0.72
Log C – Switzerland (LCSW)	126	0.52	0.07	0.41	0.64
Log C – US (LCUS)	126	45.82	14.52	26.57	75.43

Note: Sample starts at first quarter of 1973 and end at second quarter of 2004 for a total of 126 observations. Variables are constructed as follows: RATE = log ($Spot_k$ * CPIUS/CPI_k) and C = log ((Consumption Spending)$_k$/CPI_k), where k = Germany, Japan, UK, Switzerland and US. Please refer to the text and the data Appendix for more information.

Table 1.3: Pairwize Correlation Matrix

	LRG	LRJ	LRUK	LRSWI	LCG	LCJ	LCUK	LCSW
LRG	1							
obs	126							
LRJ	0.673	1						
p-value	0.000							
obs	126	126						
LRUK	0.156	-0.478	1					
p-value	0.082	0.000						
obs	126	126	126					
LRSW	0.828	0.869	-0.356	1				
p-value	0.000	0.000	0.000					
obs	126	126	126	126				
LCG	-0.271	-0.659	0.652	-0.600	1			
p-value	0.002	0.000	0.000	0.000				
obs	126	126	126	126	126			
LCJ	-0.276	-0.715	0.752	-0.668	0.963	1		
p-value	0.002	0.000	0.000	0.000	0.000			
obs	126	126	126	126	126	126		
LCUK	-0.189	-0.700	0.842	-0.643	0.927	0.976	1	
p-value	0.034	0.000	0.000	0.000	0.000	0.000		
obs	126	126	126	126	126	126	126	
LCSW	-0.209	-0.649	0.676	-0.566	0.984	0.968	0.942	1
p-value	0.019	0.000	0.000	0.000	0.000	0.000	0.000	
obs	126	126	126	126	126	126	126	126
LCUS	-0.218	-0.670	0.686	-0.587	0.981	0.974	0.946	0.994
p-value	0.014	0.000	0.000	0.000	0.000	0.000	0.000	0.000
obs	126	126	126	126	126	126	126	126

Note: Sample starts at first quarter of 1973 and end at second quarter of 2004 for a total of 126 observations. Variables are constructed as follows: RATE = log ($Spot_k$ * CPIUS/CPI_k) and C = log ((Consumption Spending)$_k$/CPI_k), where k = Germany, Japan, UK, Switzerland and US. Please refer to Table 1.2 and the data Appendix for more information.

1.4

Table 1. Estimated relationship of real exchange rates and real consumption spending.

	SRBC				*GRBC*			
	JA	GE	UK	SW	JA	GE	UK	SW
C					-0.95	-2.60*	-0.48	-1.93**
a_0					-0.001	-0.005*	-0.003*	-0.001
a_1	-0.01	0.02**	-0.01	0.01				
b_1	-0.02**	-0.02***	0.02***	-0.016				
B_{us}	0.8*	0.05**	0.01**	0.006	0.59**	0.72*	0.46***	0.47**
B_2	-0.52*	-0.01**	-0.003	-0.1***	-0.23	0.16	0.02	-0.29
Γ_1	0.67*	0.92*	1.03*	0.94*	1.02*	0.8*	1.04*	0.9*
Γ_2	-0.18***	-0.21	-0.34**	-0.069	-0.16	-0.22***	-0.34**	-0.05
Γ_3		0.17***	0.19**			0.2**	0.23	
DW	2.03	1.98	2.14	2.01	1.92	1.96	2.06	2.13
AIC	-99.5	-94.2	-112.7	-67.16	-94	–93	-111	-68
SBC	-84.2	-77.1	-94.89	-51.83	-79	-76	-93	-53

Notes: *,**,*** Denotes significance in the 1%, 5% and 10% level respectively. The sample period is from the 1st quarter of 1973 until the 1st quarter of 1997 for a total of 97 observations for countries: Japan (JA), Germany (GE), United Kingdom (UK) AND Switzerland (SW).

The estimated equation for SRBC model is: $RATE(t) = \Sigma(t)t + B_{us}LC_{us} + B_kLC_k + \sum_{i=1}^{p} \Gamma_i RATE(t-i) + \varepsilon(t)$ and variables $RATE, LC_{us}, LC_k$ and Σ are given by: $LC_k = \log\left[\frac{M_k(t)}{\Pi_k(t)}\right], \Sigma(t) = \log\left[\frac{\delta_k(t)}{\delta_{us}(t)}\right]$ that follows a deterministic sinusoidal function of the form of (10) such that $\Sigma(t) = a_0 + a_1 \sin\frac{2\pi k}{T}t + b_1 \cos\frac{2\pi k}{T}t + \varepsilon(t)$ with $a_0 = \log\left[\theta_k \frac{1-\eta_k}{1-\eta_{us}}\right]$, $\sum_{i=1}^{p} \Gamma_i RATE(t-i)$ are lags of the dependent variable and $\varepsilon(t)$ an error process.

The estimated equation for GRBC model is: $RATE(t) = CONSTANT + B_0TREND + B_{us}LC_{us} + B_kLC_k + \sum_{i=1}^{p} \Gamma_i RATE(t-i) + \varepsilon(t)$.According to the theoretical model the expected signs are: (i)For $a_1 \sin\frac{2\pi k}{T}t + b_1 \cos\frac{2\pi k}{T}t = \ln(\delta(t)_{us} - \delta(t)_k)$ can be:> 0 or < 0. (ii) $B_{US} = \eta_{us}$ has to be > 0 and (iii) $B_k = \eta_k$ has to be < 0. AIC and SBC are used as model selection criteria since they trade off a reduction in the sum of squared residuals for a more parsimonious model. Durbin-Watson is also reported as an indicator of serial correlation in the residuals.

Table 2.1: Percentage of daily foreign exchange market turnover by exchange rate.

Year	1992	1995	1998	2001
Exchange Rate				
Dollar/pound	10	7	8	11
Dollar/euro	-	-	-	30
Dollar/mark	25	22	20	-
Dollar/yen	20	21	18	20
Dollar/Swiss F	6	5	5	5
Total	61	55	51	66

Note: Daily averages. Adjusted for local and cross border double counting. Source: Bank of International Settlements.

Table 2.2: Capital flows elasticities of nominal exchange rates. Dependent variable: Log of nominal exchange rate (FX), relative to the U.S. dollar. Independent variables: net flows in foreign equities (FE), net flows in U.S. equities (USE), net flows in foreign bonds (FB), Net flows in U.S. bonds (USB). *P-values* in parenthesis.

FX		$/pound		$/mark		$/swiss franc		$/yen	
		$ln(FX)_t = \alpha + \beta_i(\ln(Capital\ Flow_i))_t + \varepsilon_t$							
Flow		$\widehat{\beta}_i$	R^2	$\widehat{\beta}_i$	R^2	$\widehat{\beta}_i$	R^2	$\widehat{\beta}_i$	R^2
FE	88-00	-0.08(0.000)	0.10	0.02(0.033)	0.01	-0.08(0.002)	0.07	0.03(0.018)	0.03
	88-94	-0.10(0.072)	0.04	0.09(0.000)	0.14	-0.03(0.299)	0.01	-0.09(0.000)	0.13
	95-00	-0.05(0.015)	0.05	-0.14(0.016)	0.09	-0.15(0.000)	0.21	-0.09(0.000)	0.21
USE	88-00	-0.14(0.000)	0.27	-0.18(0.000)	0.29	-0.23(0.000)	0.26	0.05(0.000)	0.06
	88-94	-0.26(0.000)	0.33	-0.13(0.000)	0.25	-0.18(0.000)	0.16	0.18(0.000)	0.22
	95-00	-0.10(0.025)	0.11	-0.26(0.000)	0.17	-0.34(0.000)	0.42	-0.08(0.000)	0.14
FB	88-00	-0.12(0.000)	0.15	-0.03(0.246)	0.02	-0.08(0.002)	0.07	0.05(0.230)	0.06
	88-94	-0.02(0.000)	0.12	-0.07(0.002)	0.12	-0.03(0.299)	0.01	0.18(0.020)	0.21
	95-00	-0.09(0.030)	0.10	-0.01(0.897)	0.00	-0.15(0.000)	0.21	-0.08(0.050)	0.12
USB	88-00	-0.16(0.000)	0.22	-0.05(0.028)	0.02	-0.07(0.002)	0.05	0.05(0.001)	0.02
	88-94	-0.31(0.000)	0.22	-0.02(0.367)	0.01	-0.06(0.034)	0.04	0.18(0.020)	0.22
	95-00	-0.19(0.000)	0.28	-0.05(0.315)	0.01	-0.07(0.08)	0.05	-0.08(0.001)	0.10

Table 2.3: Likelihood ratio tests to identify which flows matter for exchange rates.

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
$/DEM	114.6 (0.000)	0.296 (0.862)	30.00 (0.000)	22.57 (0.000)	3.351 (0.067)	0.160 (0.899)
$/GBP	46.06 (0.000)	1.34 (0.260)	25.15 (0.000)	43.53 (0.000)	14.75 (0.000)	10.04 (0.002)
$/CHF	53.99 (0.000)	0.161 (0.688)	2.47 (0.105)	31.64 (0.000)	7.14 (0.008)	0.203 (0.652)
$/JPY	29.61 (0.000)	0.723 (0.391)	0.006 (0.933)	13.48 (0.000)	0.026 (0.871)	0.010 (0.917)

Note: All tests are based on estimated bilateral VAR's including capital flows, exchange rates, equity return differentials and interest rate differentials. Sample is 1988:01-2000:12.

(1) H_0: Net total accumulation of bonds plus net total accumulation of equities equals zero.

(2) H_0: Net total accumulation of bonds equals zero (in the presence of equities).

(3) H_0: Net total accumulation of equities equals zero (in the presence of bonds).

(4) H_0: Net accumulation of foreign equities (by U.S. residents) minus net accumulation of U.S. equities (by foreign residents) equals zero.

(5) H_0: Net accumulation of foreign equities (by U.S. residents) equals zero (in the presence of foreign equities).

(6) H_0: Net accumulation of U.S. (by foreign residents) equities equal zero (in the presence of U.S. equities). Under the null hypothesis the test statistic is distributed as a χ^2 with degrees of freedom equal to the number of the restrictions. *P-values* in parenthesis.

Table 2.4: Percentage of the in-sample forecast error variance of nominal exchange rates explained by capital flows (CF), equity return differentials (ER), and interest rate differentials (RATE).

Currency/Variable	CF	ER	RATE
Sample 1988:1-2000:12			
\$/GBP			
1-month horizon	16	0.4	0
3-months horizon	23	0.2	0
6-months horizon	25	0.2	0
9-months horizon	26	0.1	0.2
12-months horizon	27	0.1	0.4
24-months horizon	28	0.1	2
\$/DEM			
1-month horizon	7	0.4	0
3-months horizon	14	0.2	0
6-months horizon	22	0.1	0.1
9-months horizon	27	0.1	0.3
12-months horizon	30	0.1	0.5
24-months horizon	38	0.1	2
\$/CHF			
1-month horizon	13	1	0
3-months horizon	23	1	0.1
6-months horizon	30	1	0.6
9-months horizon	33	1	2
12-months horizon	34	1	3
24-months horizon	33	1	10
\$/YEN			
1-month horizon	0.3	0	0
3-months horizon	1	0.2	0.1
6-months horizon	1	0.1	0.3
9-months horizon	2	0.1	1
12-months horizon	4	0.1	1
24-months horizon	7	0.1	3

Table 2.5: Percentage of the in-sample forecast error variance of nominal exchange rates explained by equity flows (CF: foreign plus U.S.), equity return differentials (ER), and interest rate differentials (RATE). The pooled data excludes Japan. Fixed effects included.

Variable	CF	ER	RATE
Pooled-Sample 1988:1-2000:12/Observations:468			
Horizon			
1-month	7	1	0
3-months	10	1	0
6-months	13	1	0.1
9-months	17	1	0.4
12-months	19	1	1
24-months	23	2	4
36-months	23	2	7

Table 2.6: Root mean square forecast error in percentages. Meese-Rogoff revisited.

	Model:	Random Walk	Equity-augmented VAR
Exchange Rate	Horizon		
	1 month	1.18	0.61
	3 months	0.88	1.61
\$/pound	6 months	1.81	3.47
	12 months	1.53	3.88
	18 months	1.62	4.28
	1 month	4.67	5.01
	3 months	3.35	3.71
\$/mark	6 months	2.94	2.88
	12 months	2.64	2.85
	18 months	3.33	3.30
	1 month	3.62	3.32
	3 months	2.24	2.03
\$/swiss franc	6 months	2.29	2.27
	12 months	3.24	2.80
	18 months	4.45	2.91
	1 month	4.12	4.51
	3 months	5.77	6.19
\$/yen	6 months	7.79	8.27
	12 months	8.99	9.49
	18 months	11.83	12.29

Table 2.7: Diebold-Mariano statistics for the best forecast of exchange rates among a capital-augmented VAR (F), a standard VAR (VRW) and a univariate random walk (RW). *P-values* in parenthesis. The test indicates which forecast is the best according to the mean square error criterion and tests the null hypothesis that there exists a significant difference between two tested forecasts.

	Test:	F vs. RW	F vs. VRW	RW vs. VRW
FX	k			
	3	1.618 (0.105) : RW	−1.526 (0.127) : F	−2.394 (0.017) : RW
	6	2.068 (0.038) : RW	−2.397 (0.017) : F	−2.657 (0.001) : RW
\$/GBP	12	4.183 (0.000) : RW	−4.933 (0.000) : F	−5.519 (0.000) : RW
	18	4.759 (0.000) : RW	−6.39 (0.000) : F	−6.615 (0.000) : RW
	24	−4.163 (0.000) : F	−4.584 (0.000) ; F	−1.010 (0.312) ; RW
	3	−7.547 (0.000) : F	−0.203 (0.839) : F	0.357 (0.720) : RW
	6	0.913 (0.360) : RW	−1.336 (0.182) : F	−1.304 (0.192) : RW
\$/DEM	12	2.085 (0.037) : RW	2.932 (0.003) : F	−3.059 (0.002) : RW
	16	3.037 (0.002) : RW	−4.210 (0.000) : F	−4.124 (0.000) : RW
	24	−0.133 (0.895) : F	−5.536 (0.000) : F	−2.856 (0.004) : RW
	3	−0.4677 (0.640) : F	−2.796 (0.006) : F	−1.064 (0.287) : RW
	6	1.509 (0.131) : RW	−2.206 (0.027) : F	−1.844 (0.065) : RW
\$/CHF	12	1.456 (0.145) : RW	−4.64 (0.000) : F	−2.963 (0.003) : RW
	18	2.489 (0.012) : F	−5.907 (0.000) : F	1.456 (0.145) : RW
	24	−1.681 (0.092) : F	−3.655 (0.000) F	0.7865 (0.432) : VRW
	3	−0.467 (0.640) : F	−573 (0.000) : F	−577 (0.000) : RW
	6	1.509 (0.131) : RW	−178 (0.00) : $0F$	−180 (0.000) : RW
\$/JPY	12	1.456 (0.145) : RW	−177 (0.000) : F	179 (0.000) RW
	18	2.489 (0.012) : RW	226 (0.000) : F	228 (0.000) : RW
	24	−1.681 (0.092) : F	−113 (0.000) : F	−115 (0.000) : RW

Table 2.8: Estimated residual cross correlations. Sample 1988:1-2000:12. The pooled data excludes Japan. Fixed effects included.

	CFF	CFUS	ER	FX	RATE
ρ_{ij}					
CFF	1				
CFUS	0.073	1			
ER	0.137	0.063	1		
FX	-0.005	-0.254	-0.031	1	
RATE	0.062	0.127	-0.045	-0.066	1

Note: CFF denotes net accumulation of foreign equities, CFUS denotes net accumulation of U.S. equities, ER denotes equity return differentials (foreign minus U.S.), FX denotes foreign exchange rate, and RATE denotes interest rate differentials (foreign minus U.S.).

Table 2.9: Percentage of the forecast error variance of nominal exchange rates explained by net equity flows in U.S. assets (NANORMUS), net equity flows in foreign assets (NANORMF), equity return differentials (ER) and interest rate differentials (RATE). The pooled data excludes Japan. Fixed effects included.

Variable	NANORMF	NANORMUS	ER	RATE
Pooled-Sample 1988:1-1994:12/Observations:282				
Horizon				
1 month	0.2	6	0.2	0
3 months	1	7	0.2	0.2
9 months	4	13	0.3	0.1
12 months	5	14	0.3	0.2
24 months	6	15	0.3	0.4
36 months	6	15	0.3	0.5
Pooled-Sample 1995:1-2000:12/Observations:186				
Horizon				
1 month	0	9	0	0
3 months	0.1	15	2	0.2
9 months	1	20	1	4
12 months	3	21	1	6
24 months	6	20	1	14
36 months	6	20	1	16

Table 3.1: Democratization Event Chronology

Panel A— "Full" Democratization Episodes

Country & Year	Brief Description
Argentina (1983)	First free and fair elections after a prolonged period of military dictatorship.
Benin (1991)	Adoption of new democratic constitution; free and fair elections.
Bolivia (1982)	Return to civilian rule. Military steps down. Reconvention of 1980 democratic constitution.
Brazil (1985)	First internationally deemed fair and free elections after a more than two decade long military rule.
Bulgaria (1990)	First post-communism free and fair general assembly elections.
Cape Verde (1991)	First post independence free and fair elections.
Chile (1990)	First free and fair presidential elections; Ending of two decade long military rule of Augusto Pinochet.
Croatia (2000)*	First free and fair legislative and presidential elections since independence and the ending of the Bosnian War.
Czech Republic (1993)	Independence from Czechoslovakia. First post-communism constitution came into effect.
Dominican Republic (1978)	Return to civilian rule. Free and fair presidential elections.
Ecuador (1979)	Internationally deemed free and fair Presidential elections after a prolonged period of military rule.
El Salvador (1994)	First free and fair elections after the end of a long-lasting civil war and military rule.
Estonia (1991)	Independence from USSR. Ratification of old (1938) constitution.
Greece (1975)	New democratic constitution; parliamentary elections marked the return to civilian rule.
Grenada (1984)	First free and fair elections after the US invasion (in October 1983) and the ending of military rule.
Guyana (1992)	First post independence (1966) free and fair elections.
Honduras (1982)	Adoption of new democratic constitution after a prolonged period of oppressive rule. First democratically elected president takes office.
Hungary (1990)	First post communism free and fair Presidential elections.
Korea, Republic of (1988)	Democratically elected government resumes office. Adoption of new democratic constitution.
Latvia (1991)	Independence from USSR. Ratification of old (1922) democratic constitution.
Lithuania (1991)	Independence form USSR.
Malawi (1994)	First post independence (1961), free and fair parliamentary and presidential elections.
Mali (1992)	New democratic constitution established a multi-party system. Fair and free legislative and presidential elections followed.
Mexico (1997)	For the first time since 1929 the Institutional Revolutionary party (PRI) lost the absolute power in the Lower House after the 1997 legislative elections.
Mongolia (1992)	New democratic constitution established a multi-party system.
Panama (1994)	Free and fair presidential and legislative elections after the US intervention.
Peru (1980)	Internationally declared fair and free legislative and presidential elections.
Philippines (1987)	Adoption of new democratic constitution; free and fair elections led to the overthrown of Marco's regime.
Poland (1990)	First post communism, free and fair, presidential, legislative and local elections. Adoption of new democratic constitution.
Portugal (1976)	Return to civilian rule after a long-lived military regime (since 1926).
Romania (1990)	First post communism free and fair elections.
Sao Tome and Principe (1991)	First post independence (in 1975), fair and free legislative and presidential elections.
Slovak Republic (1993)	Independence form Czechoslovakia. First post-communism elections; a new democratic constitution came into effect.
Slovenia (1992)	First since gaining independence from Yugoslavia, free presidential and legislative elections. Adoption of a new democratic constitution.
South Africa (1994)	First free elections with universal participation, brought in power Nelson Mandela and ended the Apartheid regime.
Spain (1978)	New democratic constitution came into effect after Franco's death.
Thailand (1992)	Military was forced to step down. Free legislative elections followed.
Uruguay (1985)	Army returned the power to the democratically elected president.

Table 3.1: (cont.)

Panel B—"Partial" Democratization Episodes

Country & Year	Brief Description
Albania (1992)	Subsequent (in 1991 and 1992) elections marked the ending of the communist rule.
Bangladesh (1991)	First post independence (1971) free and fair elections.
Comoros (1990)	First post independence (1975) free and fair elections.
Djibouti (1999)*	First post independence (1977) internationally declared free and fair elections.
Ethiopia (1995)	First multi-party elections after a long-lived communist era.
Ghana (1996)	Internationally deemed free and fair parliamentary and presidential elections.
Guatemala (1996)	End of civil-war; return to civilian rule.
Haiti (1994)	A US intervention brought in power the winner of the 1990 elections.
Indonesia (1999)*	First multi-party elections after the collapse of the Suharto regime.
Lesotho (1993)	Military abandons power and internationally deemed free and fair elections mark the return to civilian rule.
Macedonia, FYR (1991)	Independence from Yugoslavia. First constitution approved. National unity government formed.
Madagascar (1993)	Presidential elections after a twenty-year long military junta.
Mozambique (1994)	First post independence (1975) parliamentary and presidential elections.
Nepal (1991)	First free and fair elections since the early sixties.
Nicaragua (1990)	Free and fair elections after the Somosa dictatorship and the Santinistas revolution.
Nigeria (1999)*	After consecutive coups and military interventions, internationally declared free and fair elections mark the return to civilian rule.
Paraguay (1993)	First Presidential elections after decades of military rule.
Russia (1993)	Adoption of first post-communism constitution; Free and fair Duma elections.
Senegal (2000)*	First post independence (1960), internationally deemed fair and free elections.
Suriname (1991)	Return to civilian government after a one-party regime; free and fair elections.
Tanzania (1995)	First post independence, internationally deemed free and fair elections.
Turkey (1983)	Fist free and fair legislative elections after a military dictatorship.
Ukraine (1991)	Independence from USSR. Legislative elections followed.
Zambia (1991)	First post independence, free and fair elections. New democratic constitution came into effect.

Panel C—"Borderline" Democratization Episodes

Country & Year	Brief Description
Central African Republic (1993)	First free election after the oppressive Bokassa rule. The 1996 presidential elections were deemed free, but marked by fraud allegations. Huge political instability is still present.
Iran, Islamic Republic of (1997)	First multi-candidate elections were held. Political reforms took place, but basic civil rights and political liberties have not been fully granted.
Jordan (1993)	Legalization of political parties and first free elections. Yet the political system is non competitive and fundamental political rights are not secured.
Niger (1999)*	Transition to civilian rule; presidential and legislative elections took place; although they were deemed fair and free, substantial political struggles are still present.
Pakistan (1988)	Legislative elections were held; restoration of the 1985 democratic constitution. In spite of fair and free elections in the nineties, the military coup of 1999 blocked democratization.

Table 1.1 reports the country, timing and a brief description of the democratization events. Panel A gives "Full Democratization" incidents; in these countries democratic institutions have been fully consolidated according both to the Polity and Freedom House indicators. Panel B gives "Partial Democratization"; in these countries a substantial democratic progress has occurred, but either of the two measures does not indicate a perfect democracy level. Panel C gives "Borderline Democratization" incidents; in these countries, democratic progress (reflected in either index) has taken place, but protection level of civil liberties is still very low. For details on our event identification methodology see Section 1.3. The Democratization Dataset Appendix A gives a more detailed description of the democratization events. * indicate late democratization episodes (after 1998) that are not considered in some of our regression estimates, due to limited data availability in the democratic period.

Table 3.2: Summary Statistics

Variable	All Episodes			"Full" Democratization			"Partial" Democratization		
	Pre-Democratization	Post-Democartization	Equality Test	Pre-Democratization	Post-Democartization	Equality Test	Pre-Democratization	Post-Democartization	Equality Test
Real GDP growth (3-year)	-0.0025	-0.0002	0.0023	0.0016	0.0027	0.0011	-0.0070	-0.0010	0.0060
std.	*0.06*	*0.06*	*0.71*	*0.06*	*0.07*	*0.90*	*0.06*	*0.05*	*0.42*
Real GDP growth (5-year)	0.0032	0.0045	0.0014	0.0120	0.0075	-0.0044	-0.0059	0.0006	0.0065
std.	*0.06*	*0.06*	*0.76*	*0.05*	*0.06*	*0.46*	*0.06*	*0.05*	*0.30*
Real GDP growth (10-year)	0.0071	0.0088	0.0017	0.0162	0.0113	-0.0049	-0.0036	0.0044	0.0080 *
std.	*0.06*	*0.05*	*0.60*	*0.05*	*0.05*	*0.25*	*0.06*	*0.00*	*0.10*
Investment	22.22	22.97	0.75	24.32	24.39	0.07	19.64	19.83	0.18
std.	*9.64*	*8.44*	*0.17*	*9.34*	*7.80*	*0.91*	*10.05*	*9.39*	*0.38*
Government Consumption	14.76	14.69	-0.08	14.64	14.91	0.27	14.92	14.30	-0.61
std.	*14.16*	*14.19*	*0.85*	*6.94*	*5.31*	*0.58*	*7.71*	*7.08*	*0.38*
Trade Openness	57.04	69.90	12.86 ***	59.58	74.77	15.19 ***	53.94	61.47	7.53 ***
std.	*30.88*	*38.64*	*0.00*	*30.57*	*42.31*	*0.00*	*31.02*	*29.53*	*0.01*
Schooling	4.62	5.67	1.05 ***	5.50	6.34	0.84 ***	3.35	4.33	0.97 ***
std.	*2.32*	*2.50*	*0.00*	*2.34*	*2.42*	*0.00*	*1.59*	*2.10*	*0.00*
Life Expectancy	61.20	65.19	3.98 ***	64.96	67.97	3.01 ***	55.96	59.99	4.03 ***
std.	*9.05*	*9.31*	*0.00*	*7.80*	*7.26*	*0.00*	*8.02*	*10.45*	*0.00*
Population Growth	0.0195	0.0141	-0.0054 ***	0.0147	0.0111	-0.0036 ***	0.0275	0.0194	-0.0081 ***
std.	*0.02*	*0.01*	*0.00*	*0.01*	*0.01*	*0.00*	*0.01*	*0.01*	*0.00*

The table reports the mean and standard deviation (in italics) of real per capita GDP growth and the other control variables employed in the growth regressions in the ten years before and the ten years after a permanent democratization event (the transition year is included in the Post-Democratization period). We also explore the 3 and 5 year averages of the growth rate of real per capita GDP. The table also reports a mean equality test. The mean difference between "before" and "after" years and the p-values (in italics) of a mean equality (assuming unequal variances before and after) is reported. Statistical significance is denoted by a * for 10%, ** for 5%, and *** for 1%. For some countries, we do not have a full 3, 5, or 10 years available given the timing of democratization, so we just take the available years in the average. For all variables, the summary statistics reflect data from 67 countries from 1960 to 2000. For schooling the summary statistics reflect data from 45 countries. "Full" (N=38) and "Partial", including "Borderline" (N=29) permanent democratization countries are given in Table 1.1. For details on our event identification methodology see Section 1.3.
The Democratization Dataset Appendix A gives a more detailed description of the democratization events. For variable sources and definitions see the Data Appendix B.1.

Table 3.3: The Uunconditional Effect Of Political Liberalization

Dep. Variable: Real GDP Growth	Pooled Cross-Section Time-Series OLS (1)	Time and Country Fixed Effects (2)
All Democratization Countries		
Democratization Coeff.	0.0003	0.0066
p-value	*0.874*	*0.004*
p-value (clustering)		*0.036*
Observations	2040	2040
Countries	67	67
adj. R squared	0.0001	0.0958
All Democratization Countries		
(excl. recent and "partial borderline")		
Democratization Coeff.	-0.0005	0.0072
p-value	*0.802*	*0.003*
p-value (clustering)		*0.035*
Observations	1726	1726
Countries	57	57
adj. R squared	0.0001	0.0974
"Full" Democratization Countries		
Democratization Coeff.	-0.0005	0.0038
p-value	*0.026*	*0.188*
p-value (clustering)		*0.349*
Observations	1153	1153
Countries	38	38
adj. R squared	0.0051	0.0786
"Partial" Democratization Countries		
(excl. "borderline partial")	0.0025	0.0140
p-value	*0.516*	*0.001*
p-value (clustering)		*0.010*
Observations	716	716
Countries	24	24
adj. R squared	0.0002	0.0867
"Partial" Democratization Countries		
Democratization Coeff.	0.0026	0.0113
p-value	*0.476*	*0.003*
p-value (clustering)		*0.020*
Observations	887	887
Countries	29	29
adj. R squared	0.0004	0.0788

The dependent variable is the annual real per capita GDP growth. Estimation is performed with OLS. In column (2) we estimate specification (1.3) adding both "time" and "country" fixed effects. P-values based on heteroskedasticity-adjusted standard errors are reported in italics (above). The Table also reports p-values based on standard errors adjusted for heteroskedasticity clustered by country (below). The democratization dummy takes the value one in the year and all subsequent years of a permanent democratization episode. For a list of the democratization episodes and their categorization to “full”, “partial” and “borderline" see Table 1.1. For details on our event identification methodology see Section 1.3 and the Democratization Dataset Appendix A.

Table 3.4: Benchmark Dynamic Specification

	Pooled Cross Section		Within			
	OLS	IV	All countries	IV	20 years obs	
	(1)	(2)	(3)	(4)	(5)	(6)
Lag (1) Growth	0.2775	0.3939	0.2056	0.2617	0.1931	0.1860
	0.000	*0.008*	*0.000*	*0.000*	*0.000*	*0.000*
Lag (2) Growth	-0.0197	-0.0137	-0.0665	-0.0104	-0.0646	-0.0280
	0.554	*0.757*	*0.044*	*0.779*	*0.059*	*0.252*
Lag (3) Growth	0.0249	0.0418	-0.0431	-0.0301		0.0038
	0.354	*0.204*	*0.127*	*0.339*		*0.856*
Investment	0.0006	-0.0004	0.0006	0.0001	0.0007	
	0.000	*0.687*	*0.028*	*0.927*	*0.012*	
Differenced Investment	0.0031	-0.0028	0.0031	0.0090	0.0029	
	0.000	*0.573*	*0.000*	*0.000*	*0.000*	
Lag (1) Diff. Investmemt	0.0004	-0.0014	0.0006	-0.0042	0.0006	
	0.336	*0.721*	*0.135*	*0.024*	*0.167*	
Lag (2) Diff. Investment	0.0002	-0.0007	0.0005	0.0009	0.0005	
	0.576	*0.493*	*0.157*	*0.109*	*0.184*	
Lag (3) Diff. Investment	0.0004	-0.0003	0.0007	0.0004		
	0.148	*0.733*	*0.024*	*0.405*		
Democratization	0.0047	0.0066	0.0064	0.0057	0.0062	0.0084
	0.029	*0.022*	*0.008*	*0.084*	*0.010*	*0.003*
a1+a2+a3-1	-0.7174	-0.5780	-0.9040	-0.7789	-0.8714	-0.8382
	[0.000]	*[0.000]*	*[0.001]*	*[0.000]*	*[0.000]*	*[0.000]*
Long-run Effect of Investment on Growth	0.0008	-0.0007	0.0006	0.0001	0.0008	
	[0.000]	*[0.718]*	*[0.023]*	*[0.899]*	*[0.023]*	
Long-run Effect of Investment on Income Level	0.0056	-0.0090	0.0053	0.0078	0.0046	
	[0.000]	*[0.609]*	*[0.000]*	*[0.184]*	*[0.0000]*	
Long-run Effect of Democratization on Growth	0.007	0.011	0.007	0.007	0.007	0.010
	[0.000]	*[0.094]*	*[0.008]*	*[0.100]*	*[0.000]*	*[0.000]*
R-sq	0.1933		0.2171		0.207	0.147
Countries	67	67	67	67	45	112
Observations	1764	1699	1764	1605	1611	3969
Test of first order auto-corr.	*[0.958]*	*[0.691]*	*[0.860]*	*[0.129]*	*[0.259]*	*[0.345]*
Test of Over-identification		*[0.198]*		*[0.4756]*		

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity and autocorrelation adjusted standard errors are reported in italics. Column (1) reports OLS estimates. Column (2) reports instrumental variables (IV) estimates. Column (3) reports fixed-effects (within) estimates. Column (4) reports fixed-effects (within) IV estimates. Columns (5) and (6) give fixed-effect estimates only for countries that we have at least 20 years of observations. In column (6) we exclude former socialist countries, replace the time fixed-effects with a linear trend and performe the estimation in all countries (reforming and non-reforming). In columns (2) and (4) the contemporaneous investment level, the contemporaneous differenced and the lagged differenced investment rate are being instrumented. The instrument set in column (2) includes: lag 4 and 5 of investment level, and lag 4 and 5 of the logarithm of income level. The instrument set in column (4) includes: all instruments used in specification reported in column (2) plus lag 2 and 3 of inflation rate, lag 2 and 3 of trade openness, and lag 2 and 3 of government consumption.

The table also reports a test and in square brackets p-values of the significance of: 1) the long run effect of investment on growth, 2) the long-run effect of investment on output level, 3) the long-run effect of a permanent democratization on growth, 4) that the sum of the coefficients of the autoregressive growth terms is one. It also reports a test of first order serial correlation. P-values (in square brackets) of the significance are given. Under the null hypothesis there is no serial correlation. In the last row the p-value of a Sargan test of over-identifying restrictions is reported. Rejection of the null hypothesis casts doubt on the validity of the instruments. The democratization dummy takes the value of one in the year and all subsequent years of a permanent democratization transition. For details on our event identification methodology see Section 1.3. The Democratization Dataset Appendix A gives a more detailed description of the democratization events. For variable definitions and sources see the Data Appendix B.1.

TABLE 3.5—TIMING OF THE EFFECT OF DEMOCRATIZATION FIXED EFFECT ESTIMATES

	All	Excl. Recent, Borderline & New Independent States		At least 20 years of observations		Transition & Non-Reformers
	(1)	(2)	(3)	(4)	(5)	(6)
Lag (1) Growth	0.2370	0.2097	0.2211	0.2147	0.2279	0.1722
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Lag (2) Growth	-0.1004	-0.1208	-0.0966	-0.0818	-0.0522	-0.0246
	0.001	*0.000*	*0.003*	*0.016*	*0.095*	*0.301*
Lag (1) Investment	0.0003	0.0005		0.0001		
	0.406	*0.272*		*0.384*		
Lag (2) Investment	0.0001	0.0001		0.0001		
	0.805	*0.889*		*0.795*		
Lag (3) Ln GDP	-0.0397	-0.0302	-0.0262	-0.0337	-0.0298	
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	
D^1	0.0088	0.0099	0.0092	0.0081	0.0071	0.0066
	0.019	*0.012*	*0.013*	*0.042*	*0.060*	*0.150*
D^2	0.0009	0.0007	-0.0000	-0.0000	-0.0003	-0.00288
	0.998	*0.882*	*0.995*	*0.681*	*0.938*	*0.555*
D^3	0.0079	0.0144	0.0133	0.0080	0.0054	0.0115
	0.057	*0.000*	*0.001*	*0.082*	*0.256*	*0.005*
D^4	0.0093	0.0062	0.0045	0.0081	0.0047	0.00631
	0.009	*0.114*	*0.224*	*0.032*	*0.183*	*0.154*
D^5	0.0165	0.0127	0.0103	0.01282	0.0093	0.0085
	0.000	*0.007*	*0.024*	*0.009*	*0.050*	*0.112*
D^6	0.0162	0.0150	0.0111	0.01484	0.0104	0.0095
	0.000	*0.001*	*0.006*	*0.001*	*0.009*	*0.048*
R-squared	0.175	0.147	0.154	0.155	0.153	0.1423
Countries	67	52	52	48	48	112
Observations	1857	1590	1665	1628	1728	4082
Time-Trend	No	No	No	No	No	Yes

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. Estimation corresponds to specification of equation (5) and is performed with panel fixed effects. Column (1) performs the estimation to the full (partial, full, and borderline) sample of democratization countries. Columns (2) and (3) exclude countries with a "borderline" episode, countries that experienced a democratization episode after 1998 (where not enough post democratization observations are available) and countries where democratization occurred alongside independence from former Czechoslovakia, U.S.S.R. and Yugoslavia. Column (4) and (5) give estimates only for countries that we have at least 20 years of observations (to minimize the bias arising from the joint presence of individual country effects the lagged dependent variable). In column (6) estimation is performed to all countries (democratization and non-reforming countries). Socialist countries and nations with less than 20 years of observations are excluded, while the time nuisance parameters are replaced with a linear trend.

D^1 is a dummy variable that takes on the value one in the fifth, fourth, and third pre democratization year and zero otherwise. D^2 is a dummy variable that takes on the value one in the second, first pre-democratization year and on the transition year and zero otherwise. D^3 takes on the value one in the first, second and third post democratization year and zero otherwise. D^4 takes on the value one in the fourth, fifth, and sixth post democratization year and zero otherwise. D^5 takes the value one in the seventh, eighth, and ninth post democratization year and zero otherwise. D^6 takes on the value one in the tenth and all subsequent (eleventh, twelfth, etc.) post democratization year and zero otherwise. For a list of the democratization episodes see Table 1. For details on our event identification methodology see Section III. The *Supplementary Democratization Dataset Appendix* gives a more detailed description of the democratization events. For variable definitions and sources see *the Data Appendix*.

Table 3.6: Democratization In a Cross-Country Growth Regression Framework

	OLS		Within (all)		Within (20 obs. years)	
	simple (1)	policy (2)	simple (3)	policy (4)	simple (5)	policy (6)
Lag (1) Growth	0.3046	0.3148	0.2213	0.2260	0.2127	0.1935
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Lag (2) Growth	0.0077	-0.0278	-0.0657	-0.0993	-0.0599	-0.0953
	0.828	*0.383*	*0.072*	*0.002*	*0.110*	*0.006*
Lag (3) Ln GDP level	-0.0015	-0.0041	-0.0360	-0.0406	-0.0349	-0.0350
	0.341	*0.022*	*0.000*	*0.000*	*0.000*	*0.000*
Lag (1) Investment	0.0162	-0.0002	0.0358	0.0080	0.0004	0.0002
	0.739	*0.996*	*0.488*	*0.844*	*0.453*	*0.740*
Lag (2) Investment	-0.0079	-0.0140	-0.0014	-0.0131	-0.0001	-0.0001
	0.899	*0.767*	*0.982*	*0.774*	*0.845*	*0.799*
Lag (3) Investment	0.0177	0.0167	0.0353	0.0248	0.0003	0.0001
	0.656	*0.609*	*0.391*	*0.458*	*0.418*	*0.782*
Lag (1) Population Growth	-0.1285	-0.1189	-0.1415	-0.2418	-0.2423	-0.5600
	0.300	*0.378*	*0.387*	*0.151*	*0.454*	*0.024*
Lag (1) Schooling	0.0013		0.0049		0.0051	
	0.120		*0.094*		*0.079*	
Lag (1) Life Expectancy		0.0498		0.0819		0.0977
		0.000		*0.002*		*0.000*
Lag (1) Government Consumption		-0.1233		-0.1535		-0.0019
		0.000		*0.001*		*0.000*
Lag (1) Trade		0.0086		0.0217		0.0002
		0.161		*0.031*		*0.087*
Democratization	0.005	0.004	0.009	0.009	0.0097	0.0077
	0.037	*0.081*	*0.001*	*0.001*	*0.000*	*0.003*
Long-run Effect of Democratization on Income Level	*3.370*	*0.971*	*0.264*	*0.228*	*0.278*	*0.220*
	[0.346]	*[0.171]*	*[0.001]*	*[0.000]*	*[0.001]*	*[0.002]*
R-sq	0.12	0.15	0.16	0.190	0.4560	0.1780
Countries	45	67	45	67	41	48
Observations	1453	1791	1453	1791	1388	1575

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. All control variables are one year lagged. Columns (1) and (2) report OLS estimates. Columns (3)-(6) report fixed-effects (within) estimates. In columns (5) and (6) we exclude all countries, for which less than 20 years of observations are available. The table also reports a test and in square brackets p-values of the significance of the long run effect of a permanent democratization on output level. The democratization variable takes on the value one in the year and all subsequent years of a permanent democratization transition. For details on our event identification methodology see Section 1.3. The *Democratization Dataset Appendix A* gives a more detailed description of the democratization events. For variable definitions and sources see the *Data Appendix B.1* .

Table 3.7: Fixed Effects Dynamic Panel Cross-Country Growth Regressions With Transition Effects & Additional Ccontrols

	Full Sample						Excl.Borderline & Recent Episodes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lag (1) Growth	0.2520	0.2395	0.2387	0.2352	0.2345	0.2284	0.2646	0.2524	0.2515	0.2405
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Lag (2) Growth	-0.0694	-0.0930	-0.0921	-0.0960	-0.0947	-0.0894	-0.0829	-0.1061	-0.1055	-0.1004
	0.017	*0.003*	*0.010*	*0.002*	*0.003*	*0.006*	*0.007*	*0.001*	*0.002*	*0.003*
Lag (3) Ln GDP Level	-0.0348	-0.0381	-0.0388	-0.0391	-0.0389	-0.0409	-0.0330	-0.0355	-0.0349	-0.0376
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Lag (1) Investment		0.0004	0.0004	0.0003	0.0002	-0.0002		0.0002	0.0001	0.0000
		0.098	*0.092*	*0.231*	*0.310*	*0.522*		*0.471*	*0.625*	*-0.290*
Lag (1) Population Growth			-0.1946	-0.2633	-0.2748	-0.2013		-0.3959	-0.4156	0.2690
			0.550	*0.427*	*0.414*	*0.539*		*0.096*	*0.083*	*0.068*
Lag (1) Life Expectancy				0.1068	0.1000	0.0967		0.0881	0.0764	0.2310
				0.000	*0.000*	*0.001*		*0.002*	*0.008*	*0.017*
Lag (1) Government Consumption					-0.0003	-0.0004			-0.0004	-0.0004
					0.497	*0.424*			*0.427*	*0.448*
Lag (1) Trade						0.0004				0.0005
						0.000				*0.000*
Transition dummy	-0.0005	0.0010	0.0012	-0.0002	-0.0002	0.0015	-0.0011	0.0001	0.0002	0.0020
	0.885	*0.767*	*0.722*	*0.948*	*0.966*	*0.677*	*0.777*	*0.973*	*0.959*	*0.596*
Post Democratization dummy	0.0071	0.0100	0.0101	0.0088	0.0081	0.0092	0.0072	0.0093	0.0085	0.0096
	0.004	*0.000*	*0.000*	*0.001*	*0.005*	*0.003*	*0.005*	*0.001*	*0.004*	*0.003*
R-sq	0.177	0.175	0.175	0.180	0.178	0.194	0.176	0.182	0.180	0.203
Countries	67	67	67	67	67	67	57	57	57	57
Observations	1978	1870	1870	1870	1832	1811	1684	1582	1544	1528
Test of first order serial correlation	*[0.770]*	*[0.913]*	*[0.901]*	*[0.951]*	*[0.862]*	*[0.759]*	*[0.755]*	*[0.900]*	*[0.785]*	*[0.664]*

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. Estimation is performed with panel (country) fixed effects (within). In columns (1) to (6) estimation is performed to all 67 countries that experienced a permanent democratization. In columns (7) to (10) we exclude countries with "borderline" democratization and countries that experienced a democratization episode after 1998 (where not enough post democratization observations are available). The transition dummy takes on the value one in the five years surrounding a permanent democratic transition.

The Post democratization dummy takes on the value one in the third post democratization and all subsequent democratic years. All independent variables are one-year lagged. The table also reports p-values (in square brackets) of a test for first-order residual correlation. Under the null hypothesis there is no serial correlation. For details on our event identification methodology see Section 1.3. The Democratization Dataset Appendix A gives a more detailed description of the democratization events. For variable definitions and sources see the Data Appendix B.1.

Table 3.8: Endogeneity

	All (Full & Partial) Democartizations			At least 20 years of observations				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag (1) Growth	0.1929	0.1748	0.1695		0.1673	0.1610	0.2330	0.1541
	0.000	*0.000*	*0.000*		*0.000*	*0.000*	*0.000*	*0.000*
Lag (2) Growth	-0.0559	-0.0829	-0.0784552		-0.0552	-0.0653	-0.0693	-0.0646
	0.042	*0.000*	*0.006*		*0.003*	*0.028*	*0.077*	*0.041*
Lag (3) Ln GDP Level	-0.0518	-0.0587	-0.0621		-0.0433	-0.0477	-0.0514	-0.0540
	0.000	*0.000*	*0.000*		*0.000*	*0.000*	*0.000*	*0.000*
Lag (1) Investment		0.0007	0.0002			0.0004	-0.0001	0.0000
		0.000	*0.457*			*0.117*	*0.800*	*0.284*
Lag (1) Population Growth		0.0264	-0.3447			-0.3995	0.0786	-0.3596
		0.001	*0.283*			*0.235*	*0.736*	*0.969*
Lag (1) Ln. Life Expectancy		0.0802	0.0894			0.0881	0.0869	0.0918
		0.002	*0.021*			*0.020*	*0.013*	*0.026*
Lag (1) Government Consumption			-0.0005				-0.0004	-0.0005
			0.197				*0.331*	*0.240*
Lag (1) Trade			0.0005				0.0004	0.0004
			0.000				*0.001*	*0.000*
Democratization	0.0172	0.0268	0.0254	0.0169	0.0203	0.0219	0.0203	0.0177
	0.100	*0.004*	*0.010*	*0.050*	*0.012*	*0.049*	*0.017*	*0.068*
Selection Estimation (Heckman)	ML	two-step	ML	ML	two-step	ML	ML	ML
2nd stage indep. variables	Independ. Years Trend Religion	Independ. Years Trend Religion Region Dummies	Independ. Years Trend Religion	Independ. Years Trend Region Dummies	Independ. Years Trend Religion Region Dummies	Independ. Years Trend Religion Fractionalization	Independ. Years Trend Religion Fractionalization Openness	Independ. Years Trend & Region Religion Fractionalization Income & Oil
Rho	-0.13 (0.122)	-0.237	-0.233 (0.120)	-0.136 (0.092)	-0.1678	-0.171 (0.144)	-0.195 (0.112)	-0.138 (0.126)
Equation independence [p-value]	[0.2916]		[0.0473]	[0.1430]		[0.2457]	[0.0881]	[0.279]
Observations	3140	2986	2902	2792	2665	2550	1854	2483

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. The democratization variable takes on the value of one in the year and all subsequent years of a permanent democratization transition. In all specifications, it is treated as endogenous. In columns (3)--(8) we exclude all countries, for which less than 20 years of observations are available. The table reports diagnostics of the first-stage selection (treatment) equation. ML stands for maximum likelihood estimation (probit) and 2-step stands for Heckman's efficient two-step procedure. Depending on the specification, the democratization indicator variable is predicted in the treatment equation with: Religion that includes the share of Muslim and Confucian in the population. Fractionalization that includes proxies of religious and ethnical heterogeneity. Oil is a dummy variable for oil-producing nations. Indep. Years measures years since independence. Income is the 3-year lag value of the logarithm of per capita GDP. A linear time trend, which controls for the trending behavior of the democratization variable and some of the selection equation regressors. For variable definitions and source Rho is the estimated correlation coefficient (and its standard error in parenthesis) between the residuals of the growth regression (second stage) and the democratization selection equation (first stage). The table also reports p-values of a Wald test of equation independence. Under the null hypothesis the first and second stage equations are independent.

Table 3.9: Measurement Error & Alternative Democratization Episodes

	Excluding Recent and Borderline			Freedom House			Polity		
	OLS	Within	Within with Control Group	OLS	Within	Within with Control Group	OLS	Within	Within with Control Group
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag (1) Growth	0.3528	0.2539	0.1557	0.2642	0.1718	0.1289	0.2100	0.1241	0.1517
	0.000	*0.000*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Lag (2) Growth	-0.0180	-0.1025	-0.0362	-0.0338	*-0.1113*	-0.0570	-0.0228	-0.0980	-0.0368
	0.589	*0.003*	*0.13*	*0.31*	*0.00*	*0.04*	*0.48*	*0.00*	*0.14*
Lag (3) Ln GDP level	0.0020	-0.0373	-0.0287	0.0027	-0.0487	-0.0460	0.0022	-0.0383	-0.0310
	0.056	*0.000*	*0.00*	*0.02*	*0.00*	*0.00*	*0.04*	*0.00*	*0.00*
Lag Investment	0.0077	0.0305		0.0448	*0.0600*		0.0504	0.0810	
	0.615	*0.204*		*0.005*	*0.060*		*0.006*	*0.003*	
Lag (1) Differenced Investment	-0.0128	-0.0198		-0.0120	-0.0217		0.0250	0.0035	
	0.745	*0.613*		*0.755*	*0.599*		*0.527*	*0.933*	
Lag (2) Differenced Investment	-0.0252	-0.0302		-0.0116	*-0.0184*		-0.0051	-0.0218	
	0.452	*0.354*		*0.715*	*0.587*		*0.880*	*0.531*	
Democratization	0.0047	0.0113	0.0053	0.0032	0.0088	-0.0001	0.0052	0.0100	0.0053
	0.046	*0.000*	*0.07*	*0.26*	*0.01*	*0.99*	*0.03*	*0.00*	*0.08*
R-sq	0.13	0.18	0.16	0.10	0.14	0.17	0.07	0.13	0.16
Countries	57	57	112	53	53	112	61	61	103
Observations	1544	1544	4082	1264	1264	3193	1815	1815	3736

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. Columns (1), (4) and (7) report OLS estimates. Columns (2), (5) and (8) report fixed-effects (within) estimates. Columns (3), (6) and (9) report fixed-effects (within) estimates, when we exclude countries with less than 20 years of observations, former socialist countries, but bring in the system non-reforming countries as control group. In columns (3), (6), and (9) we replace the time nuisance fixed parameters with a linear time trend. Columns (1), (2) and (3) exclude countries with a "borderline" episode and countries that experienced democratization after 1998 (where not enough post democratization observations are available). In columns (4), (5) and (6) a democratization is defined solely according to a permanent jump of Freedom House characterization. In columns (7), (8) and (9) a democratization is defined solely according to a permanent jump of the Polity 21-scaled democracy indicator. For more details on construction of the democratization dummy and the serial correlation see notes in Table 1.4.

Table 3.10: Dynamic Growth Regressions With Non-Reformers Aa Control Group & Time Trends

	Average Effect		Regional Effects		Intensity of Reforms		High vs. Low Schooling	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag (1) Growth	0.1562	0.1828	0.1547	0.1804	0.1561	0.1827	0.1364	0.1561
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.001*	*0.000*
Lag (2) Growth	-0.0534	-0.0305	-0.0543	-0.0320	-0.0535	-0.0306	-0.0143	0.0022
	0.034	*0.215*	*0.031*	*0.194*	*0.033*	*0.214*	*0.577*	*0.932*
Lag (3) Growth	-0.0178	0.0000	-0.0193	-0.0019	-0.0178	-0.0003	-0.0136	-0.0005
	0.400	*0.999*	*0.363*	*0.927*	*0.400*	*0.990*	*0.541*	*0.983*
Lag (4) Ln GDP Level	-0.0408		-0.0405		-0.0410		-0.0330	
	0.000		*0.000*		*0.000*		*0.000*	
Democratization	0.0060	0.0073						
	0.051	*0.022*						
Democratization - Other			0.0100	0.0115				
			0.028	*0.015*				
Democartization - Latin America			0.0073	0.0103				
			0.144	*0.043*				
Democartization - Europe			-0.0119	-0.0169				
			0.099	*0.025*				
Democratization - Full					0.0069	0.0053		
					0.054	*0.155*		
Democratization - Partial					0.0041	0.0116		
					0.410	*0.026*		
Democratization - HighSchooling							0.0103	0.0102
							0.006	*0.009*
Democratization - LowSchooling							-0.0068	-0.0043
							0.123	*0.368*
Trends	Regional	Reg. & Income	Regional	Reg. & Income	Regional	Reg. & Income	Regional	Reg. & Income
R-sq	0.1727	0.1477	0.1731	0.1486	0.1725	0.1477	0.1765	0.1578
Countries	112	112	112	112	112	112	90	90
Observations	3969	3969	3969	3969	3969	3969	3181	3181

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. Estimation is performed to all countries, so countries without a democratization (either permanent democracies or permanent autocracies) serve as control group. Estimation is performed to countries where there are at least 20 years of observations. Former socialist republics and countries are exluded from the estimation. The democratization dummy takes on the value one in the year and all subsequent post democratization years. In columns (3)-(4) we allow the democratization dummy to differ between Latin America, EUl 5 and the other reform countries. In columns (5)-(6) we allow the democratization variable to differ depending on whether a country experienced a "full" or a "partial" democratization. In columns (7)-(8) we allow the democartization dummy to differ between reforming countries with above and below median value of avergae years of schooling. Models (1), (3), (5) and (7) include six regional and a high income time trend. Models (2), (4), (6), and (8) include six regional and four income-level trends.

Table 3.11: Alternative Estimation Techniques Without Dynamics

Estimation Method:	**OLS**		**Random Effects**		**Fixed Effects**		**Arellano & Bond**		**Arellano & Bond System**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Initial Log GDP Level	-0.0002	-0.0035	-0.0038	-0.0104	-0.0323	-0.0405	-0.0731	-0.1211	-0.0477	-0.0373
	0.899	*0.056*	*0.170*	*0.000*	*0.000*	*0.000*	*0.168*	*0.019*	*0.241*	*0.135*
Investment	0.0008	0.0004	0.0007	0.0000	0.0010	0.0002	-0.0018	-0.0019	0.0004	-0.0005
	0.000	*0.055*	*0.000*	*0.915*	*0.000*	*0.290*	*0.009*	*0.000*	*0.605*	*0.421*
Population Growth	0.0140	0.0425	0.2300	0.0407	0.3214	-0.2350	1.8653	0.3758	1.6453	0.0999
	0.943	*0.794*	*0.263*	*0.808*	*0.407*	*0.413*	*0.001*	*0.539*	*0.019*	*0.861*
Schooling	0.0011		0.0024		0.0047		-0.0042		0.0206	
	0.253		*0.080*		*0.093*		*0.608*		*0.060*	
Life Expectancy		0.0569		0.0899		0.0821		0.1794		0.2639
		0.000		*0.000*		*0.003*		*0.114*		*0.021*
Government Consumption		-0.0011		-0.0009		-0.0006		0.0008		-0.0009
		0.000		*0.001*		*0.215*		*0.342*		*0.394*
Trade		0.0001		0.0002		0.0004		0.0005		0.0005
		0.078		*0.001*		*0.000*		*0.012*		*0.029*
Democratization	0.0066	0.0039	0.0070	0.0054	0.0101	0.0106	0.0223	0.0288	0.0215	0.0159
	0.008	*0.095*	*0.013*	*0.041*	*0.000*	*0.000*	0.057	0.005	0.007	0.053
adj. R-squred	0.0481	0.0504			0.1009	0.1266				
Countries	45	67	45	67	45	67	45	67	45	67
Observations	1499	1867	1499	1867	1499	1867	1455	1797	1456	1801
Test of first-order serial correlation	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Test of second-order serial correlation							*[0.067]*	*[0.023]*	*[0.043]*	*[0.021]*
Over-identification test							*[0.683]*	*[0.325]*	*[0.416]*	*[0.999]*

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. All control variables are one year lagged. Columns (1) and (2) report OLS estimates. Columns (3) and (4) report random-effects estimates. Columns (5) and (6) report fixed-effect (within) estimates. OLS, Fixed and Random Effect specifications also include a constant term (not reported). Columns (7) and (8) report the one-step GMM estimates based on the Arellano and Bond (1991) difference procedure. Columns (9) and (10) report system two-step GMM estimates based on the Arellano and Bover (1995) and Blundel and Bond (1998) procedure. Standard errors are corrected with the procedure suggested by Windmeijer (2004).

The table also reports a test of first and second-order (for the GMM methods) order residual serial correlation. P-values (in square brackets) of the significance are given. Under the null hypothesis there is no serial correlation. The table also reports a Hansen-Sargan test of over-identifying restrictions. Under the null of instrument validity it is distributed as chi-square. P-values of both tests are reported in square brackets. The democratization dummy takes the value of one in the year and all subsequent years of a permanent democratization transition. For details on our event identification methodology see Section 1.3. The Democratization Dataset Appendix A gives a more detailed description of the democratization events. For variable definitions and sources see the Data Appendix B.1.

Table 3.12: Dynamic Panel GMM Techniques

	Arellano-Bond Difference		Arellano-Bond System		
	(1)	(2)	(3)	(4)	(5)
Lag (1) Growth	0.0772	0.1953	0.2729	0.2677	0.3286
	0.567	*0.078*	*0.064*	*0.000*	*0.001*
Lag (2) Growth	-0.1882	-0.2347	-0.0192	-0.0556	-0.0968
	0.029	*0.001*	*0.768*	*0.181*	*0.036*
Lag (3) Ln GDP level	-0.0968	-0.1164		-0.0093	-0.0077
	0.040	*0.001*		*0.250*	*0.597*
Investment		-0.0005		0.0000	-0.0004
		0.511		*0.957*	*0.541*
Population Growth		-0.2579		0.1286	-0.2435
		0.769		*0.801*	*0.752*
Life Expectancy		0.2191		0.1457	0.2240
		0.139		*0.026*	*0.080*
Government Consumption		-0.0003		-0.0014	-0.0020
		0.860		*0.096*	*0.068*
Trade		0.0002		0.0003	0.0002
		0.584		*0.057*	*0.649*
Democratization	0.0128	0.0176	0.0135	0.0081	0.0147
	0.092	*0.101*	*0.216*	*0.133*	*0.044*
Countries	57	57	57	57	57
Observations	1567	1472	1624	1532	1532
Test of first order serial correlation	-3.77	-3.81	-2.66	-2.62	-4.38
	[0.000]	[0.000]	[0.008]	[0.010]	[0.000]
Test of second order serial correlation	0.09	0.85	-0.46	-0.18	0.50
	[0.928]	[0.393]	[0.642]	[0.855]	[0.615]
Test of Over-identification [p-value]	0.87	47.32	40.93	44.13	41.76
	[1.000]	[1.000]	[0.302]	[1.000]	[1.000]
Procedure	onestep	twostep	twostep	onestep	twostep

The dependent variable is the annual real per capita GDP growth. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. Columns (1) and (2) report GMM estimates based on the Arellano and Bond (1991) difference procedure. Columns (3), (4) and (5) report system GMM estimates based on the Arellano and Bover (1995) and Blundell and Bond (1998) procedure. All control variables are treated as endogenous and are being instrumented with further lag values. The last row states whether the one or the two step method is used. In the two-step estimates standard errors are corrected with the procedure suggested by Windmeijer (2004). The table also reports a test of first and second-order order residual serial correlation. P-values (in square brackets) of the significance are given. Under the null hypothesis there is no serial correlation. The table also reports a Hansen-Sargan test of over-identifying restrictions. Under the null of instrument validity it is distributed as chi-square. P-values of both tests are reported in square brackets. The GMM system specifications also include a constant term (not reported). The democratization dummy takes the value of one in the year and all subsequent years of a permanent democratization transition. For details on our event identification methodology see Section 1.3. The Democratization Dataset Appendix A gives a more detailed description of the democratization events. For variable definitions and sources see the Data Appendix B.1.

Table 4.1: Country sample

Panel A: Base Sample

Full Democratizations (A1)	Partial Democratizations (A2)	Always Authoritarian (A3)	Always Authoritarian (A4)
Argentina (1983)	Albania (1992)	Afghanistan [B/L]	Jordan [bord]
Benin (1991)	Bangladesh (1991)	Algeria	Kenya
Bolivia (1982)	Comoros (1990)	Angola [n/a]	Kuwait [B/L]
Brazil (1985)	Djibouti (1999) [n/a]	Arab Emirates [n/a]	Lao [n/a]
Bulgaria (1990)	Ethiopia (1995)	Bahrain [B/L]	Lebanon
Cape Verde (1991)	Ghana (1996)	Bhutan [n/a]	Liberia
Chile (1990)	Guatemala (1996)	Brunei [n/a]	Libya [n/a]
Dom. Rep. (1978)	Haiti (1994)	Burkina Faso	Morocco
Ecuador (1979)	Indonesia (1999)	Burundi	Myanmar
El Salvador (1994)	Lesotho (1993)	Cambodia	Niger [bord]
Greece (1975)	Madagascar (1993)	Cameroon	North Korea [n/a]
Grenada (1983)	Mozambique (1994)	Centr. Africa [bord]	Oman [n/a]
Guyana (1992)	Nepal (1991)	Congo, Dem. [B/L]	Pakistan [bord]
Honduras (1982)	Nicaragua (1990)	Congo, Rep.	Qatar [n/a]
Hungary (1990)	Nigeria (1999)	Cote d'Ivoire	Saudi Arabia [n/a]
Malawi (1994)	Paraguay (1993)	Cuba [B/L]	Sierra Leone [B/L]
Mali (1992)	Senegal (2000)	Egypt	Singapore
Mexico (1997)	Suriname (1991) [n/a]	Equatorial Guinea	Somalia
Mongolia (1992) [n/a]	Tanzania (1995)	Eritrea [n/a]	Sudan [B/L]
Panama (1994)	Turkey (1983)	Gabon	Togo
Peru (1980)	Zambia (1991)	Gambia	Tonga [n/a]
Philippines (1987)		Guinea-Bissau	Tunisia
Poland (1990)		Iran [bord]	Uganda
Portugal (1976)		Iraq [B/L]	Zimbabwe
Romania (1990)			Vietnam
Sao Tome (1991) [n/a]			
South Africa (1994)			
South Korea (1988)			
Spain (1978)			
Thailand (1992)			
Uruguay (1985)			

Panel B: New Independent States

Full and Partial Democratizations (B1)	Always Authoritarian (B2)
Full Democratizations	Armenia
	Azerbaijan
Croatia (2000)	Belarus
Czech Rep. (1993)	Bosnia [n/a]
Estonia (1991)	Georgia
Latvia (1991)	Kazakhstan
Lithuania (1991)	Kyrgyz Republic
Slovak Rep. (1993)	Moldova
Slovenia (1991)	Serbia-Montenegro
	Tajikistan
	[n] Turkmenistan [/a]
Partial Democratizations	Uzbekistan
FYROM (1991)	
Russia (1993)	
Ukraine (1991)	

Panel C: Always Democracies

(C)	
Australia	Israel
Austria	Italy
Bahamas	Jamaica
Belgium	Japan
Belize	Luxembourg
Botswana	Malta
Canada	Mauritius
Colombia	Namibia
Costa Rica	Netherlands
Cyprus	New Zealand
Denmark	Norway
Venezuela	Papua New Guinea
Finland	Sri Lanka
France	Sweden
Germany	Switzerland
Iceland	Trinidad & Tobago
India	United Kingdom
Ireland	United States

Panel A gives the base sample countries. Panel B reports the new independent states that followed the separation of Czechoslovakia, U.S.S.R., and Yugoslavia. For completeness Panel C reports the countries that are throughout the sample period stable democracies. Sample countries are categorized depending on whether they managed to permanently abandon autocratic ruling. There are three categories. Full democratizations are countries that permanently abandoned autocratic ruling and get an almost perfect score in civil rights and political liberties protection. Partial democratizations are countries that abandoned autocracy, but the level of civil rights protection is not perfect. Always authoritarian are those countries that are consistently throughout the sample period stable autocracies. For details on the sample and our democratization event identification methodology and for a brief description of the democratic transition see Appendix A. The year of democratic transition is given in parenthesis. [n/a] indicates that data is unavailable for both GDP and schooling. [B/L] indicates data unavailability for GDP, but availability on schooling statistics. [bord] indicates "borderline" democratization countries.

Table 4.2: Descriptive Analysis

The "modernization hypothesis" & Social Structure Theories

Panel A: By Income Level

Degree of Change in Democratic Institutions	Level of Economic Development (in 1975)				
	< 1,000 USD (19 count.)	< 2,000 USD (17 count.)	< 4,000 USD (20 count.)	< 6,000 USD (8 count.)	>6,000 USD (10 count.)
"Always Authoritarian"	Guinea-Bissau, Uganda, Burkina Faso, Burundi, Rwanda, China, Congo, Dem. Rep., Pakistan	Cameroon, Central African Republic, Chad, Congo, Rep., Egypt, Kenya, Mauritania, Sierra Leone, Togo	Algeria, Angola, Côte d'Ivoire, Equatorial Guinea, Guinea, Morocco, Syria, Tunisia, Zimbabwe	Iran,	Gabon, Singapore,
"Partial Democratic Transition"	Tanzania, Ethiopia, Nepal, Haiti, Bangladesh, Lesotho, Nigeria	Ghana, Indonesia, Madagascar, Mozambique, Senegal, Zambia	Comoros, Guatemala, Paraguay	Nicaragua, Turkey	
"Full Democratic Transition"	Malawi, Mali, São Tomé, Benin	Honduras, Cape Verde	Bolivia, Dominican Republic, Ecuador, Guyana, Korea, Philippines, Romania	Chile, Panama, El Salvador, Brazil, Peru,	Mexico, Uruguay, Hungary, Portugal, South Africa, Argentina, Greece, Spain

Panel B: By Schooling

Degree of Change in Democratic Institutions	Average Years of Schooling (in 1975)				
	< 1.5 years (14 count.)	< 2.5 years (15 count.)	< 3.5 years (14 count.)	< 5 years (16 count.)	>5 years (12 count.)
"Always Authoritarian"	United Arab Emirates Sudan, Togo, Afghanistan, Myanmar, Uganda, Rwanda, Niger	Sierra Leone, Congo, Egypt, Liberia, Iraq, Algeria, Libya, Zimbabwe, Iran Cameroon, Kenya, Tunisia, Pakistan	Syria, Bahrain, Kuwait	Jordan, Swaziland, China, Cuba	Singapore
"Partial" Democratic Transition"	Nepal, Mozambique, Haiti, Bangladesh	Guatemala, Senegal	Tanzania, Indonesia, Zambia, Nicaragua, Ghana, Turkey	Lesotho, Paraguay	Russia
"Full" Democratic Transition"	Mali, Benin		Malawi, Honduras Portugal, El Salvador, Brazil	Dominican Republic, Mexico, Thailand, South Africa, Guyana Ecuador, Peru, Bolivia, Spain	Panama, Chile, Philippines, Uruguay, Argentina, Korea, Bulgaria, Romania, Hungary, Poland

4.2

Table ~~2.2~~ (cont.)

The "modernization hypothesis" & Social Structure Theories (cont.)

Panel C: By Religious Fragmentation

Degree of Change in Democratic Institutions	Religious Heterogeneity				
	< 0.19 (25 count.)	< 0.35 (25 count.)	< 0.50 (25 count.)	< 0.65 (27 count.)	>0.65 (26 count.)
"Always Authoritarian"	Morocco, Algeria [O], Tunisia, Mauritania Libya [O], Qatar [O], Cambodia, Equatorial Guinea, Saudi Arabia [O], Jordan, Iran [O]	Niger, Myanmar, Egypt, Uzbekistan, Turkmenistan [O], Guinea, Afghanistan, United Arab Emirates [O], Tajikistan	Bhutan, Eritrea, Syria, Oman [O], Brunei [O] Swaziland, Kyrgyz Republic, Armenia, Iraq [O], Liberia, Korea, Dem., Azerbaijan, Pakistan	Cuba, Rwanda, Burundi, Sierra Leone, Laos, Bahrain [O], Moldova, Burkina Faso, Kazakhstan, Belarus, Guinea-Bissau, Tonga, Angola, Uganda, Chad	Georgia, Singapore, Togo, Congo, Rep [O]., China, Gabon [O], Kuwait [O], Bosnia and Herzegovina, Congo, Dem., Cameroon, Zimbabwe, Côte d'Ivoire, Kenya, Lebanon, Central African Republic
"Partial" Democratic Transition"	Turkey, Comoros Djibouti, Nepal, Senegal	Bangladesh, Paraguay, Indonesia [O]	Guatemala, Nicaragua, Russian Federation, Haiti, Albania	Madagascar, FYROM, Ukraine, Ethiopia, Tanzania	Mozambique, Lesotho, Zambia, Nigeria [O], Suriname Ghana
"Full Democratic Transition"	Cape Verde, Mongolia, Thailand Ecuador, Portugal, Greece	Poland, Mexico, Mali São Tomé, Peru, Bolivia, Argentina, Honduras, Romania, Slovenia, Philippines, Dominican Republic, Panama	Uruguay, El Salvador, Chile, Pakistan, Lithuania Croatia, Spain, Estonia	Hungary, Benin, Latvia, Slovakia, Grenada, Bulgaria, Brazil	Czech Republic, Korea, Rep, Guyana Malawi, South Africa

"Always authoritarian" are those countries that remained non-democratic throughout the sample period (1960-2000). "Partial Democratic Transition" countries moved form a non-democratic regime to democracy, but democratic institutions have not reached the highest level. "Full Democratic Transition" countries managed to move out of an autocratic regime and have fully consolidated democratic institutions. "Democratization" and "always authoritarian" countries are reported in Table 2.1. For the methodology in identifying "always authoritarian", "partial democratic transition" and "full democratic transition countries" see Appendix A.
Depending on data availability, in Panel A countries are split into five groups based on their level of per capita GDP in 1975 (before the Third Wave of Democratization began). GDP data come from the World Bank. In Panel B, the countries are split in five groups based on the schooling level in 1975. Educational attainment statistics come from Barro and Lee (2001). In Panel C countries are split based on the level of religious fragmentation. Fragmentation data are retrieved from Alesina *et al.* (2003). [O] indicates an oil exporting country (classification follows World Bank's World Economic Outlook or OPEC membership). For detailed variable definitions and sources see Appendix B.2.

Table 4.3: Descriptive statistics and tests of means

Variables	*Always* Authoritarian	*All* Democratization	*Full* Democratization	*Early* Democratization
GDP	2257.5	3375.08**	4427.5**	5553.55**
	(1838.8)	(2761.89)	(3008.2)	(3095.32)
Urbanization	0.341	0.3785	0.45**	0.506**
	(0.334)	(0.202)	(0.194)	(0.175)
Radios	127.62	177.11*	238.88***	284.25***
	(102.48)	(158.72)	(169.38)	(208.10)
Schooling	2.26	3.88***	4.648***	4.534***
	(1.29)	(2.06)	(2.03)	(1.40)
Literacy rate	0.5875	0.725*	0.831***	0.893***
	(0.2255)	(0.254)	(0.2205)	(0.897)
Ethnical Fragmentation	0.556	0.457*	0.409***	0.344***
	(0.26)	(0.26)	(0.23)	(0.22)
Religious Fragmentation	0.437	0.385	0.355	0.306**
	(41.96)	(0.248)	(0.229)	(0.19)
Muslim share	0.466	0.189***	0.052***	0.072***
	(41.96)	(32.6)	(16.4)	(25.6)
Confucian share	0.076	0.042	0.069	0.016*
	(0.226)	(0.182)	(0.24)	(0.062)
Trade/GDP	0.648	0.512	0.551	0.432**
	(0.513)	(0.283)	(0.288)	(0.20)
Trade openness	0.056	0.128	0.167	0.266
	(0.233)	(0.337)	(0.379)	(0.457)
Independence	0.157	0.411***	0.477***	0.611***
	(0.212)	(0.348)	(0.33)	(0.30)
Settler mortality	251.32	301.28	276.65	72.01***
	(186.27)	(655.22)	(767.23)	(2.68)
European Lang. Share	0.024	0.299***	0.457***	0.595***
	(0.131)	(0.40)	(0.428)	(0.409)
Gini coefficient	0.4126	0.4421	0.4417	0.46
(*around 1975*)	(0.1015)	(0.1178)	(0.1254)	(0.996)

Table 2.3 reports summary statistics of the main variables employed in the regression analysis. All variables values correspond to year 1975 (before the Third Wave of democratization began). Mean values and standard deviations are reported in parenthesis. Column (1) gives the mean and standard deviation in the "always autocratic" sub-sample (i.e. countries that remained throughout the sample autocratic). Column (2) gives descriptive statistics in the democratization event countries. Column (3) gives the mean and standard deviation in countries that experienced a full consolidation of democratic institutions ("Full Democratizations"). Column (4) gives the mean and standard deviation of countries that democratized before 1990 ("Early Democratizations"). *, **, *** indicate that mean values between the always autocratic countries and each democratization group are statistically different from each other at the 10%, 5%, and 1% level respectively (assuming unequal variances). For the country classification, see Table 2.1. For details on the democratization event identification methodology and a brief description of the democratic transition see Appendix A. For detailed variable definitions and sources see Appendix B.2.

Table 4.4: The modernization hypothesis

	All Democr.	Full only	In. Borderline	Excluding new independent countries						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.151	0.138	0.152	0.129		0.077	0.048	0.066		0.188
	0.000	*0.000*	*0.000*	*0.000*		*0.034*	*0.017*	*0.009*		*0.038*
Schooling					0.053					0.046
					0.000					*0.041*
Literacy						0.003				
						0.097				
Urbanization							0.018			-0.041
							0.230			*0.219*
Ln Radios								0.055		0.004
								0.024		*0.938*
GDP Ratio									0.004	
									0.000	
Controls	No	No	No	No	No	Trend	Trend	Trend	No	Initial Values
Observed Probability	0.191	0.140	0.201	0.175	0.162	0.239	0.177	0.199	0.180	0.307
Predicted Probability	0.162	0.087	0.174	0.151	0.127	0.121	0.042	0.087	0.169	0.293
Wald test	42.62 [*0.00*]	40.60 [*0.00*]	44.39 [*0.00*]	31.71 [*0.00*]	50.94 [*0.00*]	146.78 [*0.00*]	164.90 [*0.00*]	120.78 [*0.00*]	6.81 [*0.00*]	19.17 [*0.00*]
Pseudo R-squared	0.12	0.22	0.11	0.10	0.15	0.35	0.41	0.34	0.02	0.08
Observations	3321	3321	3321	3167	2788	2048	3129	2267	3190	1430
Countries	115	115	115	95	72	78	94	95	95	55

Pooled cross-section probit estimates with annual data (1960-2000). The estimated regression is of the form Democ=F(X;B). The dependent variable takes on the value one in the year and all subsequent years following a permanent democratic transition. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. All specifications include a constant term (marginal effect not reported). The table also reports: i) Mc-Fadden's pseudo R-squared; ii) A Wald chi-square test of the joint significance of estimated coefficients with p-value given in italics; iii) the observed probability of a permanent democratization and iv) the predicted probability estimated at the mean of the independent variables. Democratization events are reported in Table 2.1. A detailed chronology is given in Appendix A. Estimation is performed in all countries that were non-democratic in the beginning of the sample (1960). For detailed variable definitions and sources see Appendix B.2.

The specifications in columns (6)—(8) include a linear time trend (marginal effect always significantly positive, not reported). In column (10) all independent variables correspond to 1975 values (before the first democratization episode). In column (1) we perform the estimation in the "full" sample (62 democratization countries and 53 always authoritarian nations). In column (2) the dependent variable equals one in the year and all subsequent years of full democratization events only (38 countries given in Table 2.1, Panel A). In column (3), "borderline" episodes are also considered as permanent democratizations (Table 2.1; Panel C). In columns (4)—(10) newly independent countries (Table 2.1; Panel E) are excluded from the estimation.

Table 4.5: Social Structure, Economic Freedom & Income

	Religion		Fractionalization				Trade			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.116	0.048	0.031	0.039	0.013	0.014	0.045	0.027	0.027	0.028
	0.000	*0.000*	*0.002*	*0.002*	*0.032*	*0.006*	*0.000*	*0.000*	*0.006*	*0.003*
Muslim	-0.152	-0.088	-0.134	-0.108	-0.075	-0.055	-0.116	-0.051	-0.130	-0.101
	0.010	*0.006*	*0.000*	*0.002*	*0.000*	*0.000*	*0.007*	*0.004*	*0.000*	*0.000*
Confucian	-0.002	-0.001	-0.001	-0.106	-0.061	-0.052	-0.143	-0.044	-0.125	-0.088
	0.269	*0.078*	*0.003*	*0.134*	*0.010*	*0.004*	*0.030*	*0.055*	*0.000*	*0.011*
Oil		-0.051				-0.018		-0.026		-0.037
		0.001				*0.003*		*0.001*		*0.006*
Religious Fragmentation			-0.149		-0.079	-0.060			-0.127	-0.104
			0.000		*0.001*	*0.001*			*0.001*	*0.004*
Ethnic Fragmentation				-0.090	-0.024	-0.008				-0.018
				0.036	*0.251*	*0.627*				*0.567*
Trade Openness							0.045		0.040	
							0.075		*0.050*	
Trade/GDP							-0.018			-0.015
							0.067			*0.401*
Additional Controls	No	Decade dum.	Decade dum.	Decade dum.	Trend	Trend	Trend	Trend	Trend	Decade dum.
Observed Probability	0.175	0.175	0.174	0.174	0.174	0.174	0.217	0.175	0.217	0.174
Predicted Probability	0.145	0.019	0.035	0.043	0.016	0.012	0.041	0.017	0.031	0.028
Wald test	42.51 [*0.00*]	248.40 [*0.00*]	233.21 [*0.00*]	253.56 [*0.00*]	193.54 [*0.00*]	188.14 [*0.00*]	207.63 [*0.00*]	199.12 [*0.00*]	176.44 [*0.00*]	243.14 [*0.00*]
Pseudo R-squared	0.13	0.47	0.48	0.45	0.52	0.54	0.52	0.51	0.55	0.51
Observations	3166	3166	3166	3144	3144	3144	2437	3166	2441	3144
Countries	94	94	94	93	93	93	75	94	75	93

Pooled cross-section probit estimates with annual data (1960-2000). Estimation is performed in all countries that were non-democratic in the beginning of the sample (1960). Newly independent countries (Table 2.1; Panel E) are excluded from the estimation. The estimated regression is of the form Democ=F(X;B). The dependent variable takes on the value one in the year and all subsequent years following a permanent democratic transition. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. All specifications include a constant term (marginal effect not reported). The table also reports: i) Mc-Fadden's pseudo R-squared; ii) A Wald chi-square test of the joint significance of estimated coefficients with p-value given in italics; iii) the observed probability of a permanent democratization and iv) the predicted probability estimated at the mean of the independent variables. Democratization events are reported in Table 2.1. A detailed chronology is given in Appendix A.
Estimation is performed in all countries that were non-democratic in the beginning of the sample (1960). For detailed variable definitions and sources see Appendix B.2. Columns (5)—(9) include a linear time trend and columns (2), (3), (4) and (10) include decade dummies (marginal effects not reported).

Table 4.6: Endowment, Institutions & Income

	Endowments	European Heritage		Legal Origin		Settler Mortality			History	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.045	0.078	0.086	0.050	0.022	0.017	0.010	0.0027	0.084	0.016
	0.001	*0.012*	*0.003*	*0.000*	*0.001*	*0.036*	*0.025*	*0.066*	*0.000*	*0.001*
Latitude	0.001	0.002	0.029		0.001					
	0.421	*0.122*	*0.188*		*0.216*					
European Heritage		0.159	0.065							0.023
		0.001	*0.044*							*0.219*
Muslim			-0.113		-0.059		-0.036	-0.0002	-0.054	-0.023
			0.048		*0.002*		*0.002*	*0.000*	*0.388*	*0.218*
Confucian			-0.097		-0.054		-0.228	-0.0007	-0.160	-0.050
			0.377		*0.099*		*0.083*	*0.000*	*0.019*	*0.283*
Oil			-0.148							-0.024
			0.003							*0.002*
Common Law				-0.006	-0.004					0.010
				0.801	*0.755*					*0.426*
Ln Settler Mortality						-0.009	-0.004	-0.0042		
						0.217	*0.410*	*0.014*		
Independence									0.204	0.052
									0.000	*0.001*
Additional Controls	Trend	No	No	Trend	Trend	Trend	Decade	Trend	No	Trend
Observed Probability	0.175	0.175	0.175	0.175	0.175	0.131	0.131	0.139	0.182	0.182
Predicted Probability	0.041	0.146	0.143	0.040	0.019	0.017	0.009	0.003	0.134	0.016
Wald test	165.87 [*0.00*]	63.42 [*0.00*]	74.51 [*0.00*]	169.31 [*0.00*]	200.95 [*0.00*]	88.85 [*0.00*]	165.78 [*0.00*]	138.35	70.08 [*0.00*]	175.65 [*0.00*]
Pseudo R-squared	0.41	0.13	0.16	0.41	0.50	0.41	0.47	0.58	0.18	0.53
Observations	3167	3167	3166	3167	3166	1938	1937	1855	3035	3035
Countries	94	94	94	94	94	49	49	46	94	94

Pooled cross-section probit estimates with annual data (1960-2000). Estimation is performed in all countries that were non-democratic in the beginning of the sample. Newly independent countries (Table 2.1; Panel E) are excluded from the estimation. The estimated regression is of the form Democ=F(X;B). The dependent variable takes on the value one in the year and all subsequent years following a permanent democratic transition. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. All specifications include a constant term. The table also reports: i) Mc-Fadden's pseudo R-squared; ii) A Wald chi-square test of the joint significance of estimated coefficients with p-value given in italics; iii) the observed probability of a permanent democratization and iv) the predicted probability estimated at the mean of the independent variables. Democratization events are reported in Table 2.1. A detailed chronology is given in Appendix A.

In column (8) we exclude from the estimation Mali and Nigeria. For detailed variable definitions and sources see Appendix B.2. Columns (1), (4), (5), (6), (8) and (10) include a linear time trend and column (7) decade dummies (marginal effects not reported).

Table 4.7: Income, Social Structure, Economic Freedom, Institutions & Democartic Transitions

	Modernization hypothesis		Social Structure			Initial Conditions, History, Institutions				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.009		0.003	0.003	0.002	0.002	0.004	0.005	0.001	0.006
	0.000		*0.001*	*0.020*	*0.046*	*0.034*	*0.096*	*0.071*	*0.596*	*0.021*
Schooling		0.005								
		0.000								
Muslim			-0.009	-0.009	-0.011	-0.006	-0.005	-0.007	0.004	-0.004
			0.003	*0.004*	*0.000*	*0.041*	*0.487*	*0.289*	*0.598*	*0.572*
Confucian			-0.009	-0.009	-0.009	-0.009	-0.013	-0.014	-0.036	-0.023
			0.250	*0.275*	*0.070*	*0.080*	*0.150*	*0.088*	*0.222*	*0.014*
Oil			-0.005	-0.005	-0.004	-0.005	-0.012	-0.013	-0.001	-0.013
			0.000	*0.074*	*0.075*	*0.053*	*0.197*	*0.167*	*0.877*	*0.124*
Ethnic Fragmentation				-0.005			-0.006			
				0.162			*0.502*			
Religious Fragmentation					-0.010		0.001			
					0.004		*0.946*			
Independence						0.009	0.020	0.021	0.036	0.024
						0.001	*0.002*	*0.001*	*0.001*	*0.000*
European Heritage								-0.001		
								0.847		
Ln Settler Mortality									0.003	
									0.308	
Common Law										0.013
										0.032
Additional Controls	No	No	Trend	Trend	Trend	Trend	No	No	No	No
Observed Probability	0.018	0.017	0.018	0.018	0.018	0.019	0.019	0.019	0.016	0.019
Predicted Probability	0.017	0.013	0.005	0.004	0.004	0.004	0.015	0.015	0.011	0.015
Wald test	10.69 [*0.00*]	41.76 [*0.00*]	97.45 [*0.00*]	91.48 [*0.00*]	103.69 [*0.00*]	98.93 [*0.00*]	47.47 [*0.00*]	48.28 [*0.00*]	68.73 [*0.00*]	50.36 [*0.00*]
Pseudo R-squared	0.02	0.06	0.19	0.19	0.21	0.20	0.05	0.05	0.08	0.06
Observations	2662	2377	2661	2644	2661	2530	2513	2530	1660	2530
Countries	93	72	93	91	93	92	91	92	48	92

Pooled cross-section probit estimates with annual data (1960-2000). Estimation is performed in all countries that were non-democratic in the beginning of the sample (1960). Newly independent countries (Table 2.1; Panel E) are excluded from the estimation. The estimated regression is of the form Democ=F(X;B). The dependent variable takes on the value one only in the year the democratic transition. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. All specifications include a constant term (marginal effect not reported). Estimation is performed in all countries that were non-democratic in the beginning of the sample.

Table 4.8: Additional Factors Influencing the Democartic Transition

	Liberal Hypothesis			Economic Growth & Crisis				Collective Action		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.001	0.002	0.011	0.006	0.001	0.003	0.012	0.007	0.003	0.006
	0.647	*0.077*	*0.024*	*0.004*	*0.762*	*0.338*	*0.015*	*0.021*	*0.344*	*0.091*
Independence	0.018	0.010	0.018	0.026	0.025	0.027	0.023	0.023	0.023	0.022
	0.016	*0.001*	*0.158*	*0.020*	*0.000*	*0.000*	*0.038*	*0.001*	*0.001*	*0.005*
Trade Openness	0.042									
	0.000									
Trade/GDP		0.001								
		0.66								
Trade Liberalization (t to t-3)			0.055				0.042	0.037	0.040	0.037
			0.000				*0.005*	*0.018*	*0.016*	*0.019*
Growth (average t to t-3)				-0.111			-0.193	-0.153		-0.154
				0.020			*0.005*	*0.004*		*0.003*
Currency Crisis (t to t-3)					0.021				0.015	
					0.002				*0.061*	
Banking Crisis						0.035			0.029	
						0.001			*0.016*	
Armed Conflict Ending (t to t-3)								0.026	0.022	0.025
								0.010	*0.026*	*0.011*
Additional Controls	Religion	Religion/Trend/Oil	Religion	No	No	No	Religion	Religion	Religion	Soc. Structure
Observed Probability	0.02	0.02	0.03	0.02	0.02	0.02	0.03	0.02	0.02	0.02
Predicted Probability	0.02	0.00	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Wald test	46.92 [0.00]	98.45 [*0.00*]	26.72 [*0.00*]	30.48 [*0.00*]	42.73 [*0.00*]	26.78 [*0.00*]	26.39 [*0.00*]	53.3 [*0.00*]	45.98 [*0.00*]	56.56 [*0.00*]
Pseudo R-squared	0.09	0.20	0.06	0.05	0.05	0.06	0.07	0.10	0.08	0.10
Observations	1851	2530	1851	2393	2280	2280	1765	1719	1684	1719
Countries	73	92	73	82	81	81	63	61	65	61

Pooled cross-section probit estimates with annual data (1960-2000). Estimation is performed in all countries that were non-democratic in the beginning of the sample (1960). Newly independent countries (Table 2.1; Panel E) are excluded from the estimation. The estimated regression is of the form Democ=F(X;B). The dependent variable takes on the value one only in the year the democratic transition. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. All specifications include a constant term. The table also reports: i) Mc-Fadden's pseudo R-squared; ii) A Wald chi-square test of the joint significance of estimated coefficients with p-values given in italics; iii) the observed probability of a permanent democratization and iv) the predicted probability estimated at the mean of the independent variables. Democratization events are reported in Table 2.1. A detailed chronology is given in Appendix A.

Estimation is performed in all countries that were non-democratic in the beginning of the sample (1960). For detailed variable definitions and sources see Appendix B.2. Columns (1) - (3) and (7) - (9) include religious controls (marginal effects not reported). Column (2) includes an oil dummy and Column (10) include social structure controls (marginal effects not reported).

Table 4.9: Democratization Theories & Post-Communism Independence Wave

	Modernization		Social Structure				Trade		Endowments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP in 1989	1.132		0.658	0.589	0.723	0.687	2.703		1.801	1.822
	0.023		*0.400*	*0.400*	*0.342*	*0.376*	*0.001*		*0.013*	*0.012*
Fertility in 1990		-1.554						-5.028		
		0.007						*0.001*		
Muslim			-0.008	-0.009	-0.008	-0.115				
			0.113	*0.123*	*0.130*	*0.136*				
Religious Fragmentation				-0.778		-1.382				
				0.509		*0.339*				
Ethnical Fragmentation					0.367	0.893				
					0.680	*0.461*				
Trade/GDP							-0.008	-0.003		
							0.154	*0.473*		
European Heritage									-6.692	*-8.152*
									0.075	*0.093*
Gini (earliest value)							-0.158	-1.830	-0.110	-0.357
							0.024	*0.046*	*0.133*	*0.729*
Gini Squared								0.035		0.005
								0.067		*0.816*
Observed Probability	0.50	0.48	0.50	0.50	0.50	0.50	0.56	0.56	0.56	0.56
Predicted Probability	0.49	0.35	0.44	0.45	0.43	0.46	0.52	0.34	0.46	0.50
Wald test	5.18 [0.02]	7.22 [0.01]	11.54 [0.01]	14.10 [0.00]	10.72 [0.01]	11.48 [0.02]	13.49 [0.01]	26.68 [0.00]	7.40 [0.06]	8.52 [0.07]
Pseudo R-squared	0.33	0.37	0.38	0.40	0.39	0.41	0.72	0.78	0.65	0.66
Observations	20	21	20	20	20	20	16	16	16	16
Countries	20	21	20	20	20	20	16	16	16	16

Cross-country probit estimates. Estimation is performed in newly independent countries that emerged after the collapse of Czechoslovakia, the U.S.S.R. and Yugoslavia (Table 2.1; Panel E). The estimated regression is of the form Democ=F(X;B). The dependent variable takes on the value one if a new independent country (former Republic) became democratic and zero if the country has remained autocratic. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. All specifications include a constant term, which is not reported. The table also reports: i) Mc-Fadden's pseudo R-squared; ii) A Wald chi-square test of the joint significance of estimated coefficients with p-value given in italics; iii) the observed probability of a permanent democratization and iv) the predicted probability estimated at the mean of the independent variables. Democratization events are reported in Table 2.1. A detailed chronology is given in Appendix A.

Table 4.10: Democratization Theories & The Fall of the Berlin Wall

	Modernization Hypothesis			Social Structure Theories			Endowments, Trade Openess & Growth			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP in 1989	0.328			-0.183	0.395	0.382	0.765	0.744	0.646	0.631
	0.054			*0.217*	*0.023*	*0.070*	*0.000*	*0.000*	*0.006*	*0.013*
Fertility in 1990		-0.320								
		0.030								
Industrialization in 1990			2.554							
			0.024							
Muslim				-0.667						
				0.001						
Confucian				-11.501						
				0.017						
Religious Fragmentation					-0.784				-0.988	-2.252
					0.264				*0.040*	*0.027*
Ethnic Fragmentation					-0.509				0.211	-0.156
					0.427				*0.635*	*0.836*
"Rich" Natural Resources						-0.457				
						0.077				
"Moderately Rich" Resources						0.030				
						0.915				
Central Planning (years in)							-0.018	-0.009	-0.011	-0.018
							0.018	*0.275*	*0.165*	*0.059*
Independence							13.103	18.672	14.363	
							0.046	*0.002*	*0.003*	
Natural Trade Openess								0.639	0.360	0.740
								0.033	*0.002*	*0.016*
Mean Growth (1985-89)										-0.098
										0.034
Observed Probability	0.57	0.53	0.57	0.57	0.57	0.57	0.57	0.57	0.57	0.57
Predicted Probability	0.58	0.52	0.58	0.11	0.58	0.58	0.67	0.71	0.90	0.79
Wald test	3.72 [*0.05*]	4.70 [*0.03*]	5.07 [*0.02*]	17.42 [*0.00*]	6.31 [*0.02*]	7.98 [*0.05*]	18.05 [*0.00*]	36.97 [*0.00*]	18.05 [*0.00*]	19.87 [*0.00*]
Pseudo R-squared	0.11	0.18	0.13	0.35	0.17	0.18	0.41	0.56	0.64	0.60
Countries	28	30	28	28	28	28	28	28	28	28

Cross-country probit estimates. Estimation is performed in newly independent countries that emerged after the collapse of Czechoslovakia, the U.S.S.R. and Yugoslavia (Table I; Panel E) and eight formerly centrally planned economies that were affected by the collapse of Soviet Union. The dependent variable takes on the value one if a new independent country or a centrally planned economy became democratic in the nineties and zero if the country has remained autocratic. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors are reported in italics. For more deatils on the regression diagnostics, see previous Table notes. Democratization events are reported in Table 1.1. A detailed chronology is given in Appendix A. All variables are retrived from de Melo et al. (2001). See Appendix B.2 for exact variable definitions.

4.11

Table XX

Political Economy - Inequality

	Transition & Consolidation							Transition		
						Full only				Full only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.152	0.110				0.096		0.010	0.001	
	0.00	*0.00*				*0.00*		*0.07*	*0.85*	
Schooling			0.091	0.077	0.078		0.072			0.003
			0.00	*0.00*	*0.00*		*0.00*			*0.00*
Gini	0.001	-0.002	0.008	0.005	0.027	0.019	0.025	0.000	-0.002	0.000
	0.79	*0.49*	*0.04*	*0.21*	*0.23*	*0.09*	*0.11*	*0.77*	*0.42*	*0.02*
Gini squred					0.000	0.000	0.000		0.000	
					0.31	*0.09*	*0.19*		*0.50*	
Muslim		-0.353		-0.045	-0.073	-0.299	-0.110		-0.029	-0.009
		0.00		*0.76*	*0.61*	*0.00*	*0.36*		*0.04*	*0.01*
Confucian		-0.315		-0.371	-0.411	-0.171	-0.233		-0.031	-0.003
		0.08		*0.00*	*0.00*	*0.12*	*0.00*		*0.03*	*0.20*
Independence				0.383	0.361		0.321		0.033	0.008
				0.00	*0.01*		*0.00*		*0.03*	*0.04*
Controls	Trend	Trend	No	No	No	Trend	No	Trend	No	No
Observed Probability	0.30	0.30	0.29	0.30	0.30	0.23	0.24	0.03	0.03	0.02
Predicted Probability	0.19	0.14	0.25	0.24	0.24	0.06	0.12	0.02	0.02	0.00
Wald test	101.05 [*0.00*]	129.27 [*0.00*]	31.17 [*0.00*]	36.74 [*0.00*]	35.99 [*0.00*]	77.52 [*0.00*]	26.26 [*0.00*]	27.56 [*0.00*]	34.92 [*0.00*]	41.43 [*0.00*]
Pseudo R-squared	0.31	0.41	0.16	0.22	0.23	0.45	0.35	0.08	0.06	0.21
Observations	1570	1570	1421	1377	1377	1570	1377	1139	1127	1067
Countries	71	71	54	53	53	71	53	63	62	49

Pooled cross-section probit estimates with annual data (1960-2000). Estimation is performed in all countries that were non-democratic in the beginning of the sample (1960). Newly independent countries (Table I; Panel E) are excluded from the estimation. The estimated regression is of the form Democ=F(X;B). The dependent variable takes on the value one only in the year the democratic transition. Estimation is performed with maximum likelihood. Probit slope derivatives (marginal effects) are reported. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. All specifications include a constant term (marginal effect not reported). The specifications reported in columns (1), (2), (6) and (8) also include a linear time trend. Estimation is performed in all countries that were non-democratic in the beginning of the sample.

The table also reports: i) Mc-Fadden's pseudo R-squared; ii) A Wald chi-square test of the joint significance of estimated coefficients with p-value given in italics; iii) the observed probability of a permanent democratization and iv) the predicted probability estimated at the mean of the independent variables. Democratization events are reported in Table I. A detailed chronology is given in Appendix A.

Table 4.12: Alternative Indicators (Polity IV and Freedom House)

	Transition & Consolidation					Transition				
	Polity IV (1960-2000)			Freedom House (1973-2000)		Polity IV (1960-2000)			Freedom House (1973-2000)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.070	0.066	0.072	0.072	0.032	0.002	0.004	-0.002	0.002	-0.005
	0.000	*0.019*	*0.001*	*0.000*	*0.012*	*0.480*	*0.154*	*0.656*	*0.677*	*0.449*
Independence	0.316	0.265	0.312	0.189	0.114	0.033	0.029	0.024	0.019	0.007
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.002*	*0.000*	*0.000*	*0.044*	*0.548*
Muslim	0.081		0.001	-0.200	-0.207				-0.017	-0.018
	0.191		*0.330*	*0.000*	*0.000*				*0.049*	*0.116*
Confucian	-0.182		-0.002	-0.422	-0.312				-0.043	-0.024
	0.000		*0.008*	*0.000*	*0.000*				*0.034*	*0.239*
Oil	-0.133	-0.124	-0.136	-0.117	-0.097				-0.006	-0.002
	0.000	*0.049*	*0.055*	*0.000*	*0.000*				*0.587*	*0.000*
Religious Fragmentation		-0.080	-0.020	-0.177	-0.185					
		0.380	*0.857*	*0.000*	*0.000*					
Ethnic Fragmentation		0.031	0.028	0.076	0.134					0.010
		0.727	*0.758*	*0.029*	*0.020*					*0.522*
Latitude			0.004							0.001
			0.000							*0.11*
European Heritage			0.131							0.020
			0.000							*0.135*
Trade Liberalization (t to t-3)					0.028					
					0.092					
Growth (average t to t-3)						-0.208				
						0.000				
Crisis (t to t-3)								0.028	0.026	0.028
								0.000	*0.002*	*0.001*
Other	No	No	No	Trend/Non-clustered S.E.		No	No	No	Trend/Non-clustered S.E.	
Observed Probability	0.20	0.21	0.21	0.25	0.25	0.02	0.02	0.02	0.03	0.03
Predicted Probability	0.17	0.18	0.17	0.13	0.13	*0.02*	0.02	0.02	0.02	0.02
Wald test	66.86 [*0.00*]	64.21 [*0.00*]	66.00 [*0.00*]	622.73 [*0.00*]	654.95 [*0.00*]	42.31 [*0.00*]	51.92 [*0.00*]	44.41 [*0.00*]	71.23 [*0.00*]	23.14 [*0.00*]
Pseudo R-squared	0.13	0.13	0.13	0.39	0.40	0.04	0.05	0.07	0.12	0.13
Observations	3035	3013	3013	2251	2251	1832	2333	2188	1572	1572
Countries	94	93	93	93	93	72	82	80	82	82

Table 4.13: Ordered Analysis (Polity IV and Freedom House)

	Polity IV (1960-2000)					Freedom House (1973-2000)				
	Probit			Logit		Probit			Logit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.333	0.309	0.319	0.612	0.489	0.361	0.508	0.360	0.421	0.653
	0.000	0.064	*0.009*	*0.000*	*0.100*	*0.034*	*0.000*	*0.004*	*0.040*	*0.002*
Schooling		0.026			0.150	0.081				
		0.629			*0.075*	*0.189*				
Independence	0.845	0.592	0.497	1.073	1.095	0.738	0.548	0.667	0.375	0.315
	0.001	0.055	*0.132*	*0.004*	*0.139*	*0.036*	*0.108*	*0.072*	*0.470*	*0.568*
Muslim			-0.513	-0.499	-0.071	-0.022	-0.004	-0.478	-0.700	-0.489
			0.157	*0.253*	*0.926*	*0.935*	*0.206*	*0.186*	*0.232*	*0.393*
Confucian			-0.056	-0.002	-0.352	-0.292	-0.003	-0.584	-0.083	0.323
			0.751	*0.993*	*0.208*	*0.144*	*0.148*	*0.013*	*0.903*	*0.604*
Oil			-0.628	-1.158	-1.593	-1.488	-0.872			-1.359
			0.093	*0.059*	*0.019*	*0.000*	*0.049*			*0.061*
Religious Fragmentation			-0.203		0.099		-0.257	0.066	0.008	-0.343
			0.709		*0.926*		*0.595*	*0.892*	*0.992*	*0.681*
Ethnic Fragmentation			0.032		0.615		0.091	-0.432		0.388
			0.928		*0.346*		*0.821*	*0.303*		*0.600*
Trade Openness								0.456	0.763	
								0.011	*0.020*	
European Heritage									1.286	1.377
									0.035	*0.027*
Latitude									0.025	0.018
									0.104	*0.215*
Trend	No	Yes	Yes	No	No	No	No	Yes	Yes	No
Anciliary parameter 1	3.120	3.460	3.267	5.278	5.270	2.963	3.589	2.987	4.526	5.037
Anciliary parameter 2	3.642	4.037	3.834	6.179	6.204	4.513	5.066	4.658	7.396	7.642
Wald test	66.25 [*0.00*]	89.78 [*0.00*]	96.60 [*0.00*]	84.82 [*0.00*]	94.30 [*0.00*]	90.75 [*0.00*]	70.8 [*0.00*]	112.09 [*0.00*]	103.66 [*0.00*]	80.26 [*0.00*]
Pseudo R-squared	0.08	0.11	0.13	0.09	0.09	0.15	0.13	0.17	0.19	0.15
Observations	2893	2201	2893	2893	2201	1697	2249	1736	1758	2249
Countries	89	64	89	89	64	65	93	74	75	93

Pooled cross-section ordered probit and logit estimates with annual data (1960-2000). Estimation is performed in all countries that were non-democratic in the beginning of the sample. Newly independent countries (Table 2.1; Panel E) are excluded from the estimation. The estimated regression is of the form Democ=F(X;B). The dependent variable takes three values. In columns (1) - (5) the three values are generated from the 21-range Polity IV index when it is divided equally into three categories. In columns (5) - (10) the three values are generated from the Freedom House characterization "Free", "Partial Free" and "Not Free". Estimation is performed with maximum likelihood assuming normally distributed errors in columns (1)-(3) and (6)--(8) and logistically distributed errors in columns (4), (5), (9) and (10). Probit and Logit coefficients are reported. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. Estimation is performed in all countries that were non-democratic in the beginning of the sample.

Table 4.14: Dynamic Probit Analysis (Polity IV Index, 1960-2000)

	Base Sample (Countries initially non-democratic)				All (Democratization, Always Autocratic, Always Democratic) Countries			
	(1a)	(1b)	(2a)	(2b)	(3b)	(3b)	(4a)	(4b)
Ln GDP	0.029	0.130	0.014	0.138	0.040	0.170	0.025	0.124
	0.084	*0.000*	*0.545*	*0.000*	*0.040*	*0.000*	*0.251*	*0.000*
Growth	-0.557	0.153	-1.164	1.150	-0.809	0.603	-1.038	0.822
	0.052	*0.250*	*0.006*	*0.275*	*0.025*	*0.443*	*0.007*	*0.369*
Muslim			-0.101	0.046	-0.064	-0.032	-0.095	0.017
			0.109	*0.642*	*0.218*	*0.734*	*0.137*	*0.854*
Confucian			0.279	-0.610	0.367	-0.467	0.235	-0.330
			0.049	*0.002*	*0.006*	*0.009*	*0.066*	*0.047*
Trade Openness			0.094	0.180			0.082	0.078
			0.102	*0.041*			*0.109*	*0.254*
Oil					-0.094	0.012		
					0.222	*0.920*		
Ethnic Fragmentation							0.118	-0.047
							0.435	*0.631*
Religious Fragmentation							-0.063	0.116
							0.479	*0.370*
Observed Probability	0.32		0.37		0.52		0.58	
Predicted Probability	0.24		0.33		0.62		0.73	
Wald test	1291.2 [*0.00*]		1021 [0.00]		1926.38 [0.00]		1357 [0.00]	
Pseudo R-squared	0.72		0.73		0.82		0.81	
Observations	2858		2222		4147		3409	
Countries	98		76		133		109	

Dynamic probit estimates (marginal effects). Columns (1a), (2a), (3a) and (4a) report estimates for the probability to transit from a non-democratic polity to democracy. Columns (1b), (2b), (3b) and (4b) report estimates of the probability to remain a democracy. For each full model the probability of a country to transit and stay democratic (transition plus consolidation) is given by the sum of the alpha (i.e. the ones taken from (1a), (2a), (3a) and (4a)) and beta (i.e. the ones taken from (1b), (2b), (3b) and (4b)) coefficients. The dependent variable takes on the value one when the Polity democracy index is greater than zero and zero otherwise. Models (1) and (2) are estimated in those countries that were in the beginning of the sample non-democratic. Models (3) and (4) are estimated to all countries. All specifications include a constant term (marginal effect not reported). P-values based on heteroskedasticity-adjusted standard errors are reported in italics.

The table also reports: i) Mc-Fadden's pseudo R-squared; ii) A Wald chi-square test of the joint significance of estimated coefficients; iii) the observed probability of a permanent democratization, iv) the predicted probability estimated at the mean of the independent variables. Democratization events are reported in Table 2.1. A detailed chronology is given in Appendix A. Estimation is performed in all countries that were non-democratic in the beginning of the sample. For detailed variable definitions and sources see Appendix B.2.

Table 4.15: Linear Probability Model & IV Non-Linear models

	Linear Probability Model - Polity IV (1960-2000)					Instrumental Variables Probit Model				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln GDP	0.151	0.024		0.135	0.104	0.070	0.237	0.344	0.060	0.049
	0.000	*0.744*		*0.001*	*0.087*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Schooling			-0.049							
			0.024							
Independence				0.084	0.113			0.075		
				0.314	*0.309*			*0.163*		
Muslim				-0.147	-0.100	-0.049	-0.113	0.001	-0.110	-0.128
				0.187	*0.437*	*0.000*	*0.001*	*0.987*	*0.000*	*0.000*
Confucian				0.096	0.110	-0.089	-0.235	-0.243	-0.253	-0.183
				0.630	*0.533*	*0.000*	*0.008*	*0.008*	*0.000*	*0.000*
Oil				-0.230	-0.662	-0.034	-0.248	-0.242		
				0.021	*0.000*	*0.000*	*0.000*	*0.000*		
Ethnic Fragmentation				-0.007	0.088			0.059		-0.112
				0.968	*0.663*			*0.392*		*0.000*
Religious Fragmentation				0.057	0.078			0.214		-0.024
				0.616	*0.620*			*0.003*		*0.261*
Trade Openness					0.062				0.420	0.153
					0.417				*0.000*	*0.020*
Latitude					0.003					
					0.403					
European Heritage					0.037					
					0.743					
Year Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Trend	No	No	Trend	Trend
Country Fixed-Effects	No	Yes	Yes	No	No	No	No	No	No	No
R-squared	0.20	0.50	0.46	0.29	0.27	0.19	0.18	0.25	0.54	0.55
Observations	3153	3153	2793	2831	2472	3035	3142	2989	2338	2316
Countries	113	113	75	103	89	94	93	92	75	74

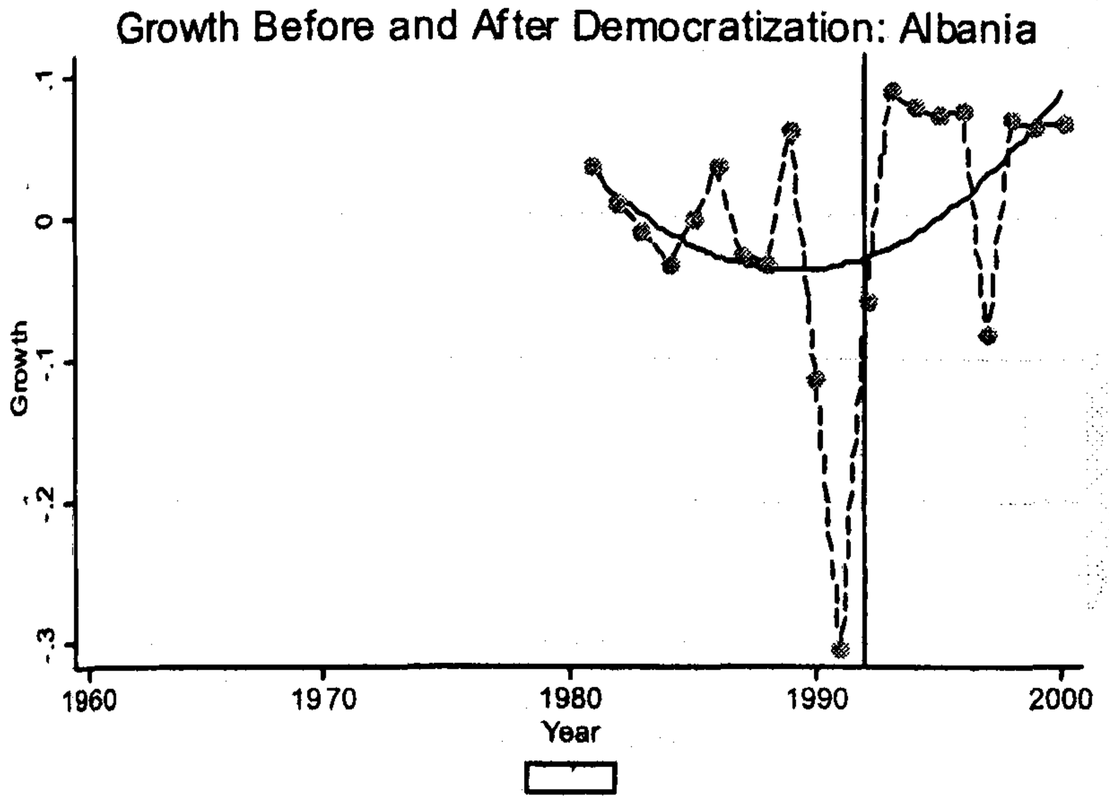
The estimated regression is of the form Democ=F(X;B). Columns (1) to (5) give OLS estimates. The dependent variable in columns (1) to (5) takes on the value one when the Polity democracy index is greater than zero and zero otherwise. Estimation excludes countries that were throughout the sample period (1960-2000) always democratic (Table 2.1; Panel C). Time-fixed effects are included, but the estimates are not reported. The specifications in columns (1) and (2) also include a vector of country specific intercepts (estimates not reported). In columns (6) to (10) the dependent variable takes on the value one in the year and all subsequent years following a permanent democratic transition.

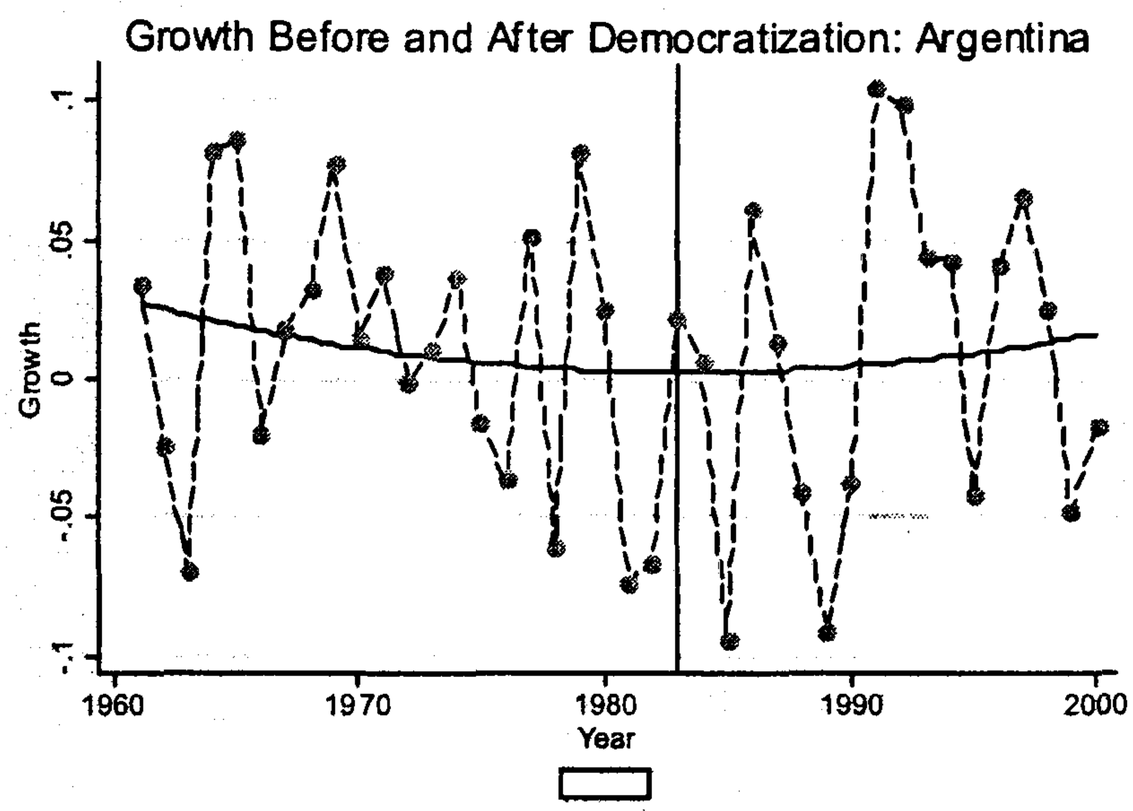
Estimation is performed in all countries that were non-democratic in the beginning of the sample. A detailed chronology is given in Appendix A.Columns (6), (7) and (8) report marginal effects of instrumental variable probit estimates. Ln GDP is treated as endogenous and instrumented with legal origin and years since independence in column 6. In columns (7) and (8) the instruments are latitude, European heritage and a linear time-trend. In columns (9) and (10) both trade openess and Ln GDP are treated as endogenous. The instrumnet set includes: latitude, landlocked, years since independence, legal origin, and European heritage. P-values based on heteroskedasticity-adjusted standard errors clustered by country are reported in italics. R-squared is the overall adjusted R-squared in the OLS estimates and Mc Fadden's pseudo R-squared in the maximum likelihood estimates. Democratization events are reported in Table 2.1. A detailed chronology is given in Appendix A. For detailed variable definitions and sources see Appendix B.2.

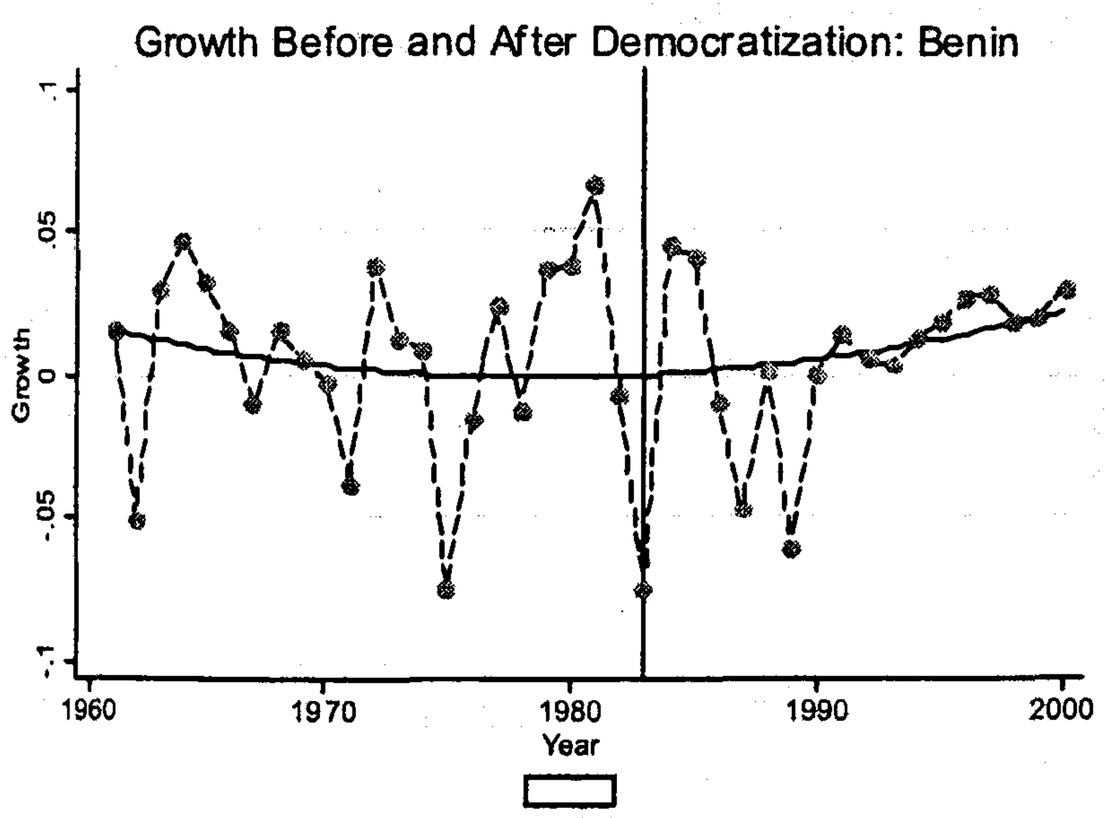
Supplementary Appendix

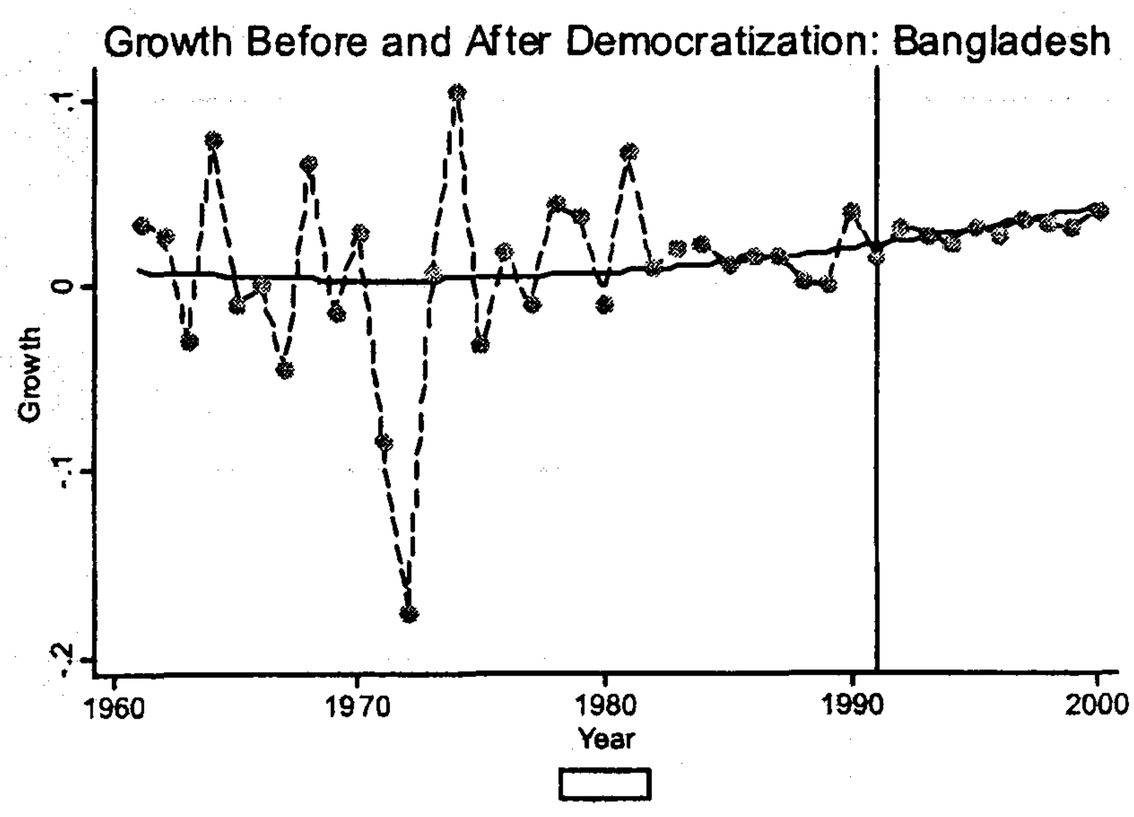
Graphs for the evolution of growth for all democratized countries as identified by the new dataset.

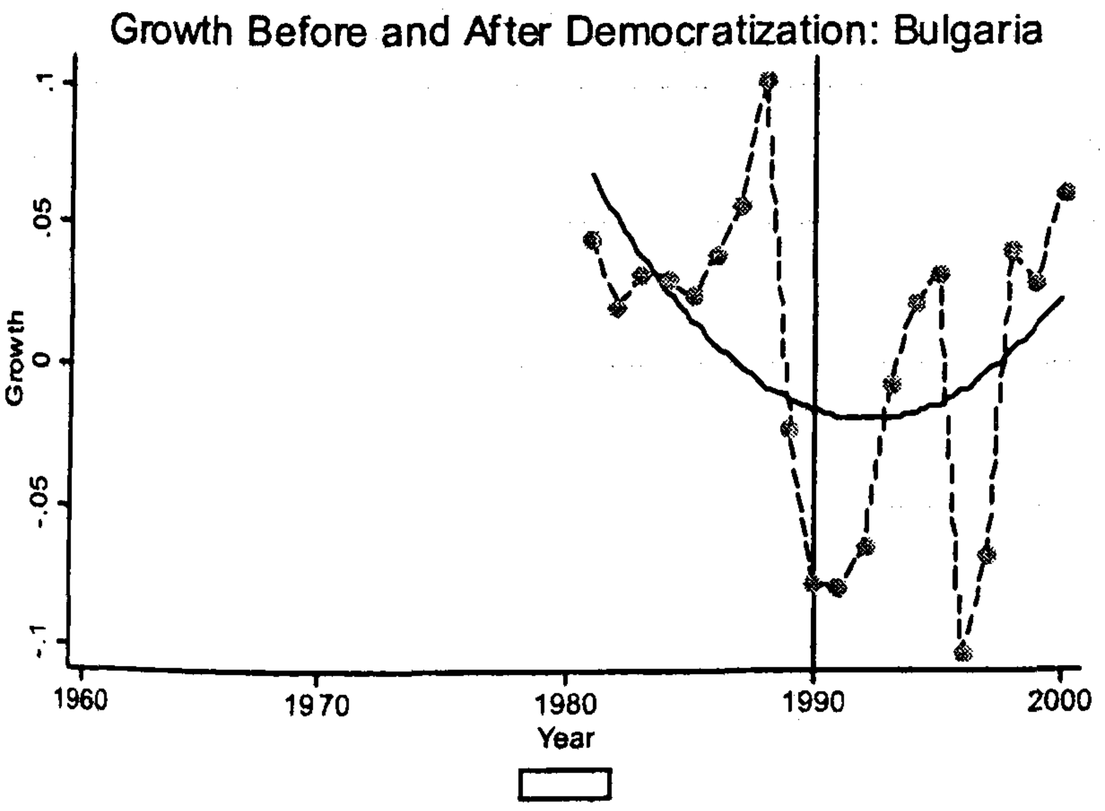
SUPPLEMENTARY APPENDIX—COUNTRY GRAPHS AROUND DEMOCARTIZATION

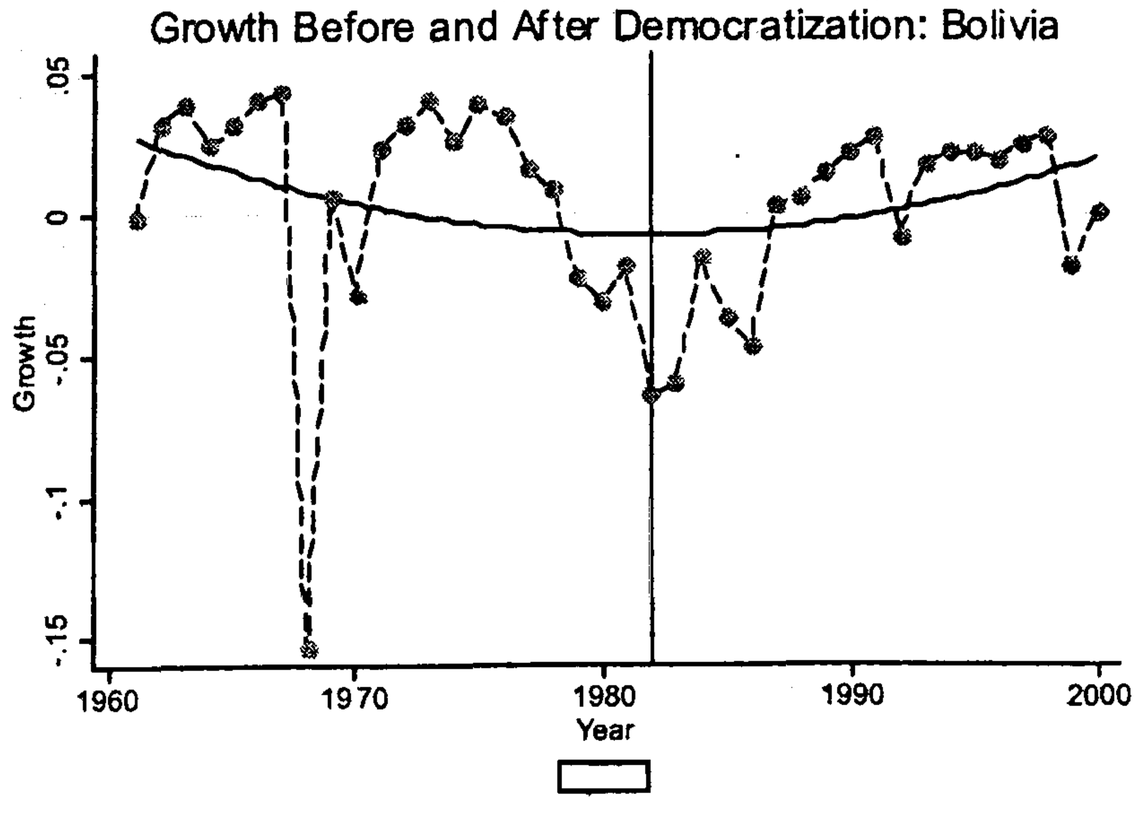




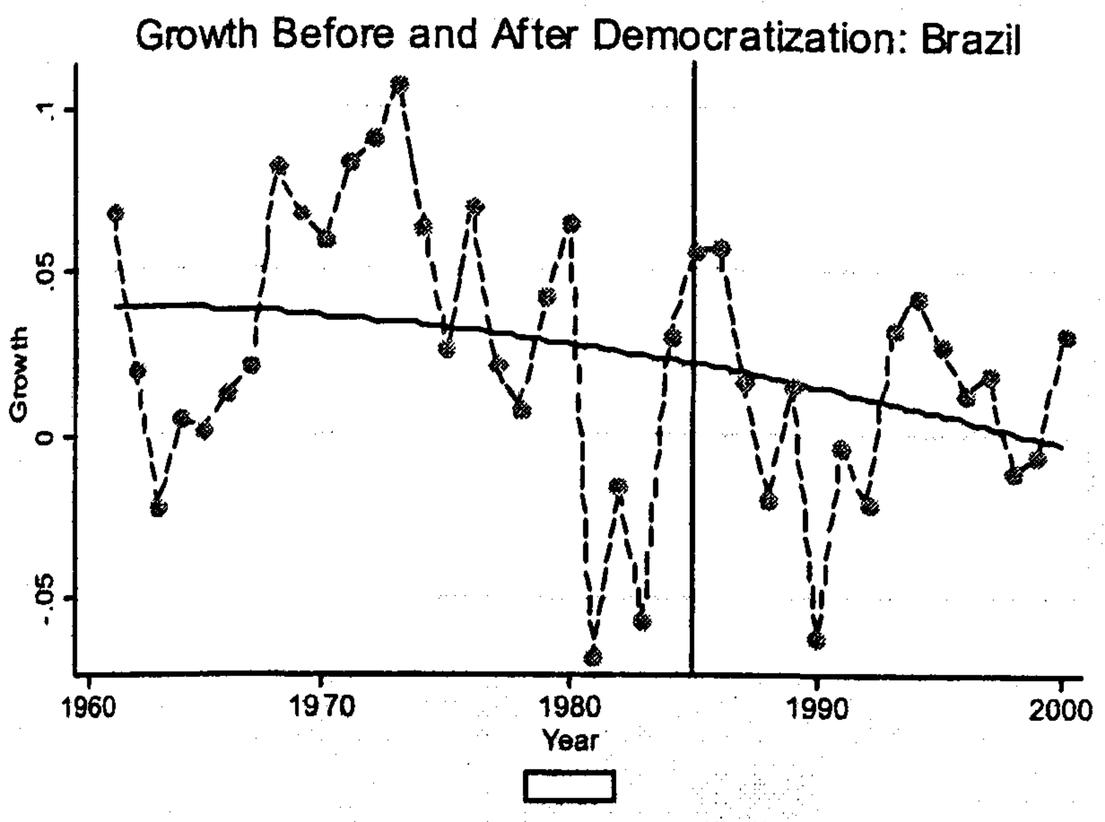


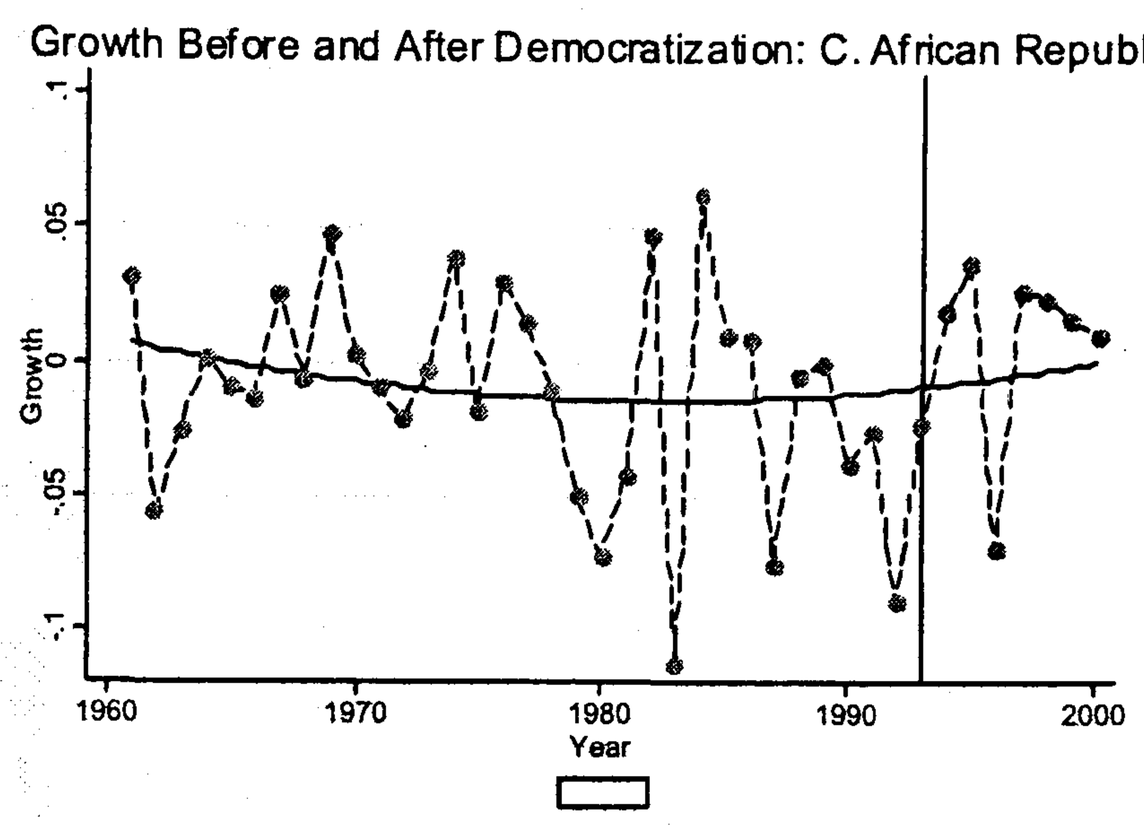


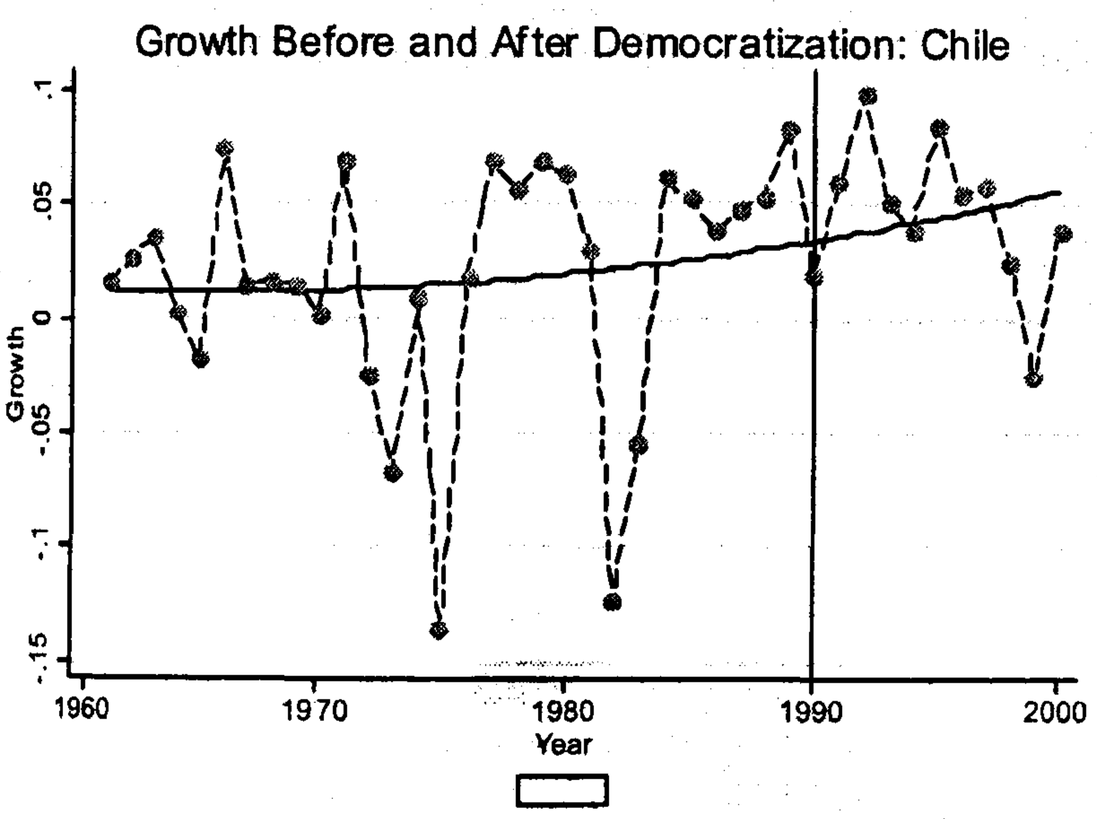


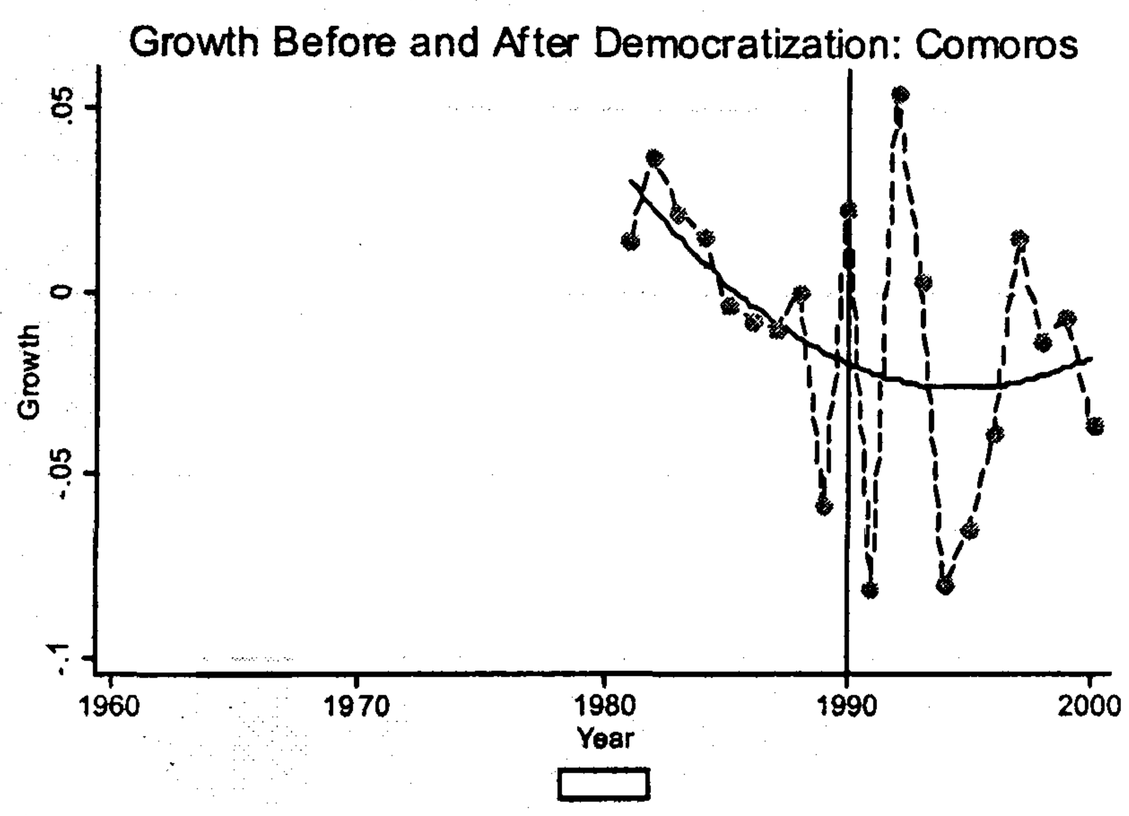


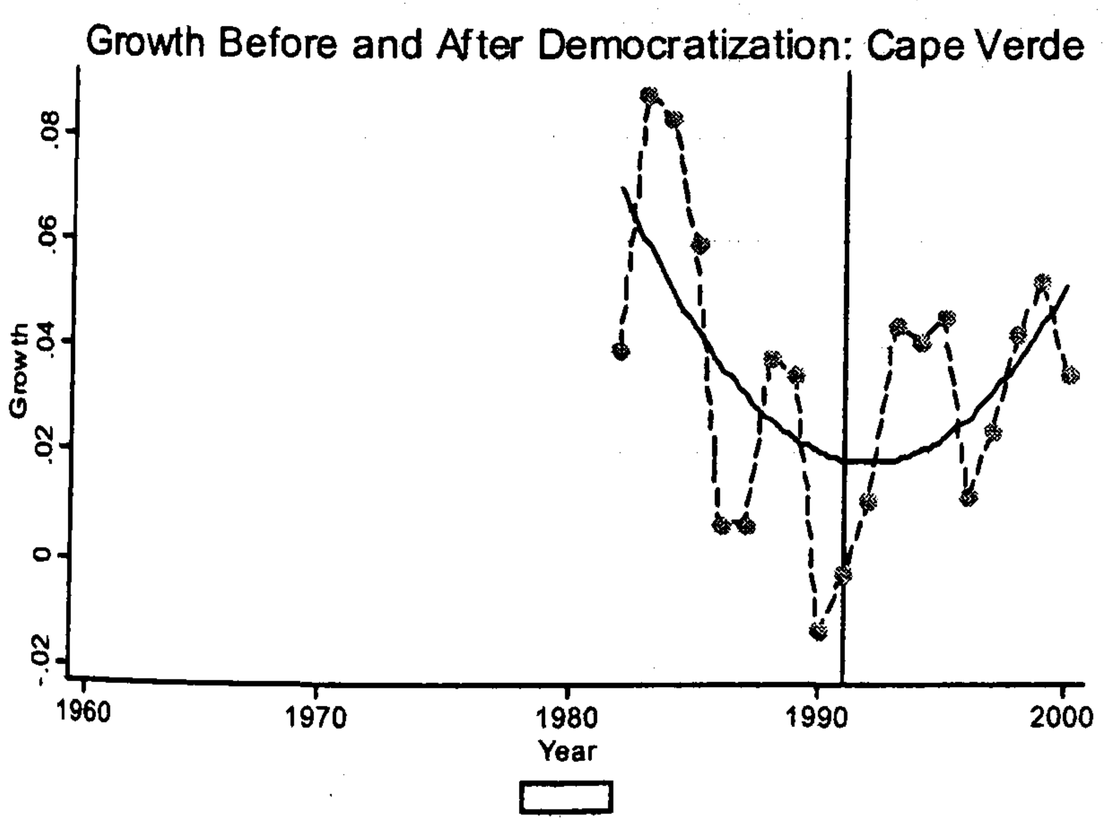
SUPPLEMENTARY APPENDIX—COUNTRY GRAPHS AROUND DEMOCARTIZATION

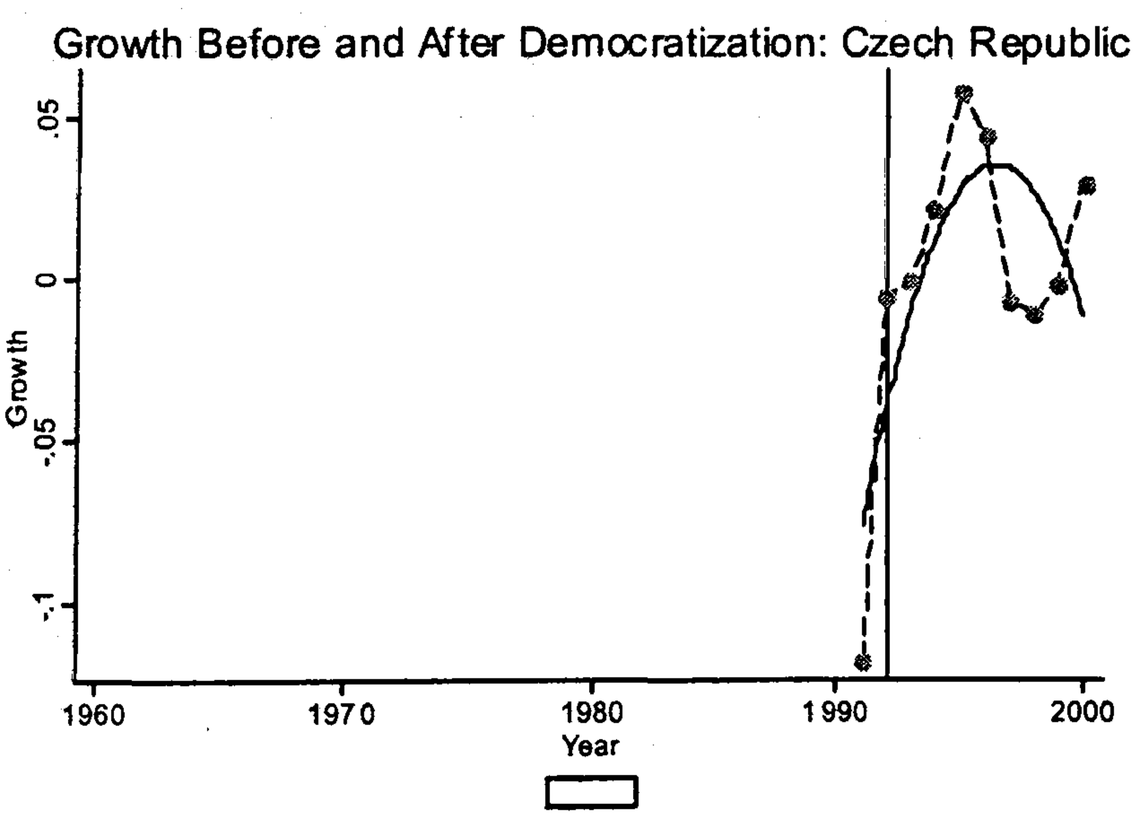




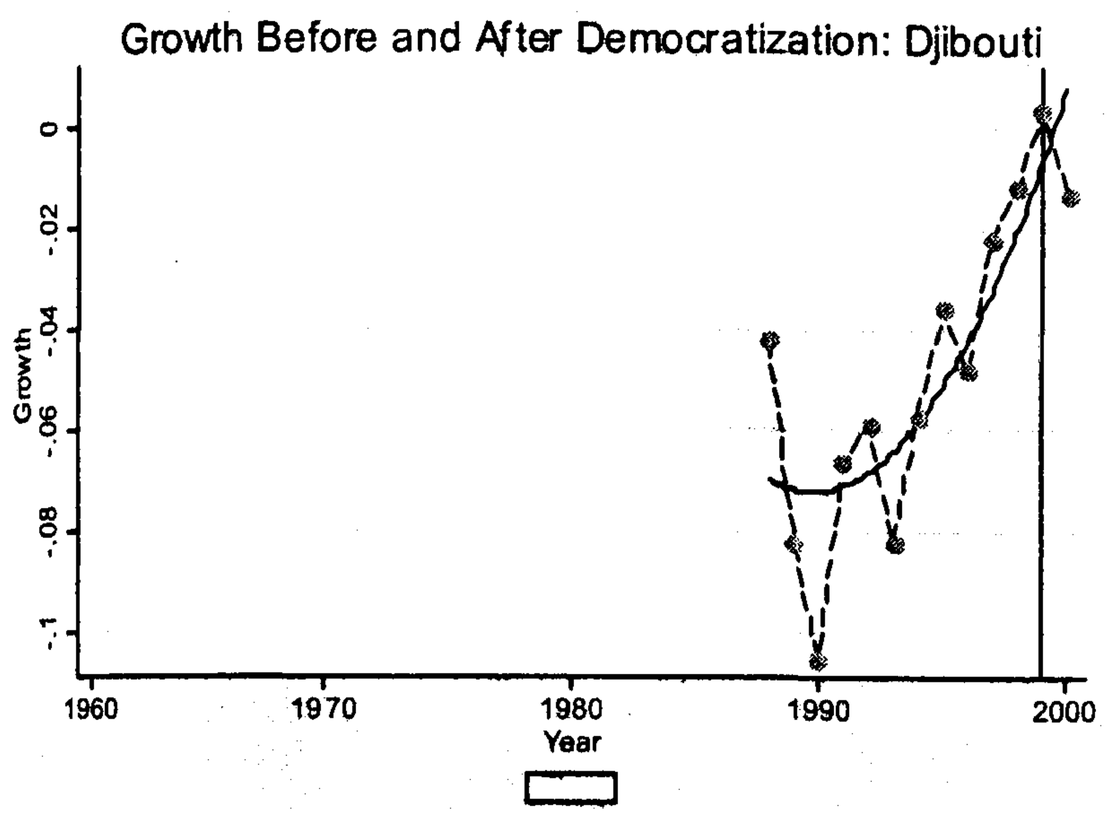


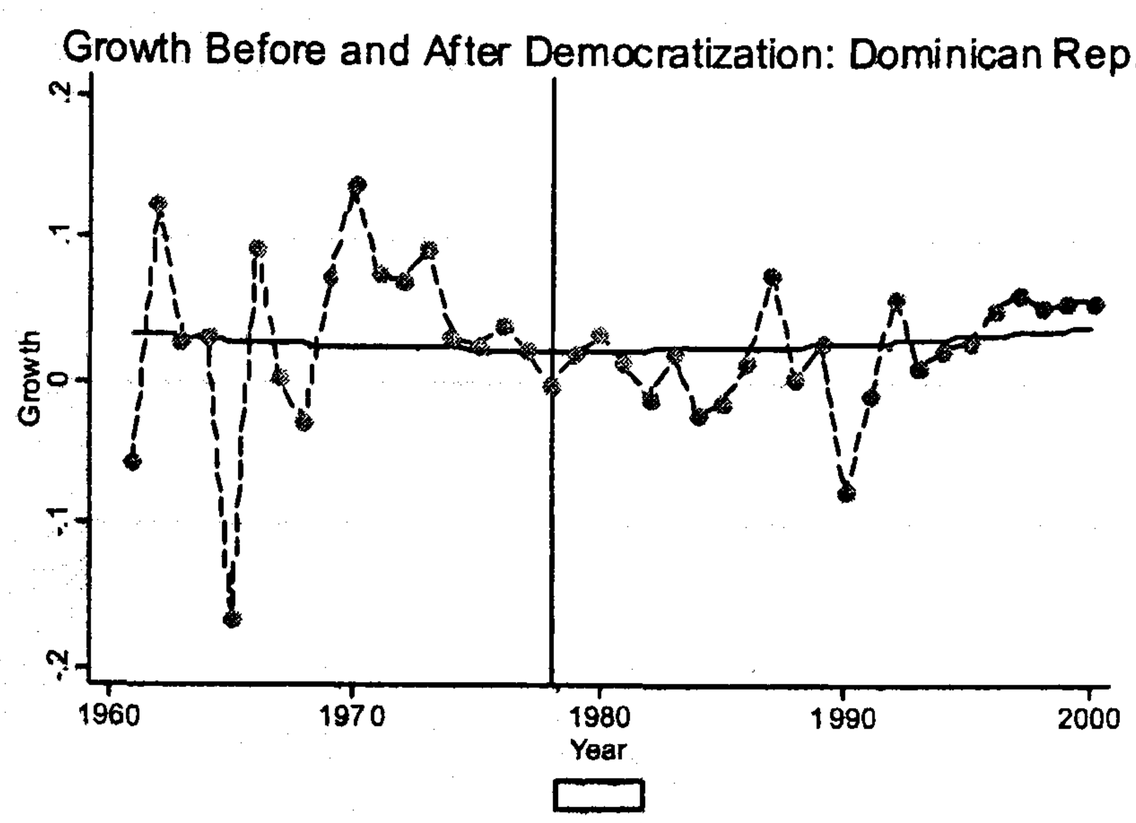


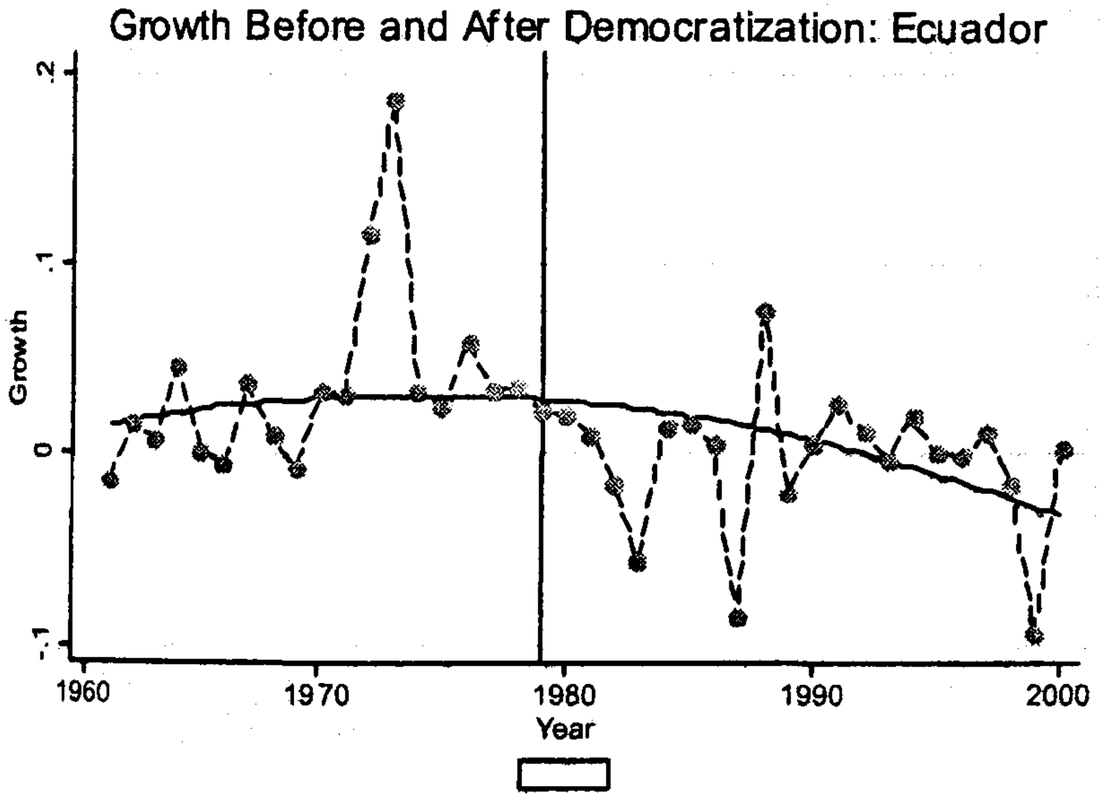


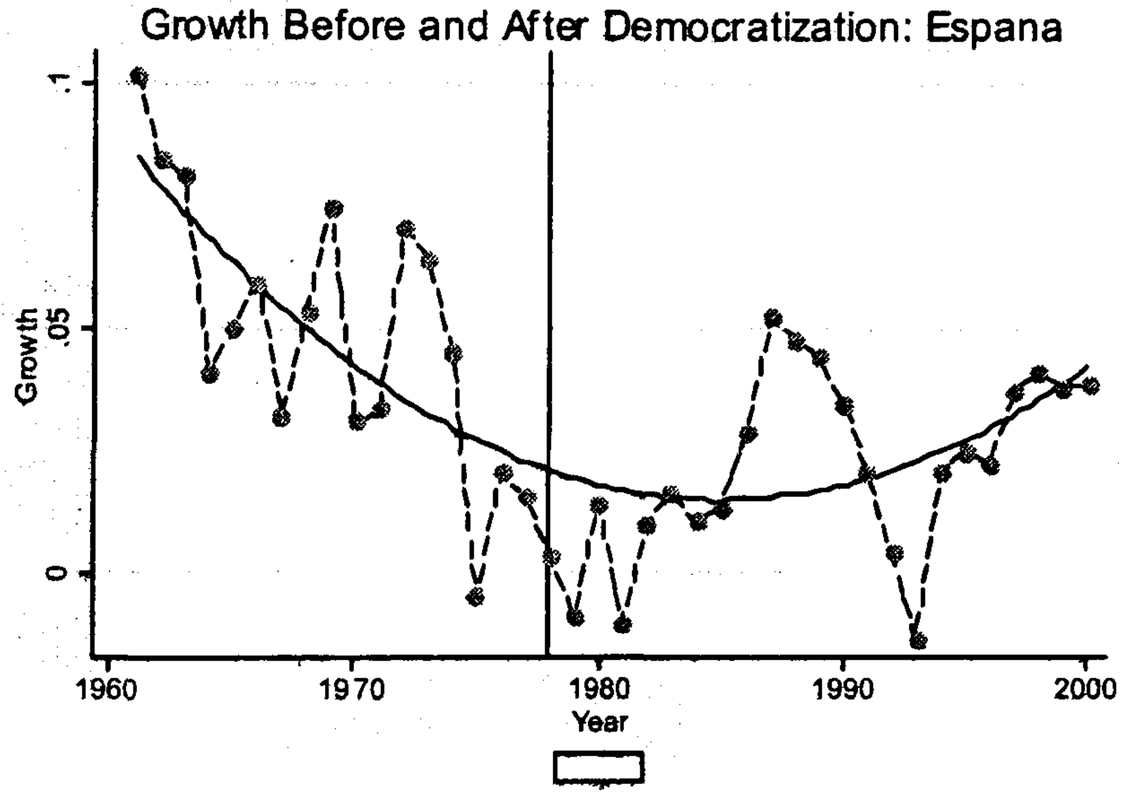


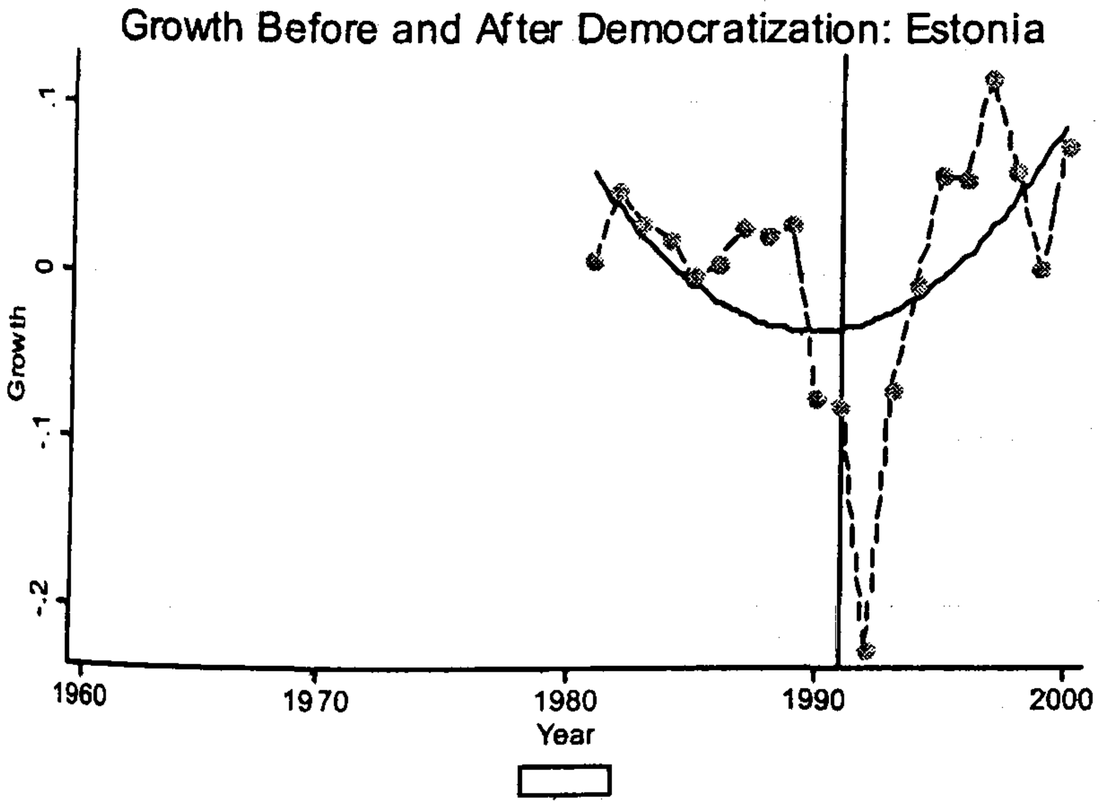
SUPPLEMENTARY APPENDIX—COUNTRY GRAPHS AROUND DEMOCARTIZATION

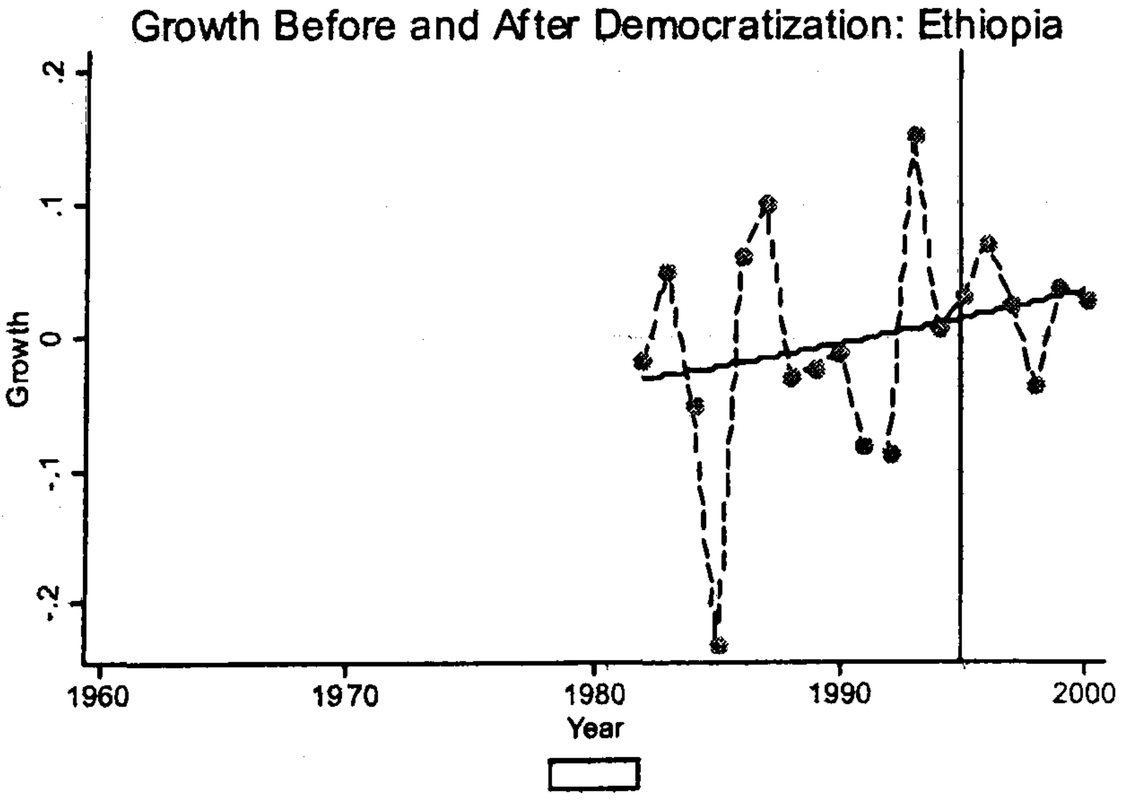




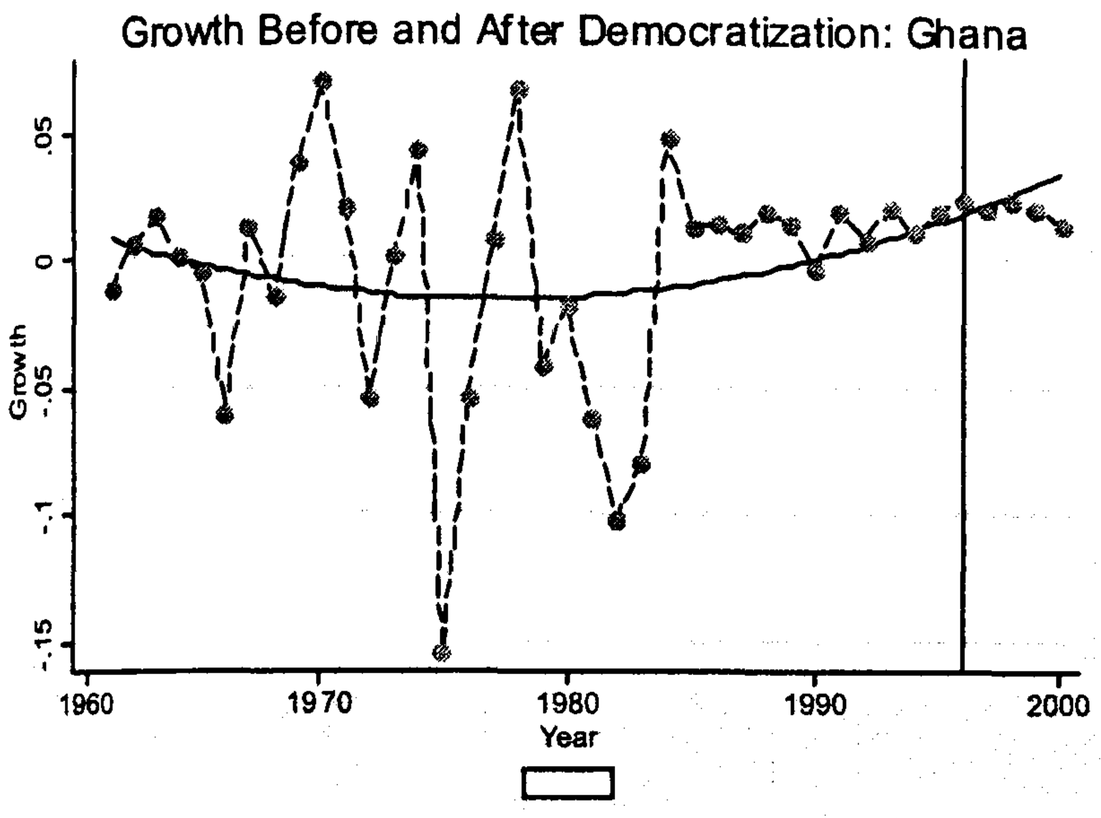


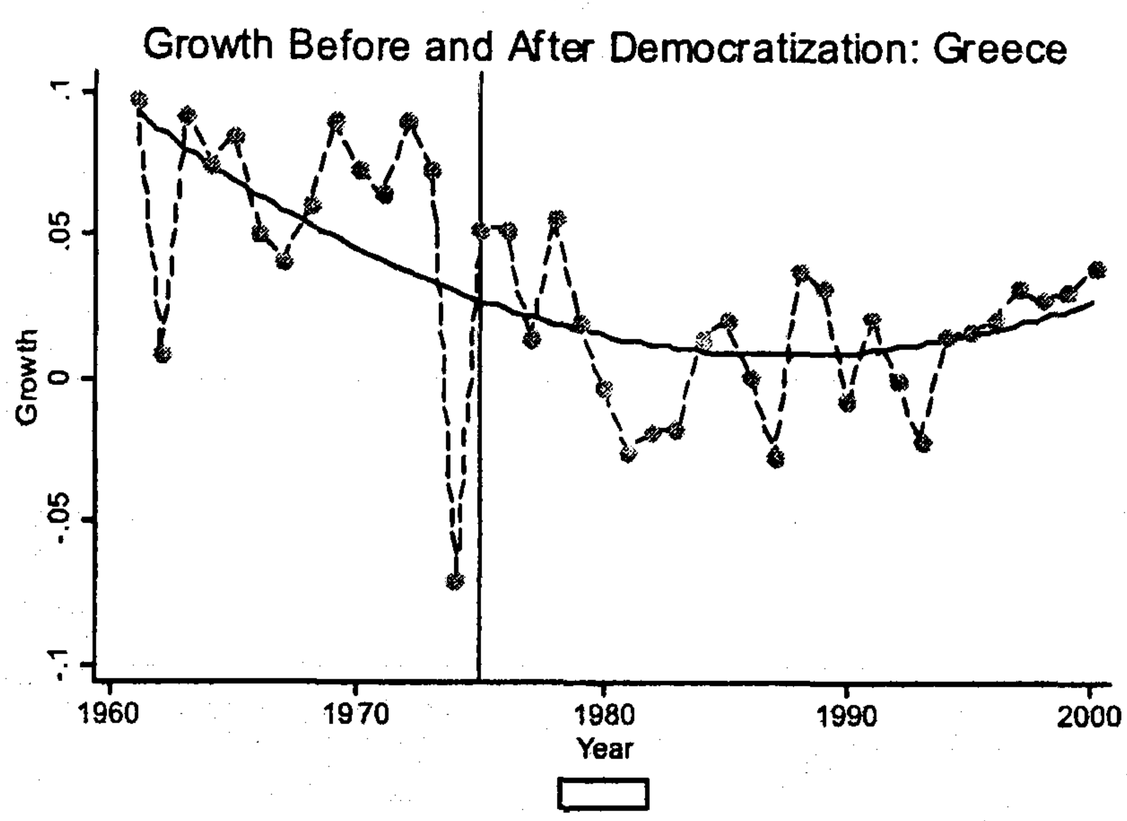


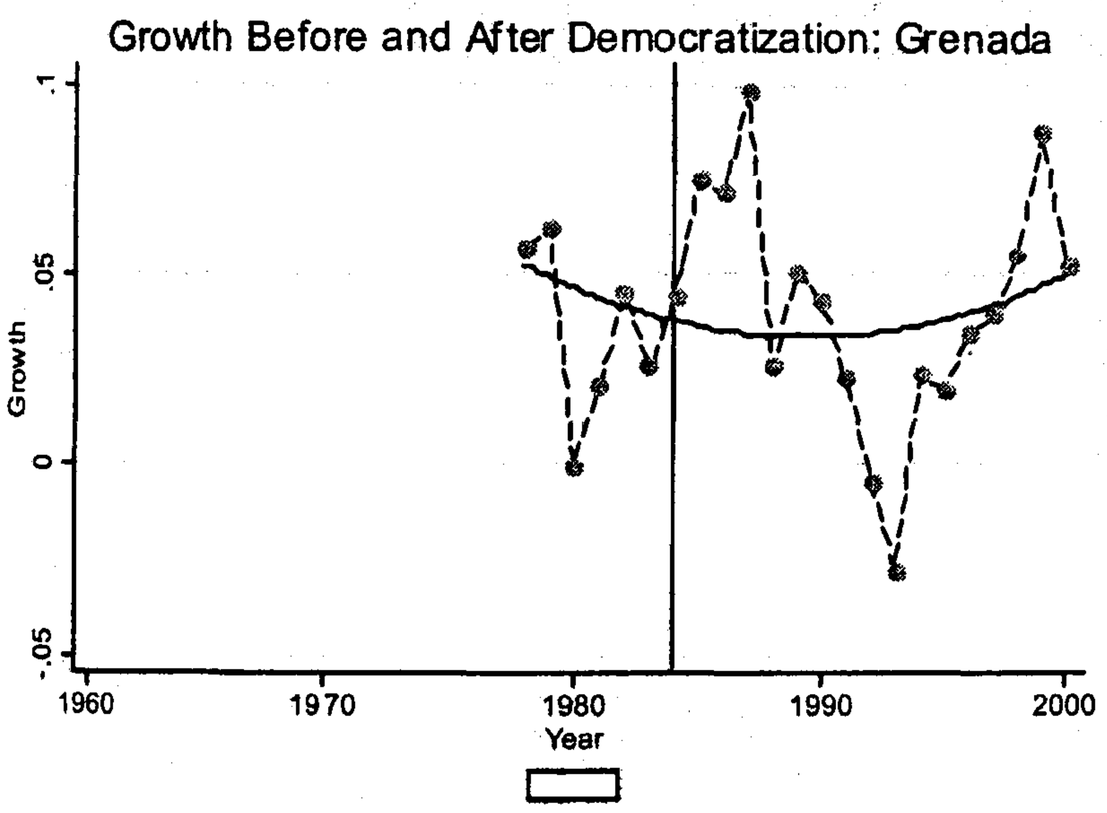


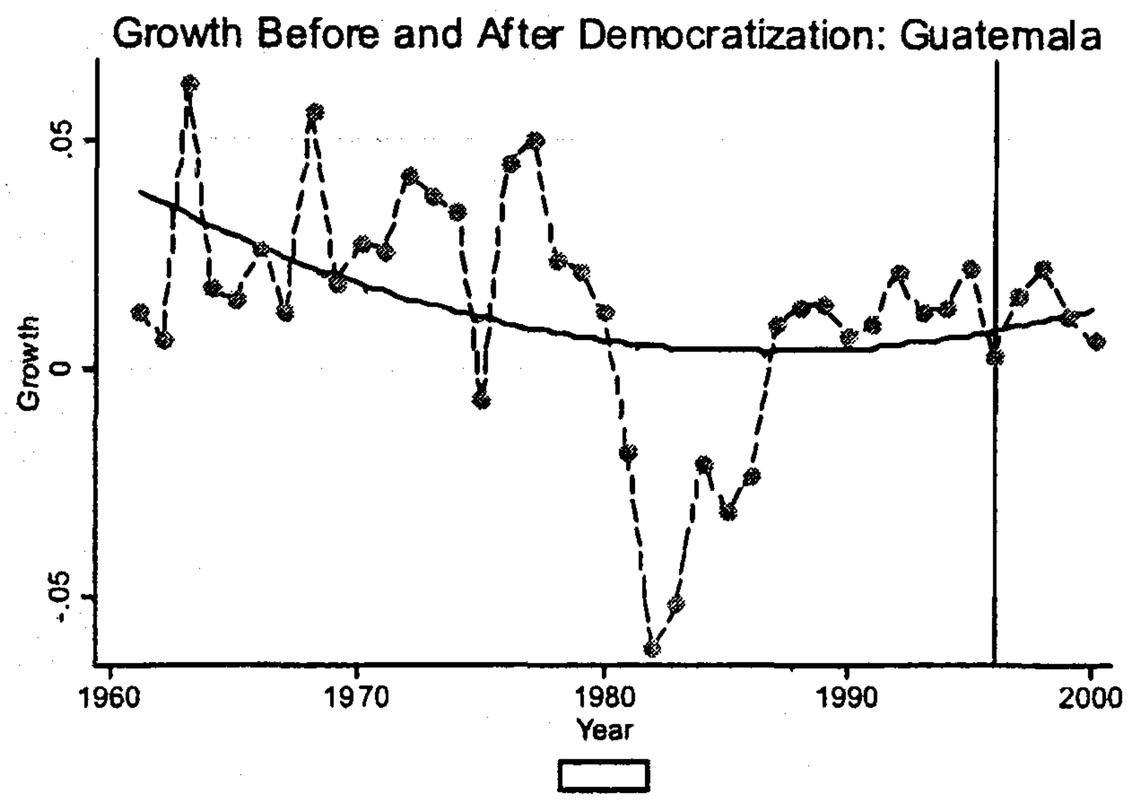


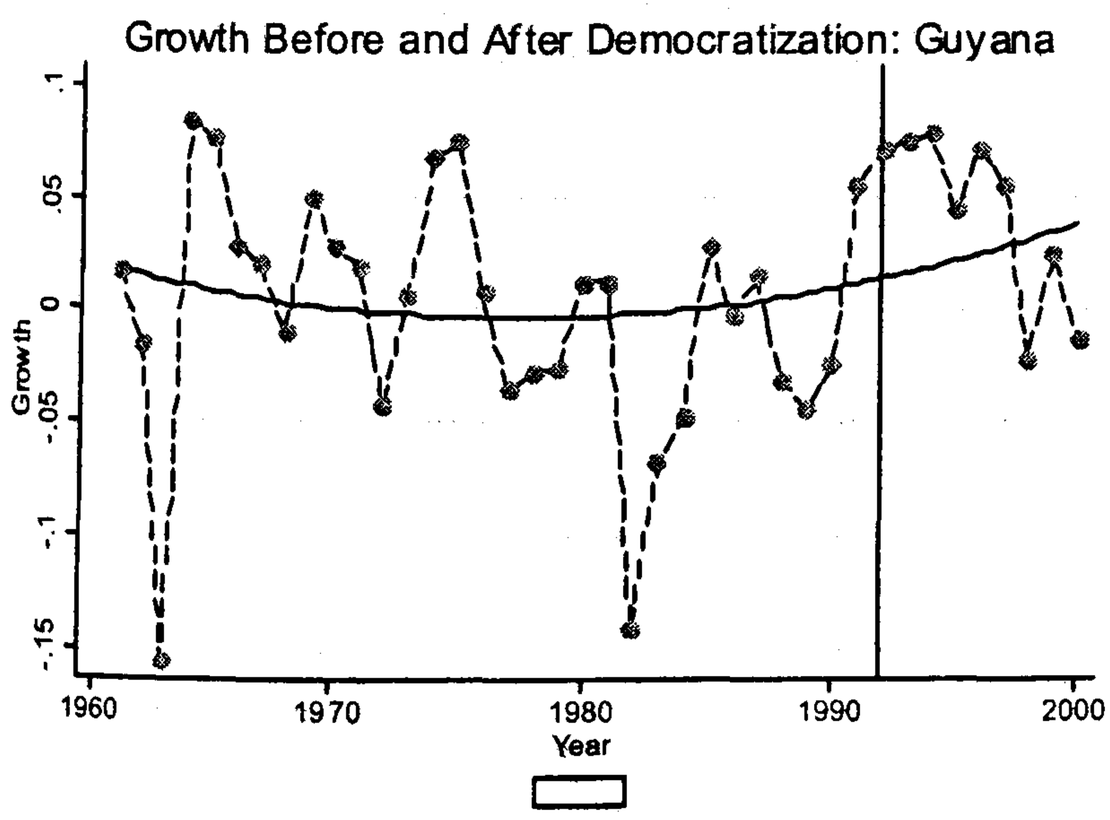
SUPPLEMENTARY APPENDIX—COUNTRY GRAPHS AROUND DEMOCARTIZATION

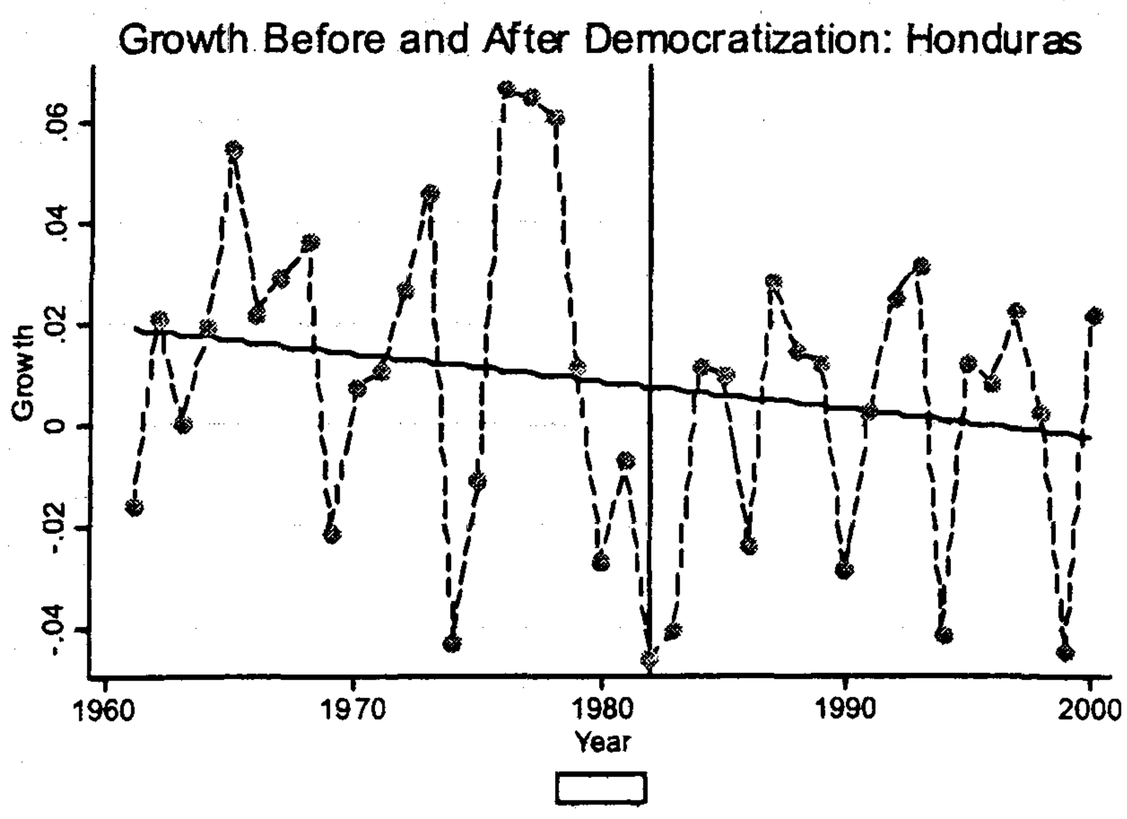












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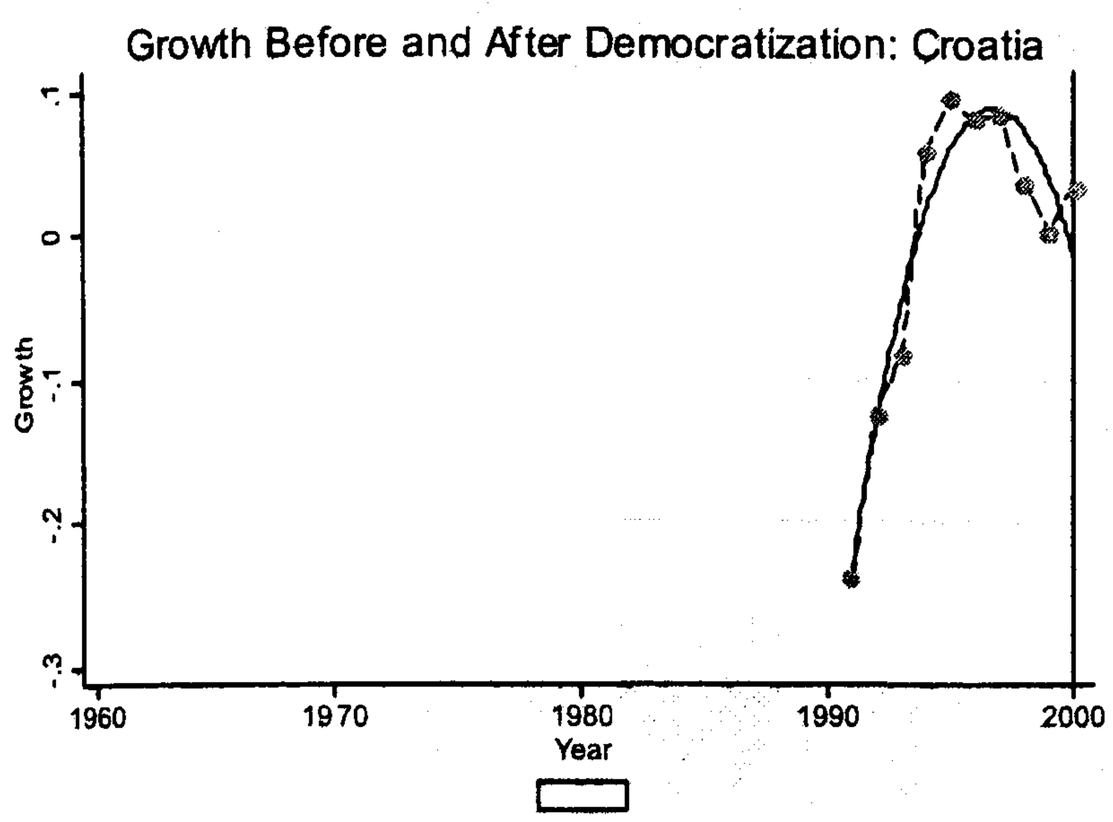
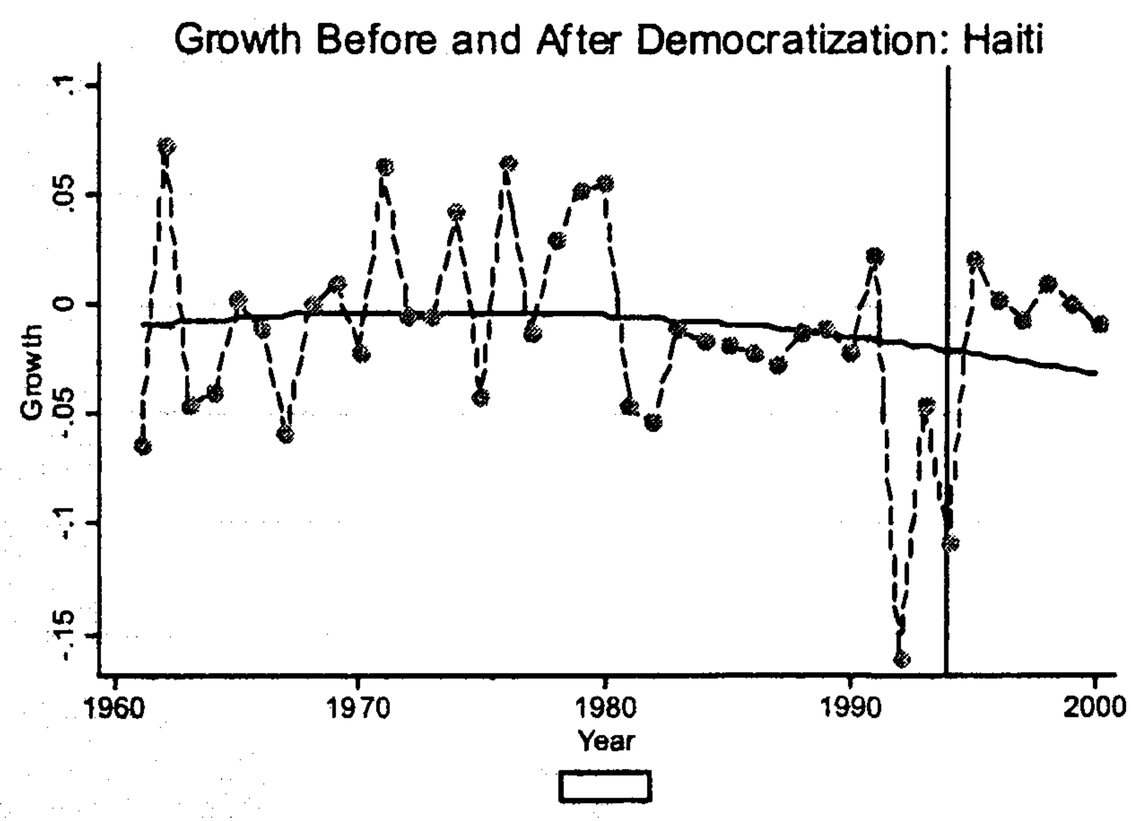
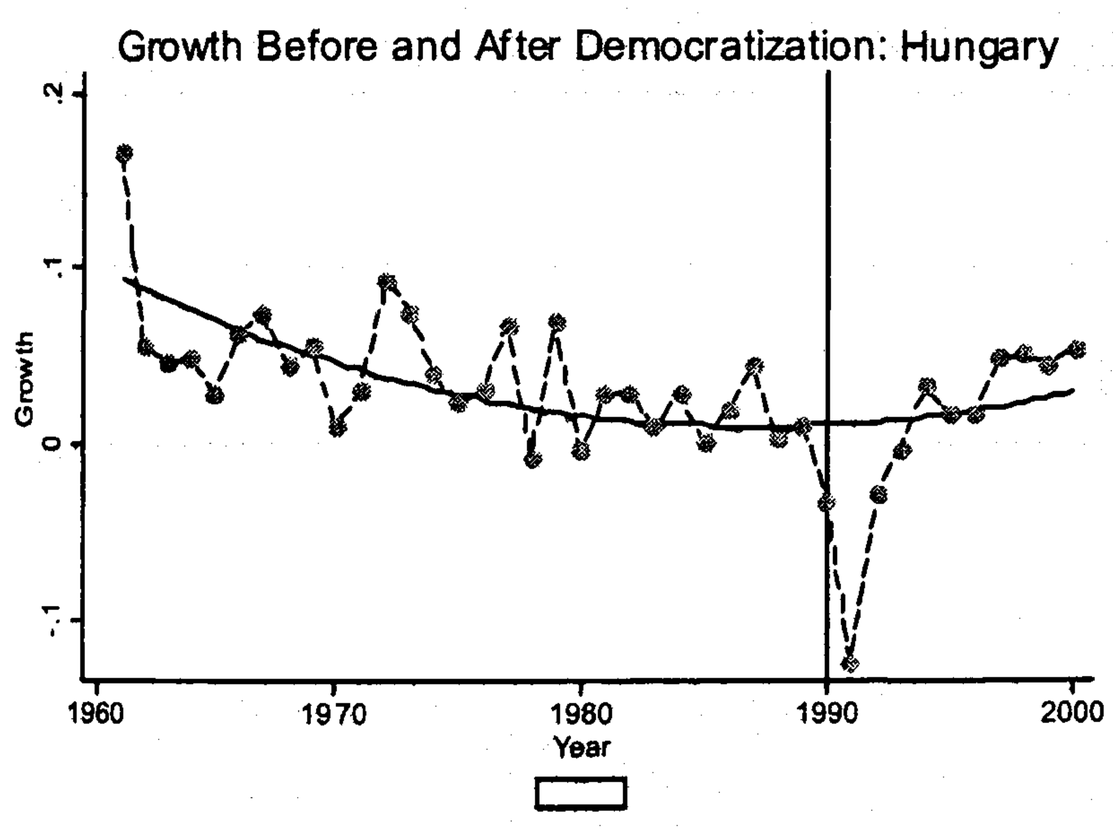
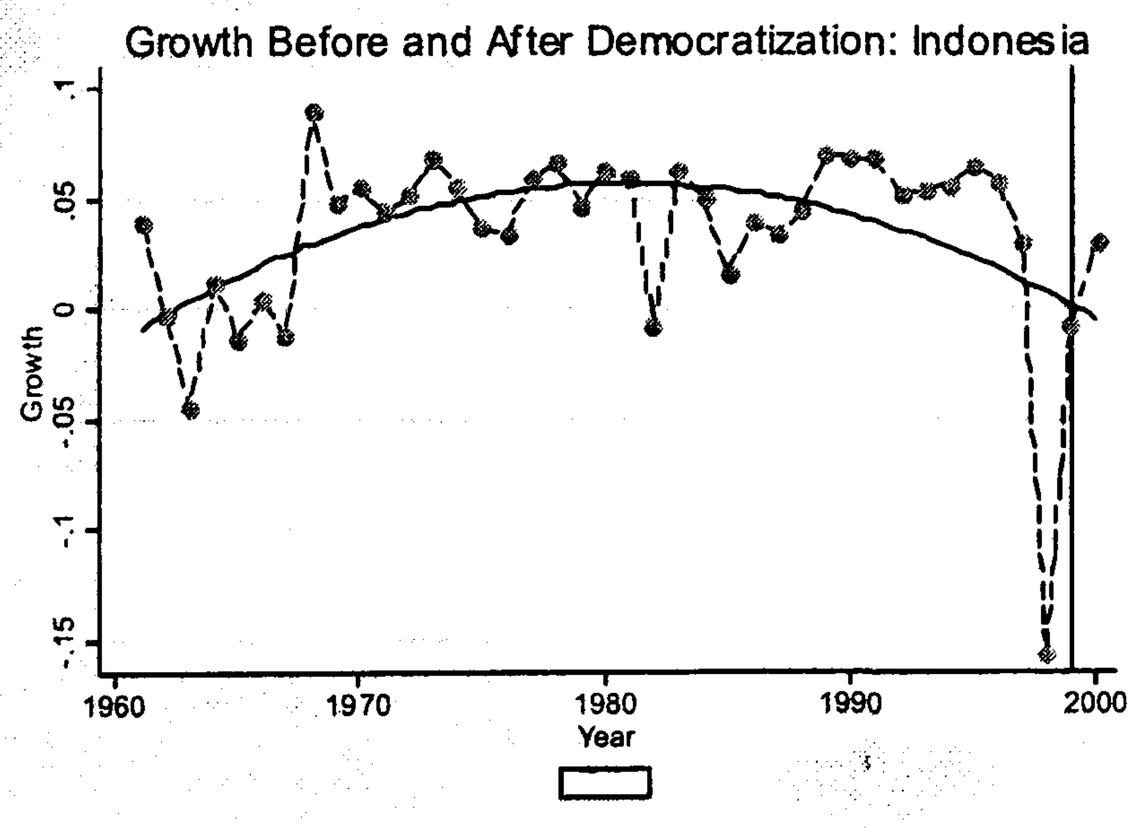
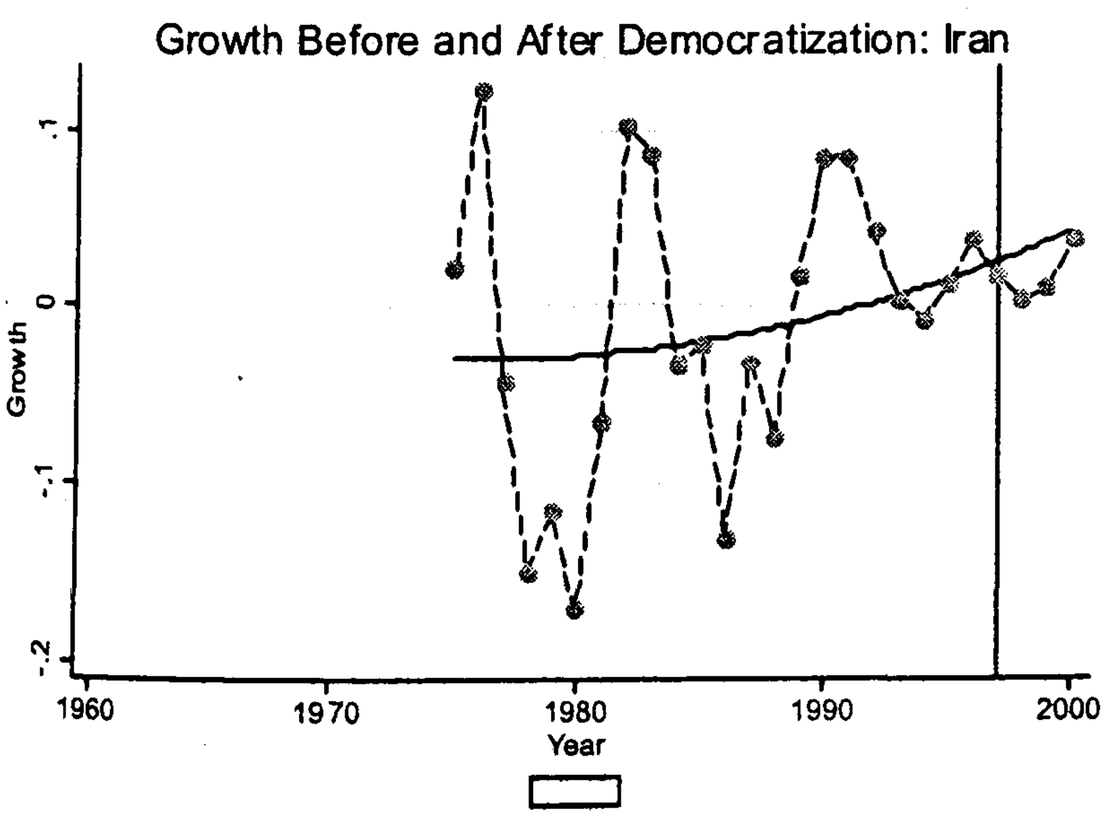
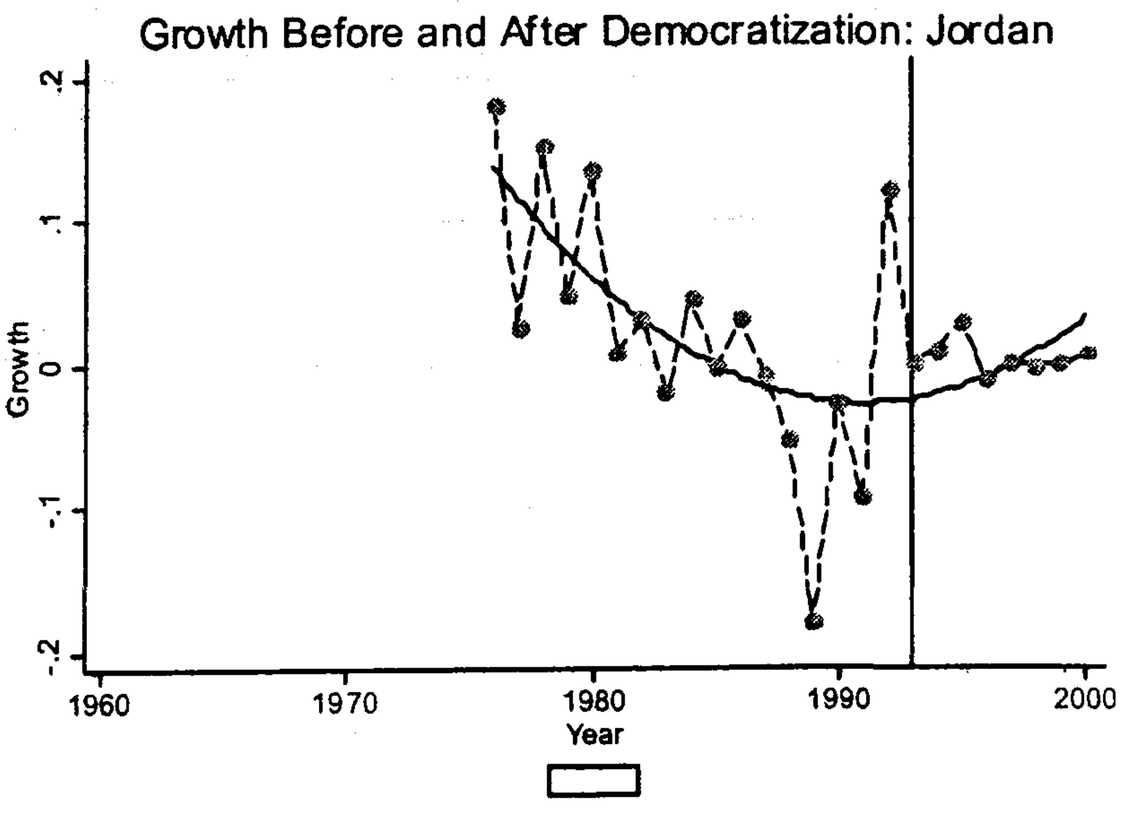




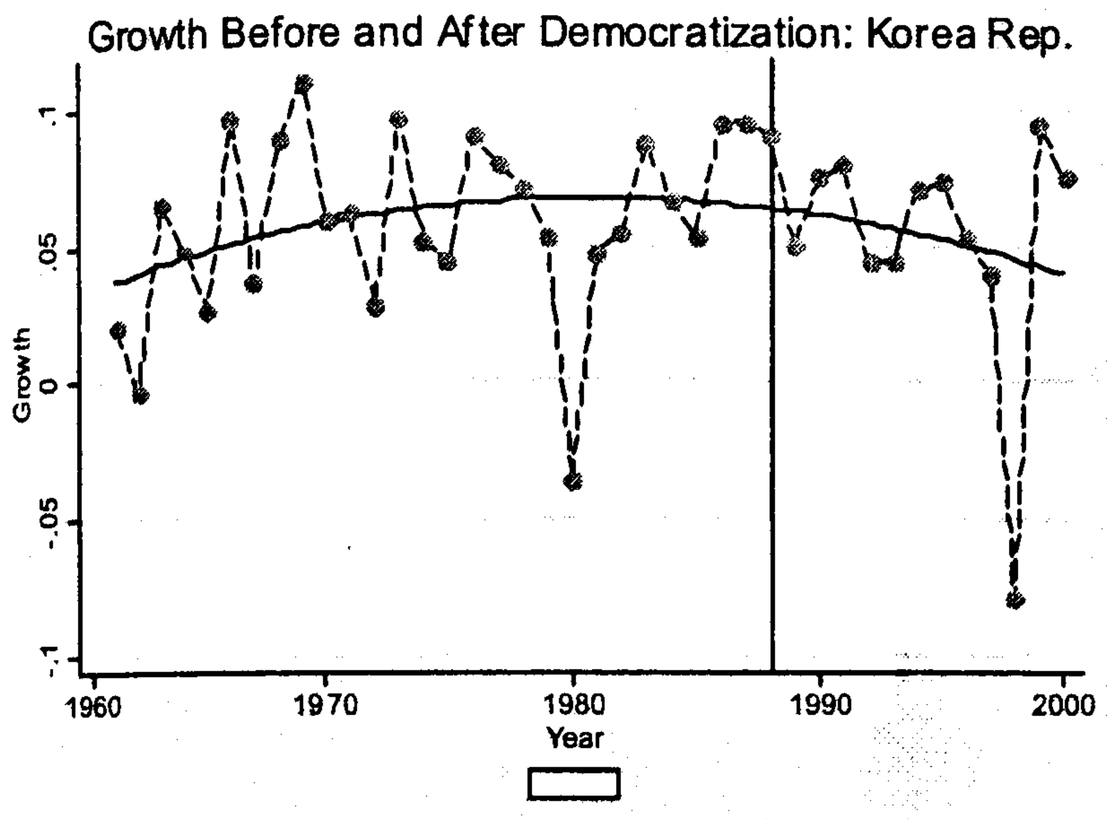


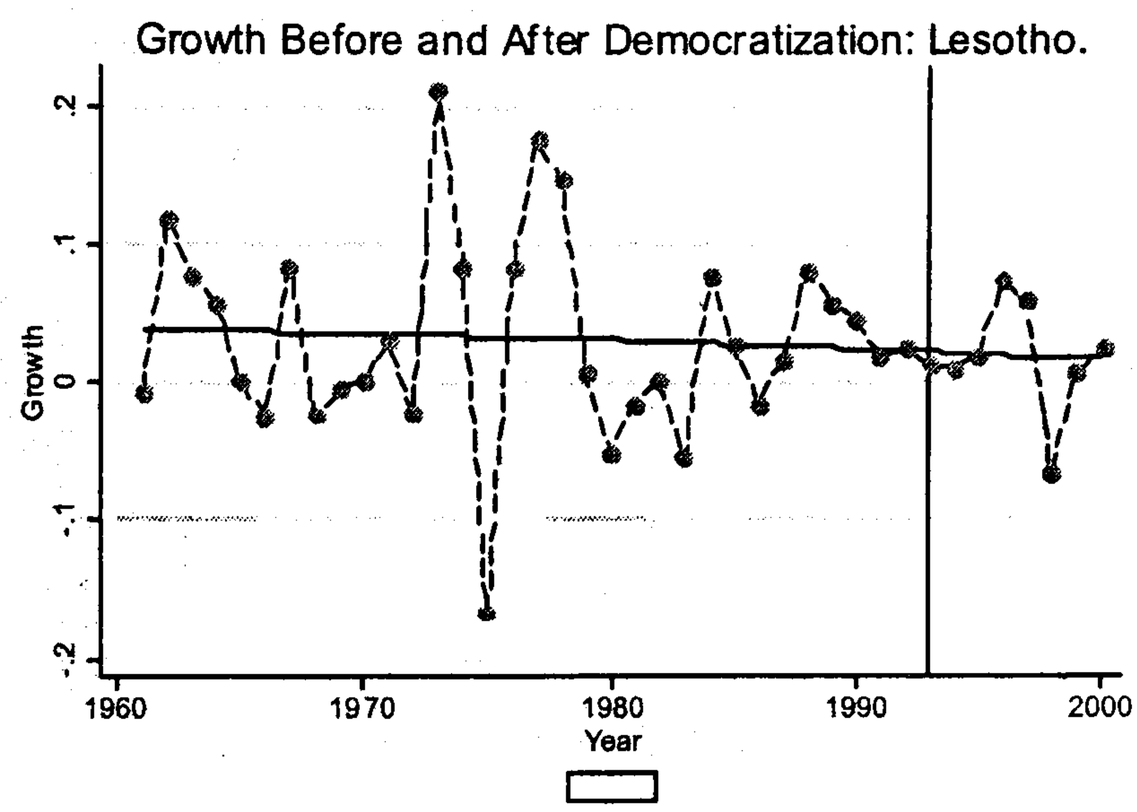


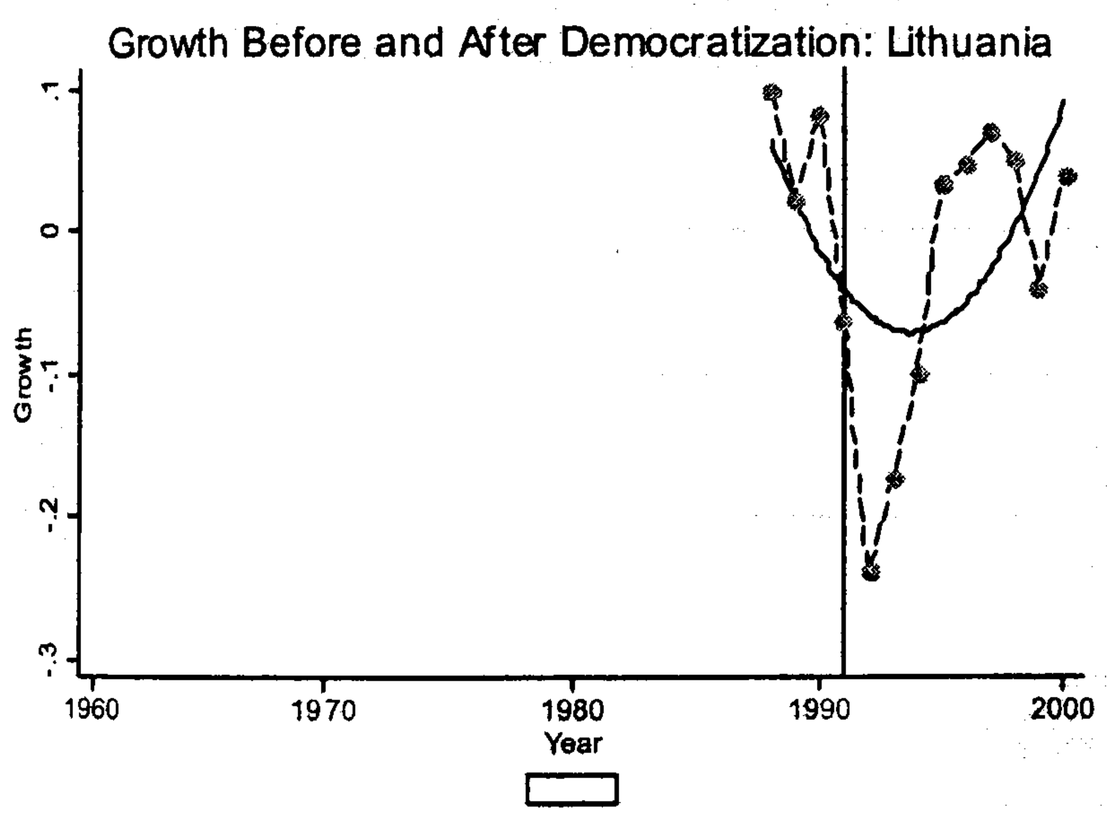


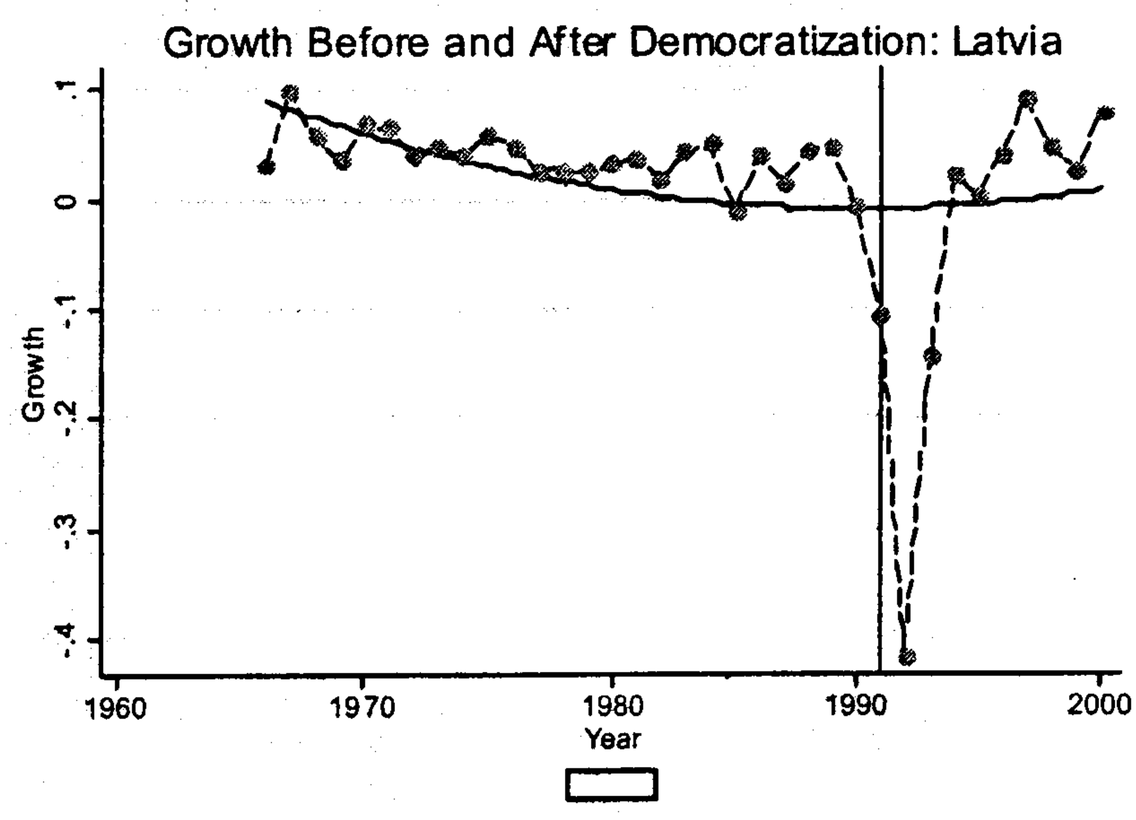


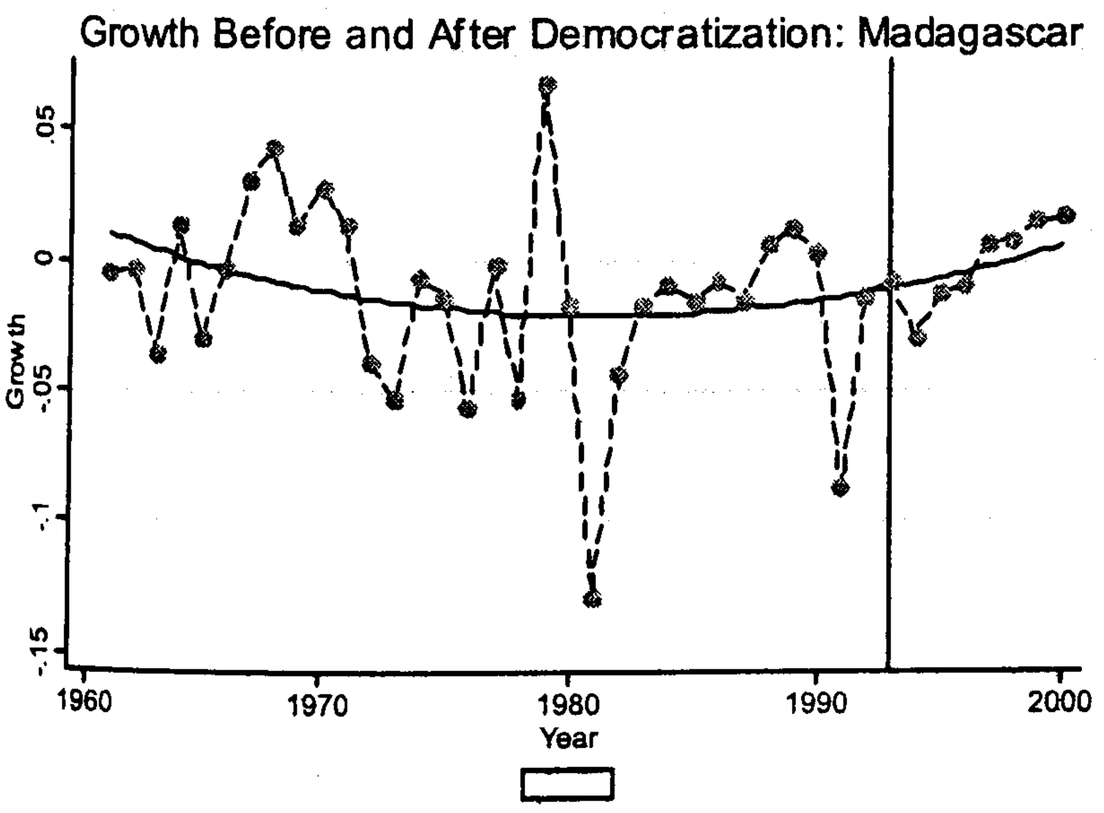
SUPPLEMENTARY APPENDIX—COUNTRY GRAPHS AROUND DEMOCARTIZATION

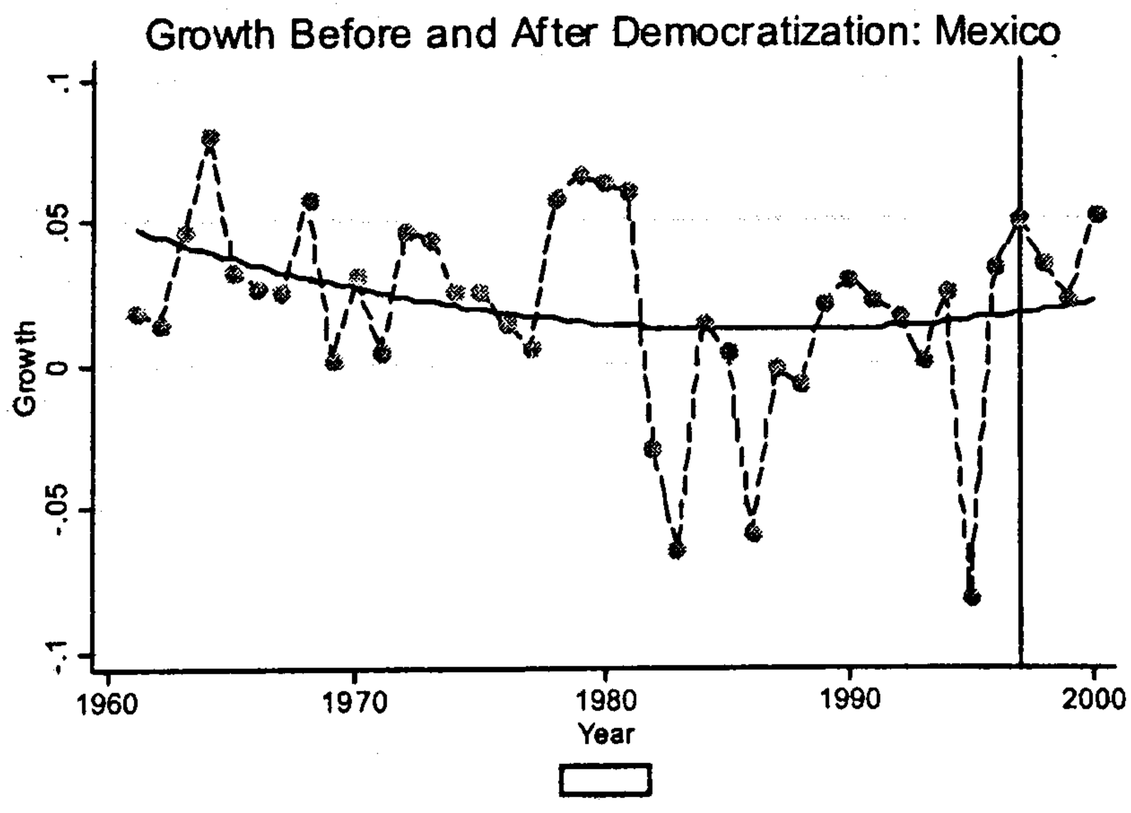












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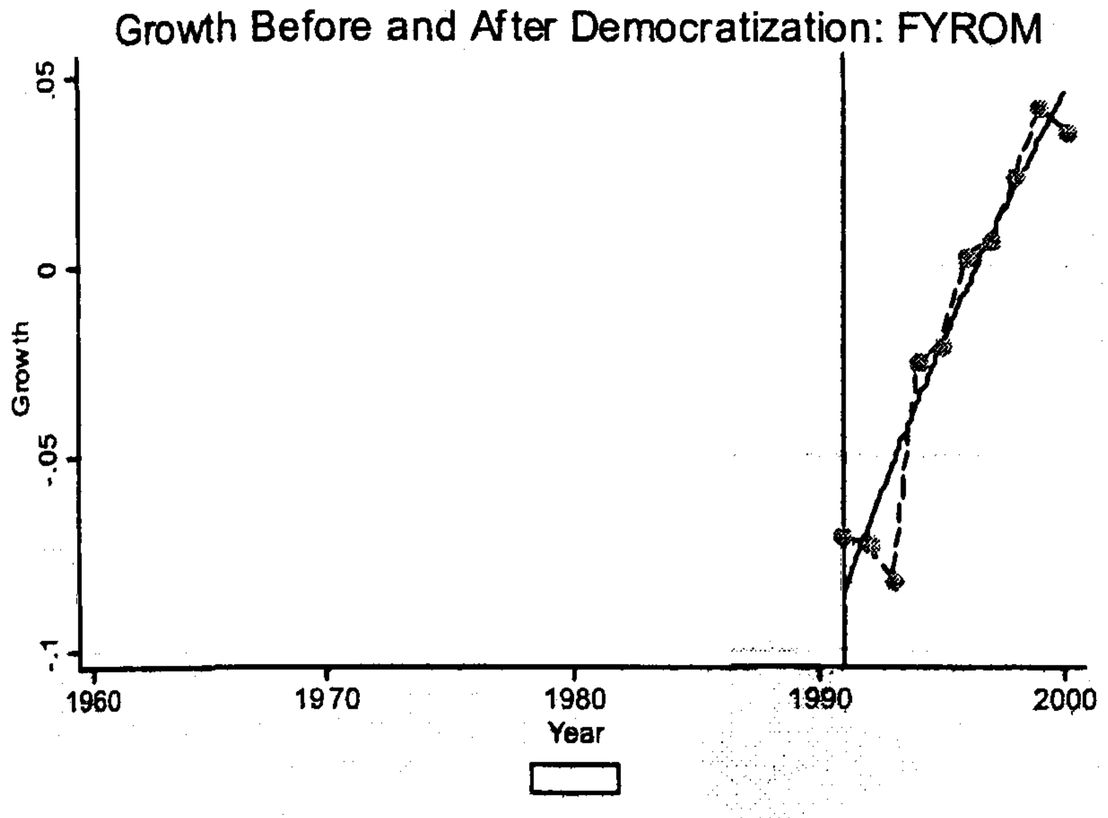
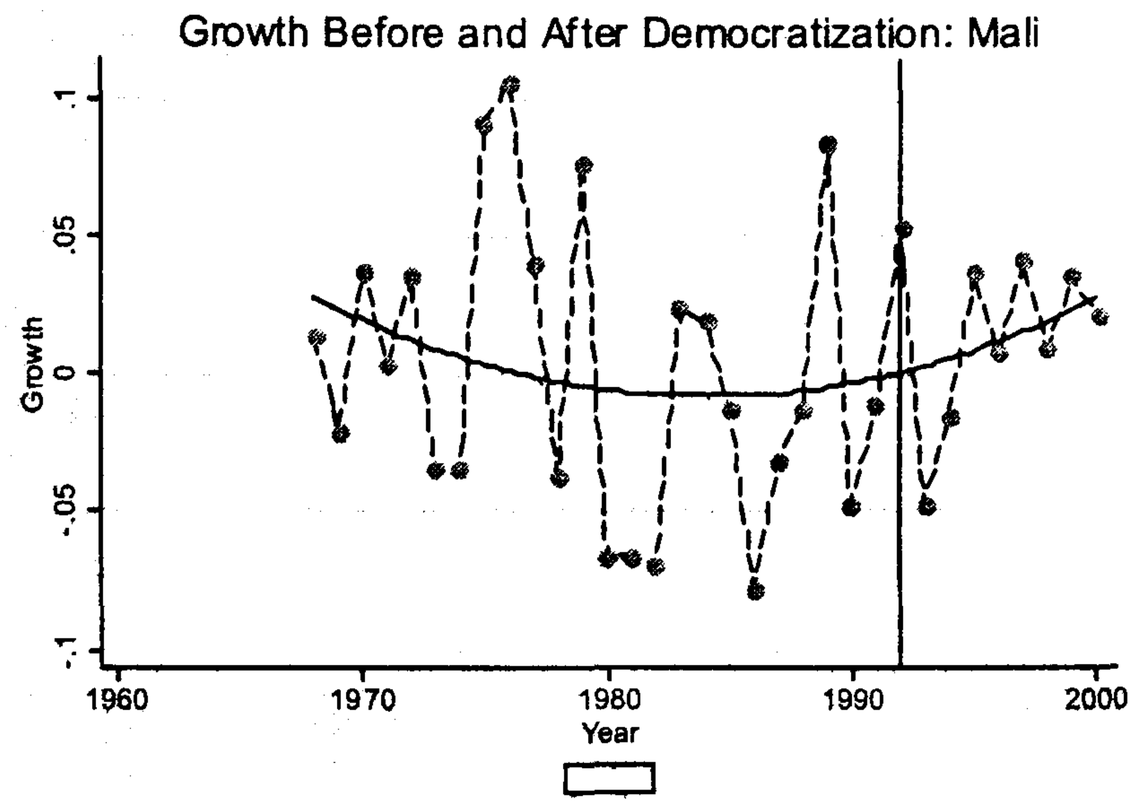
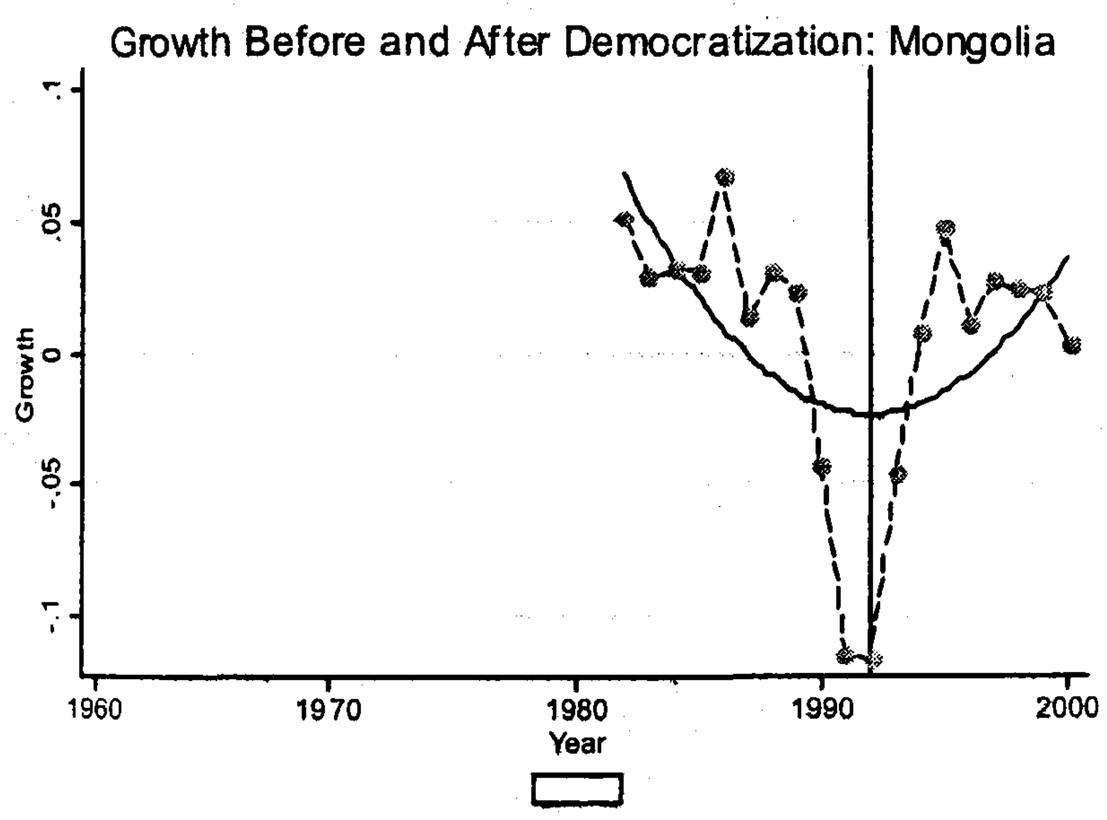
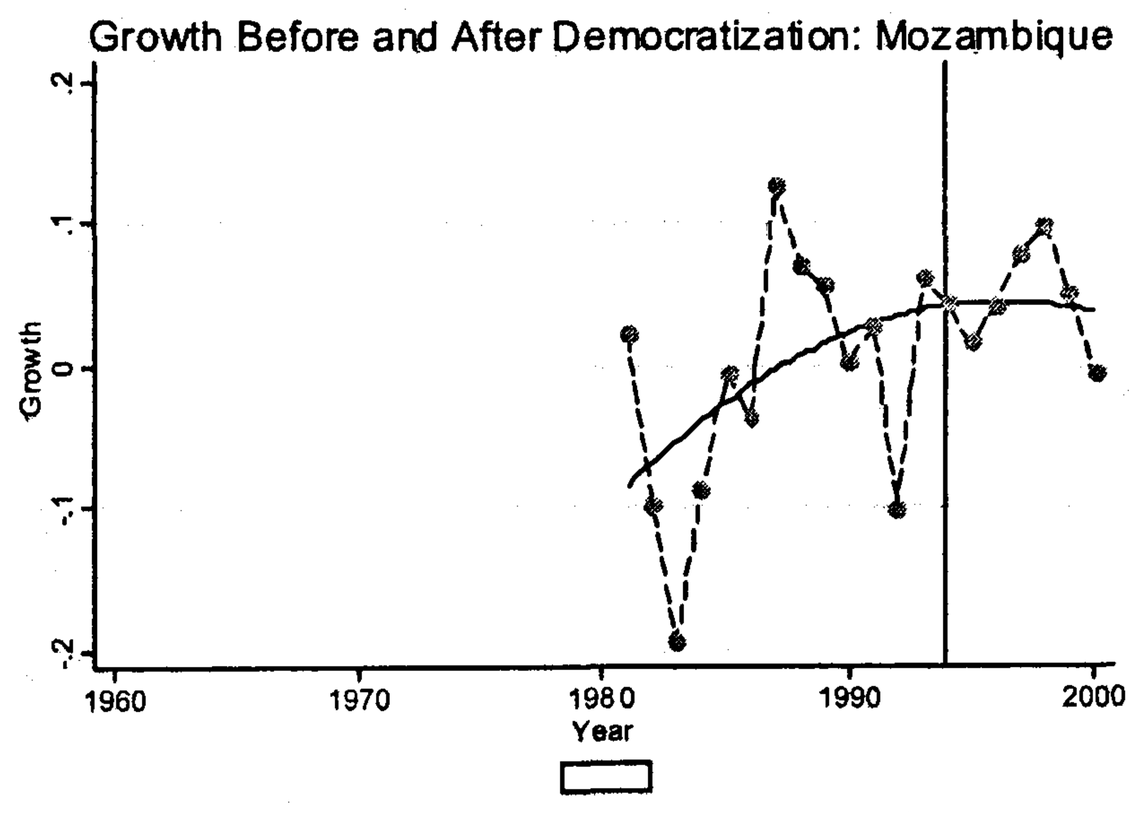
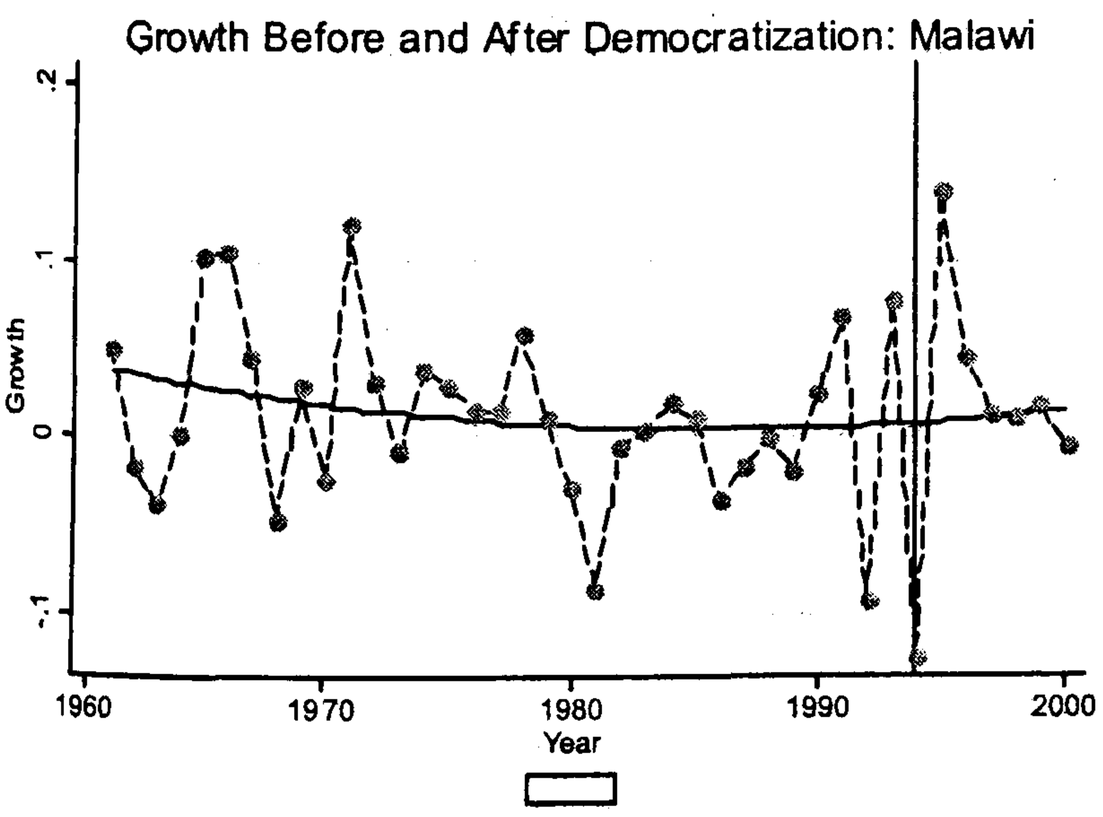
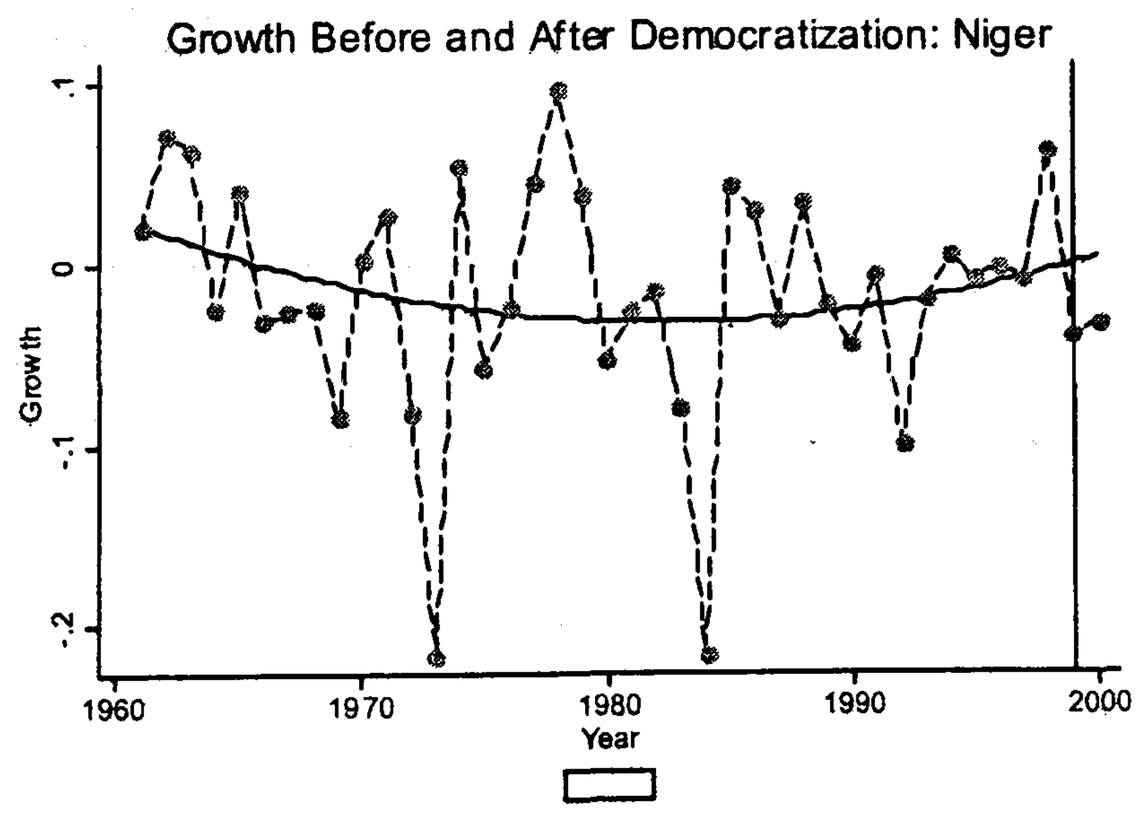




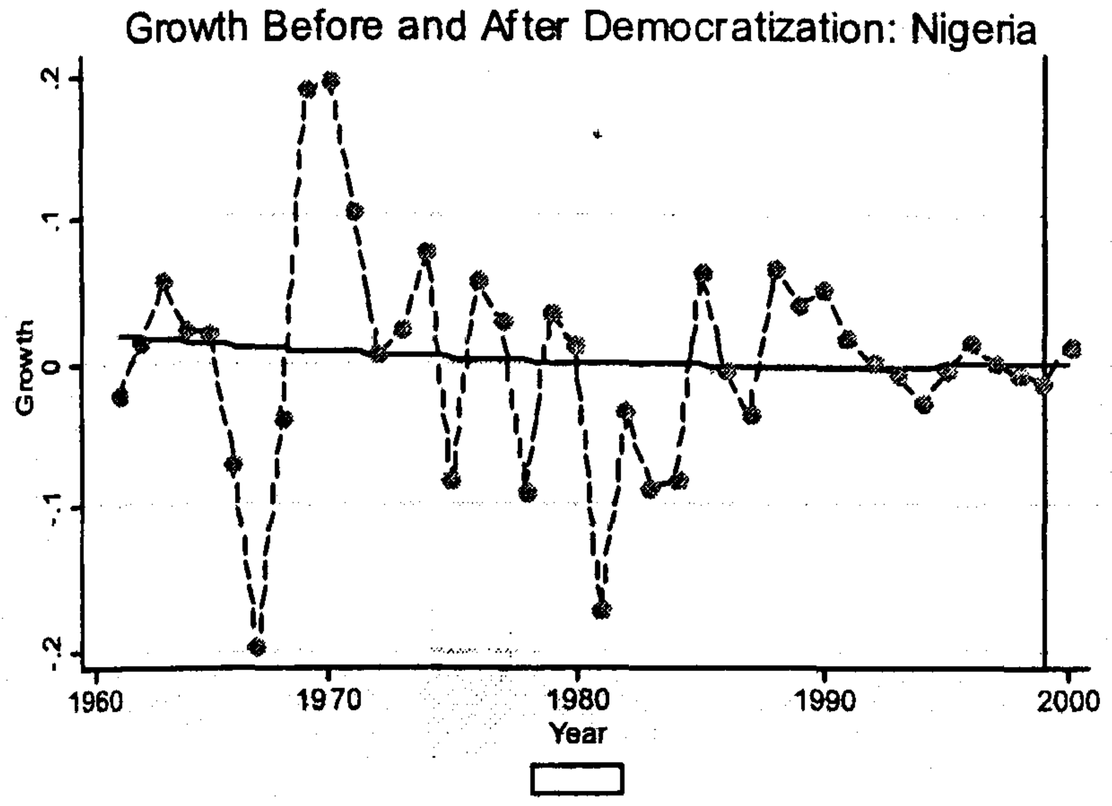


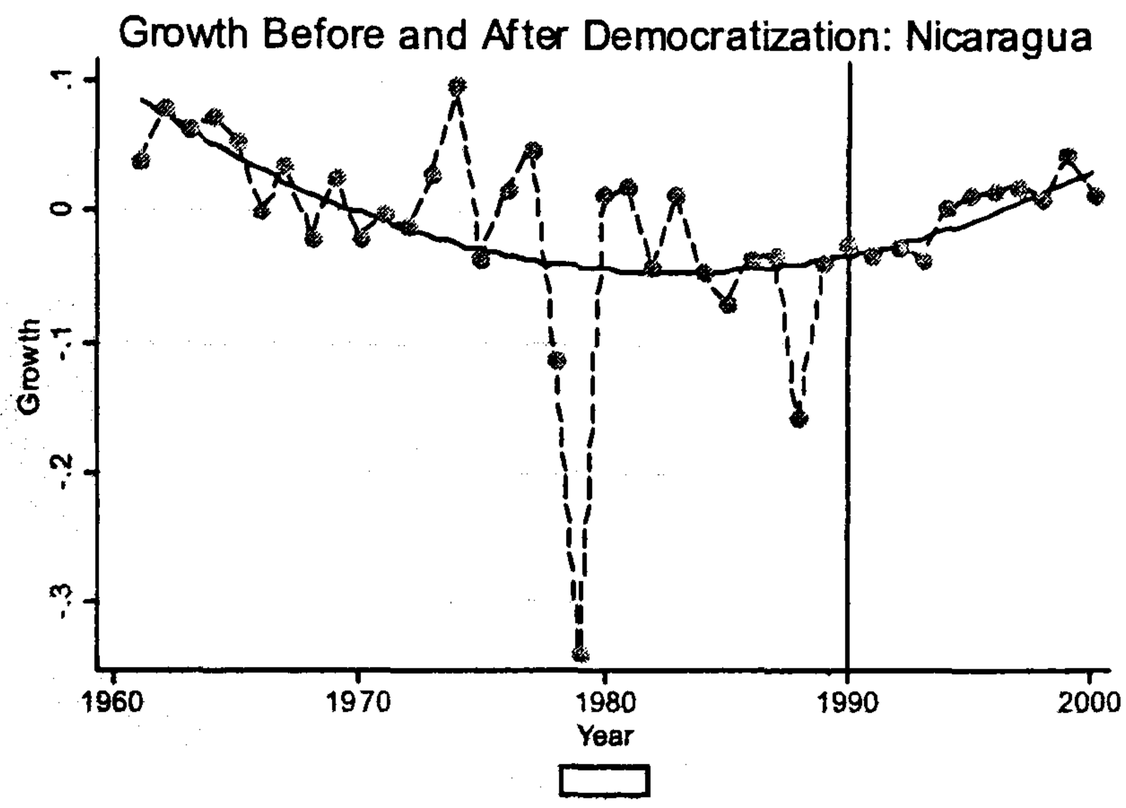


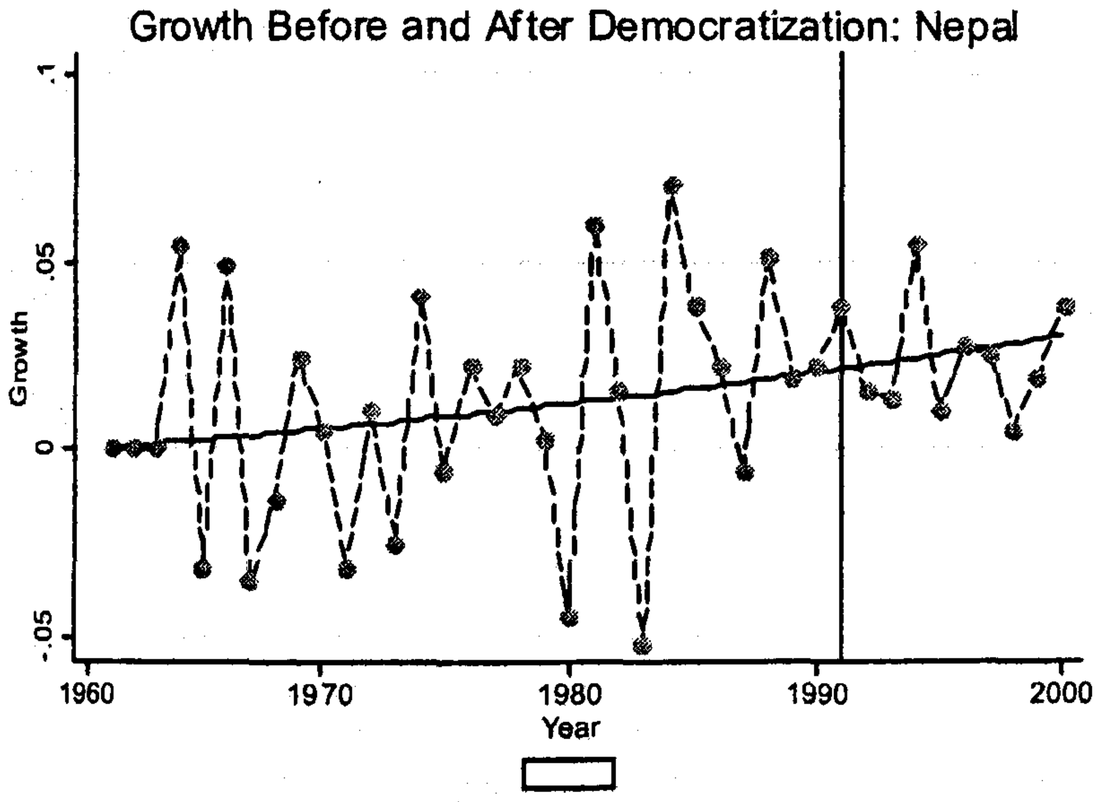


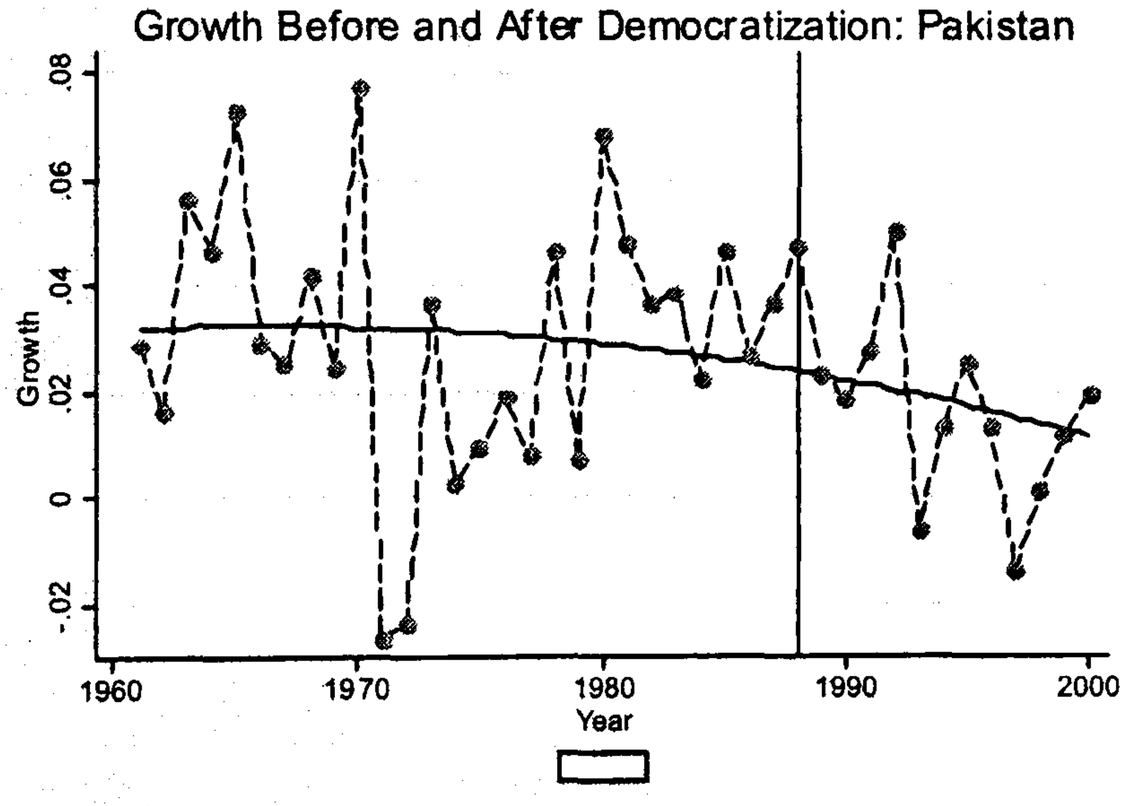


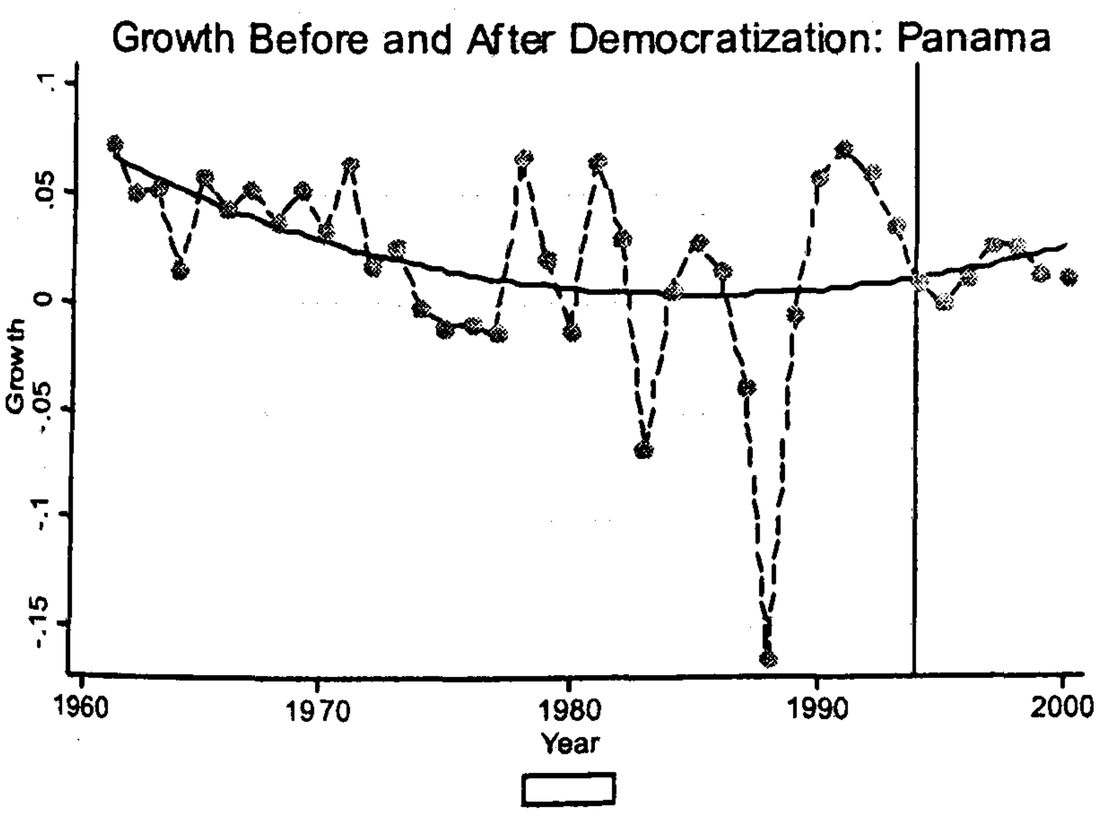
SUPPLEMENTARY APPENDIX—COUNTRY GRAPHS AROUND DEMOCARTIZATION

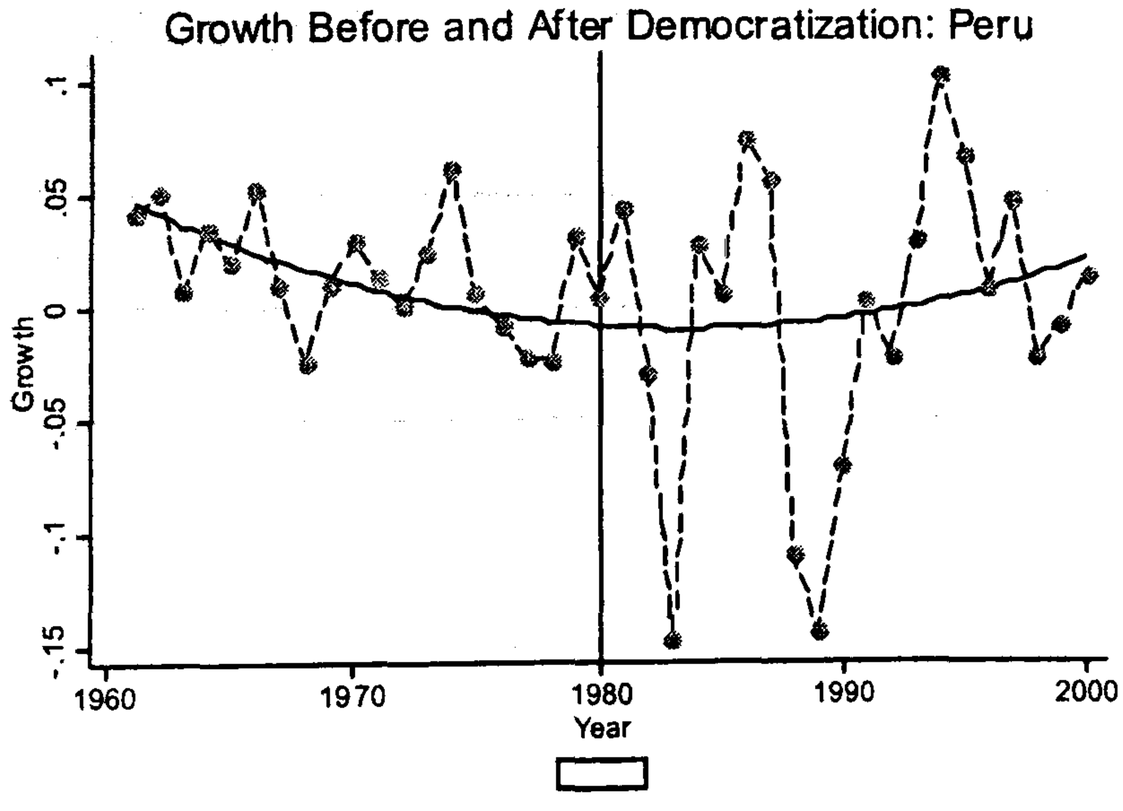




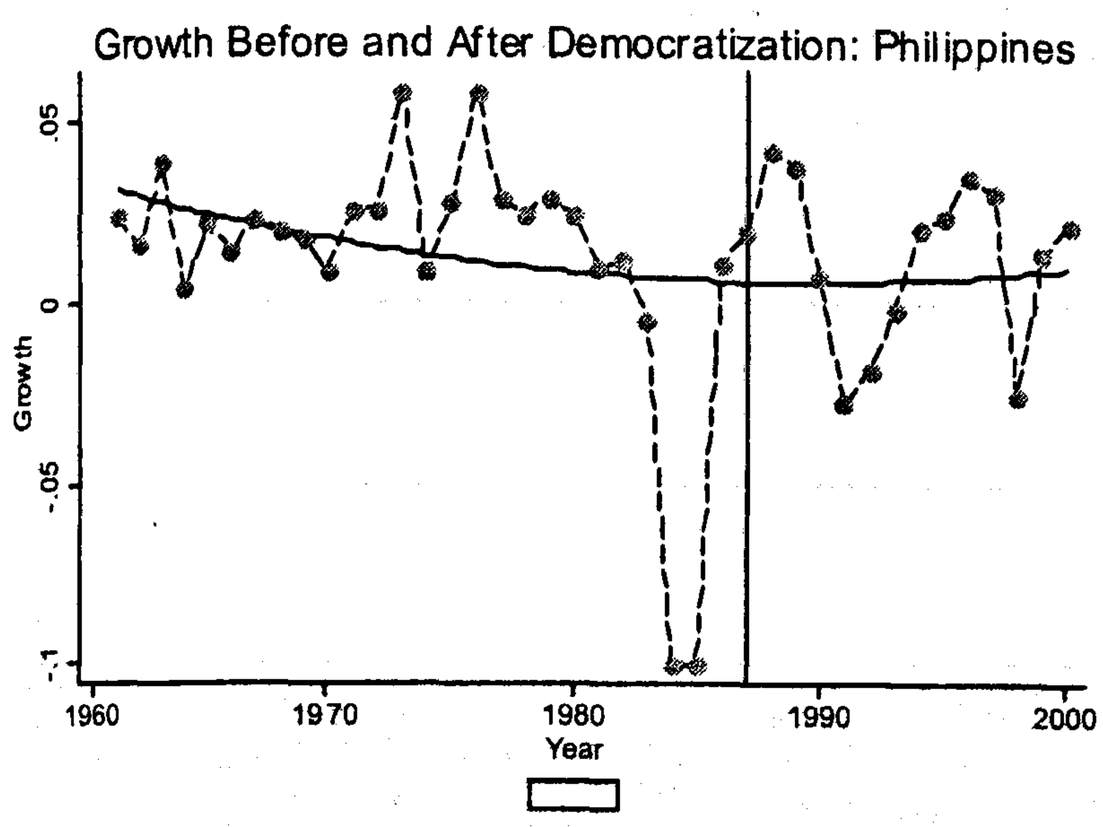


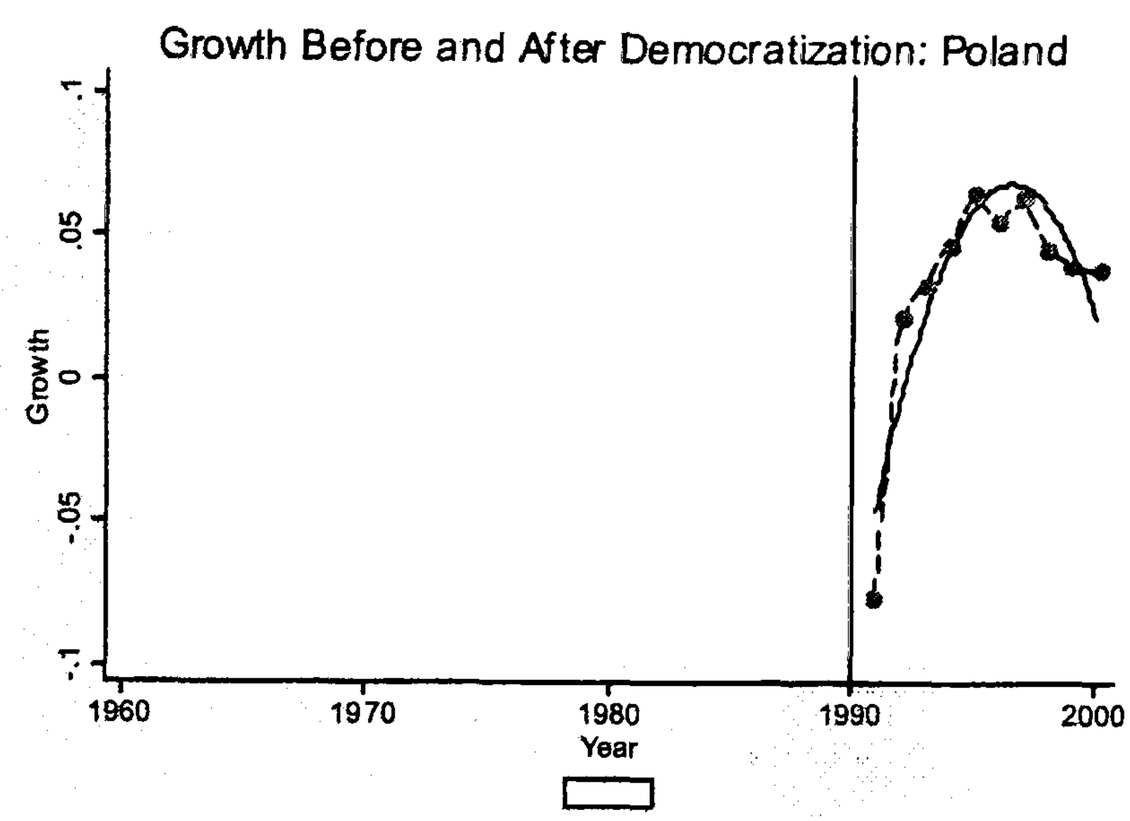


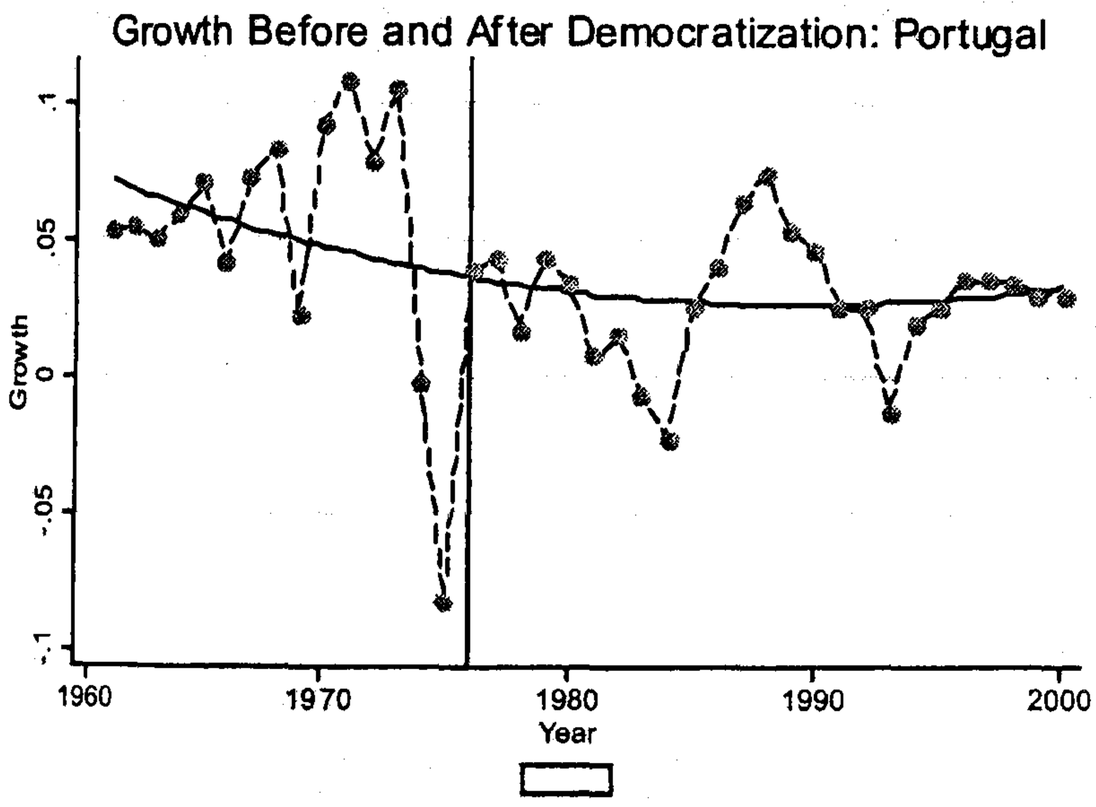


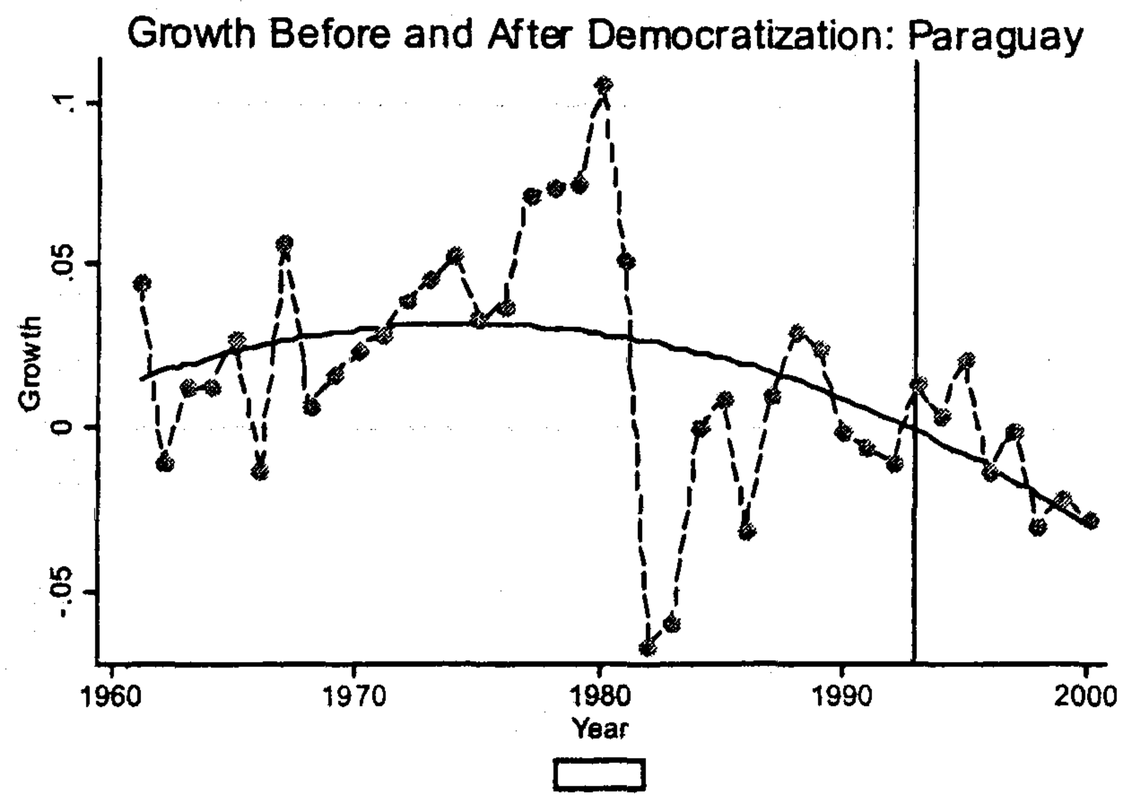


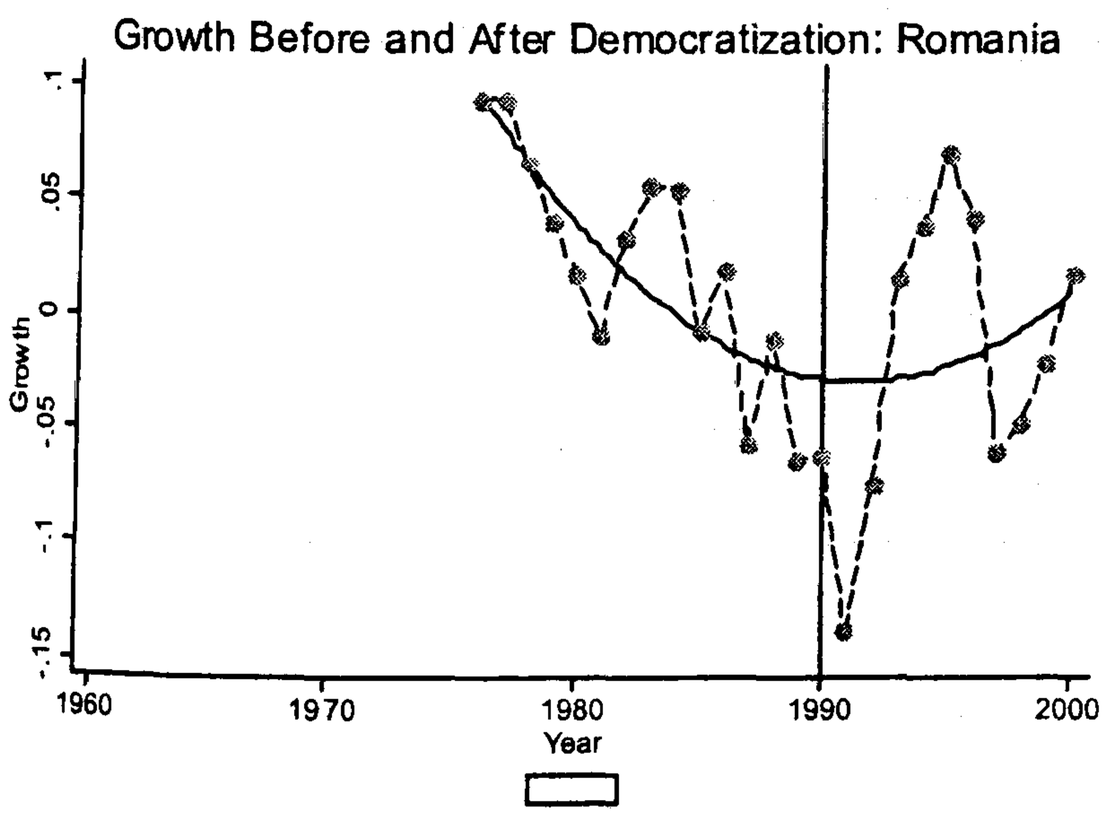
SUPPLEMENTARY APPENDIX—COUNTRY GRAPHS AROUND DEMOCARTIZATION

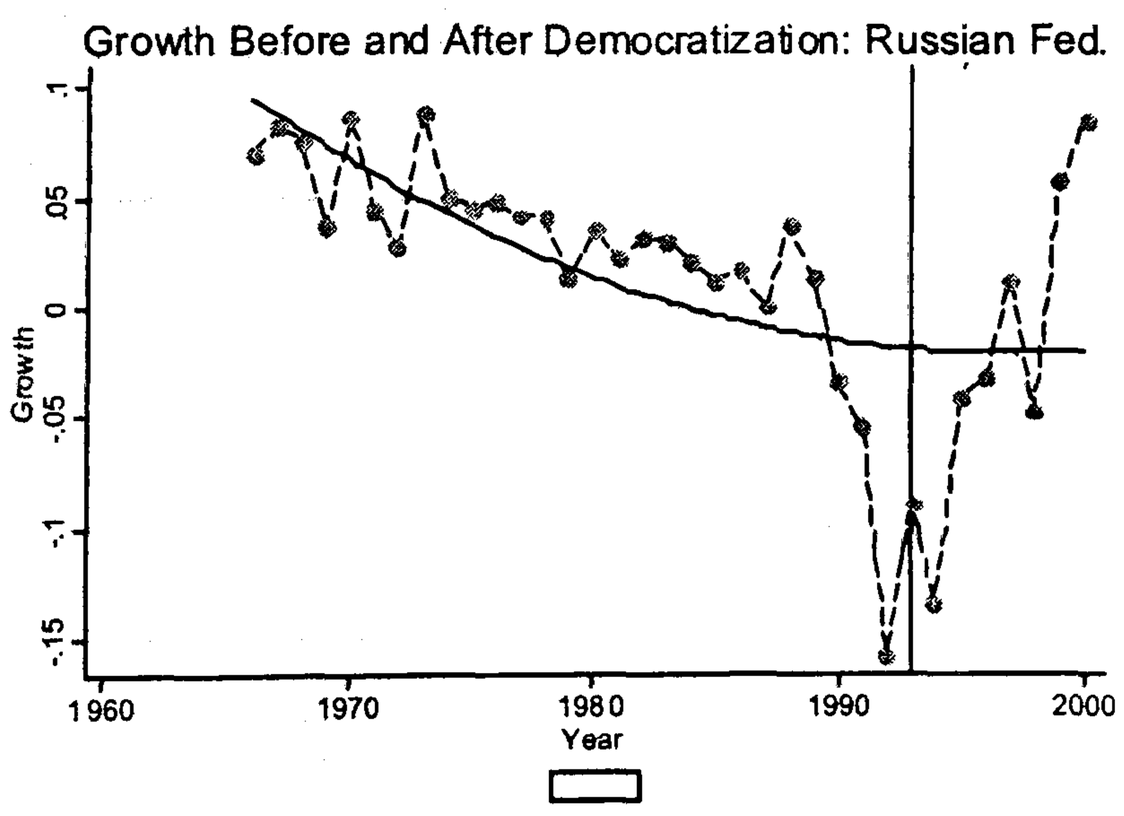












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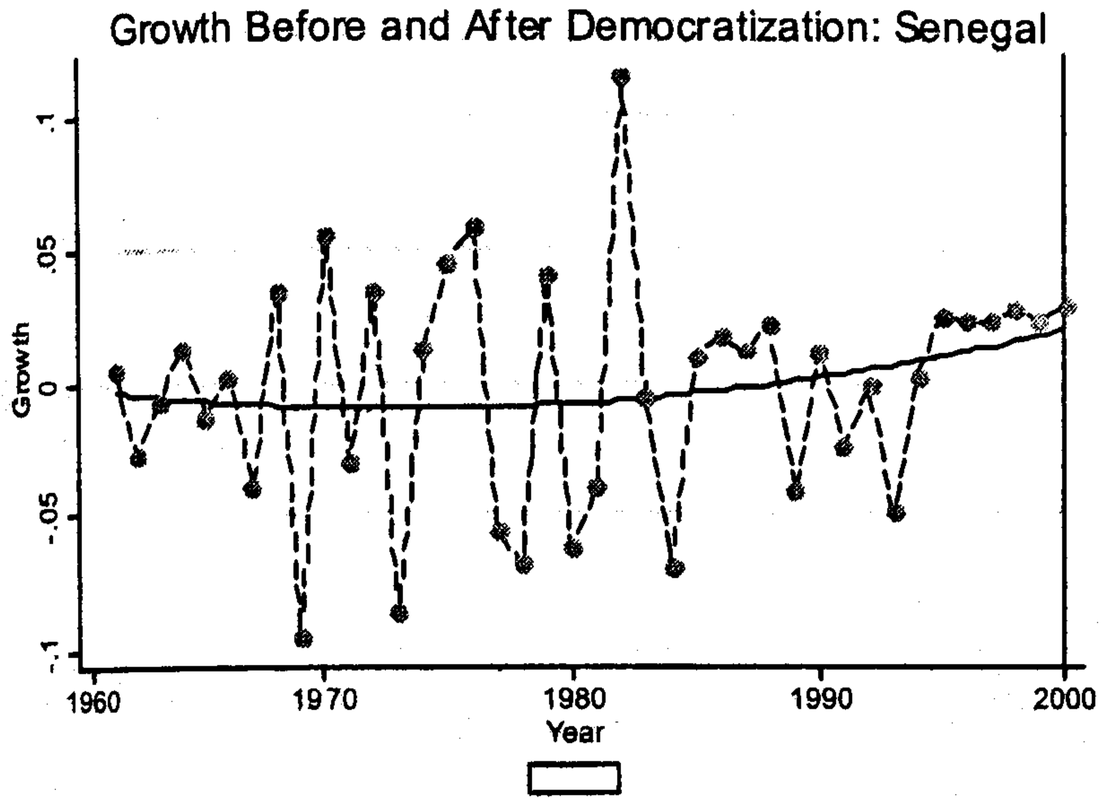

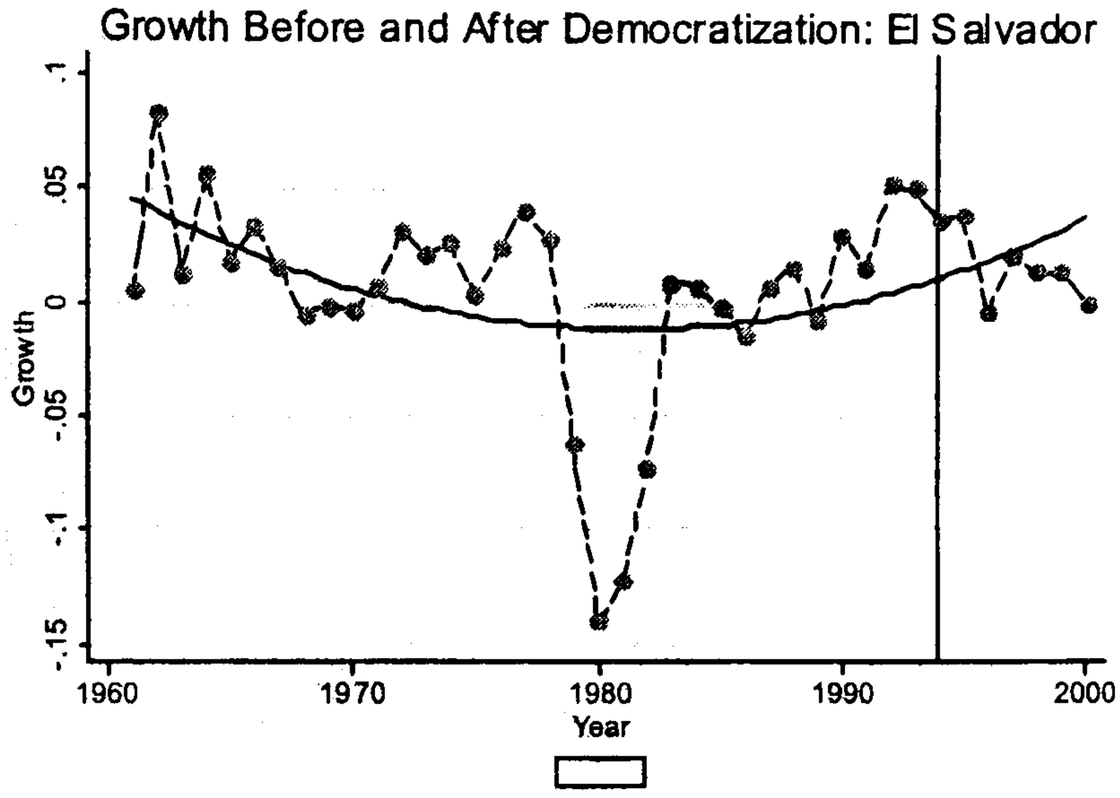

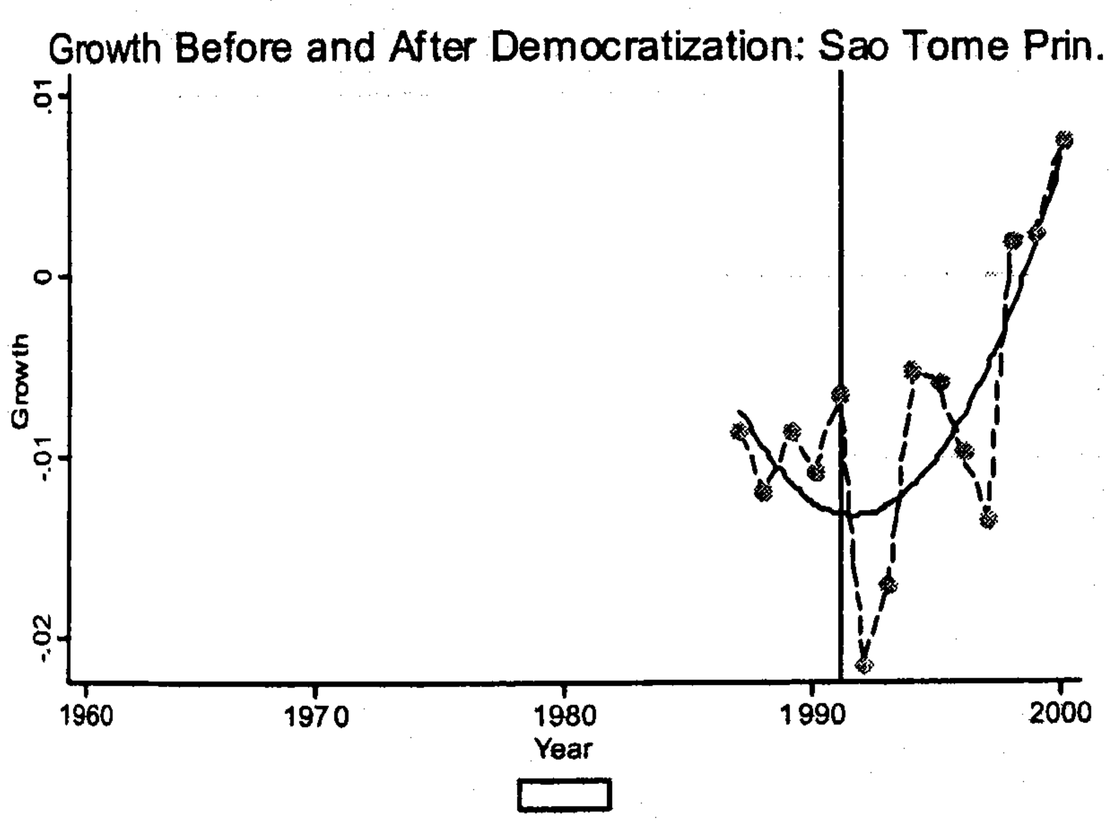

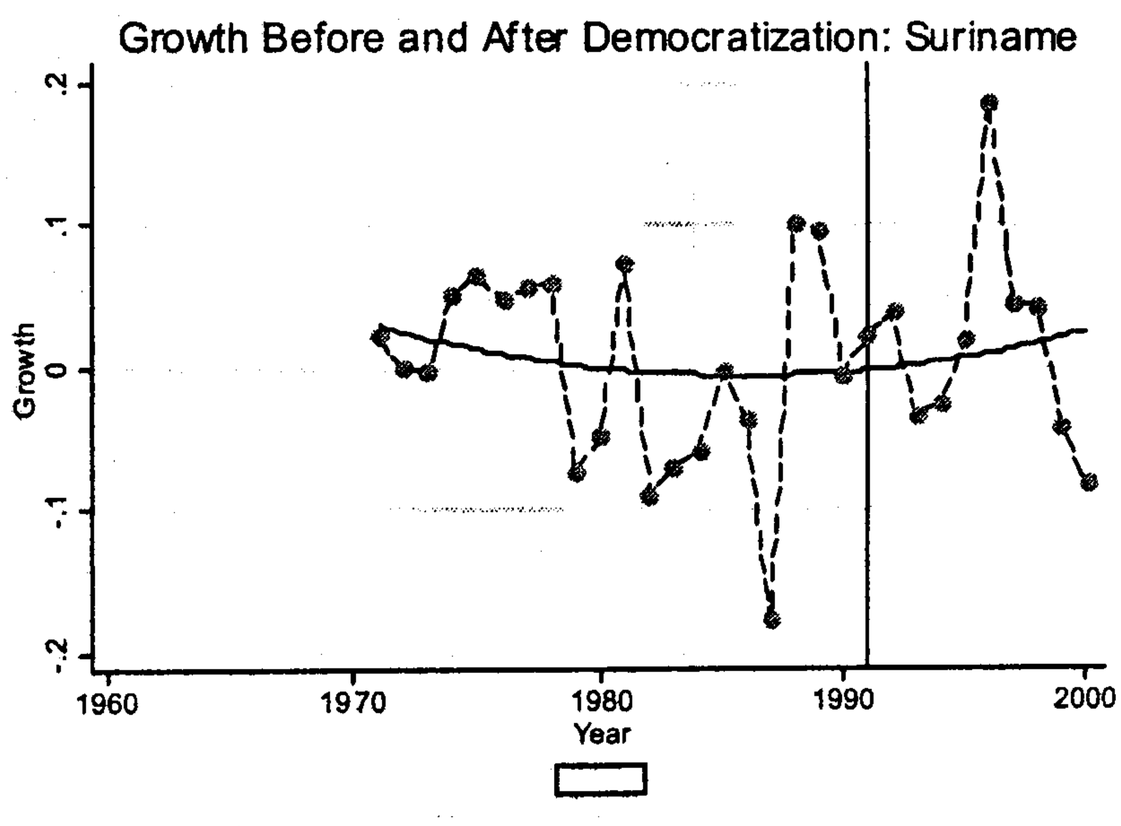

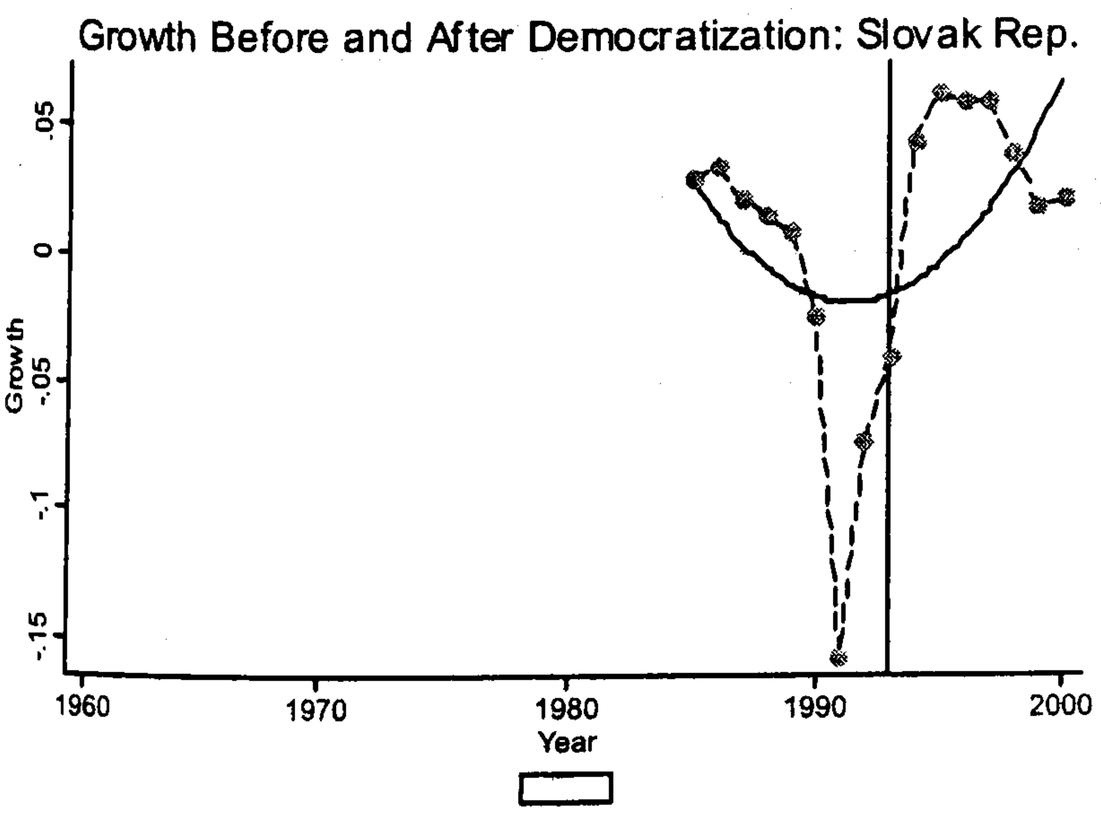

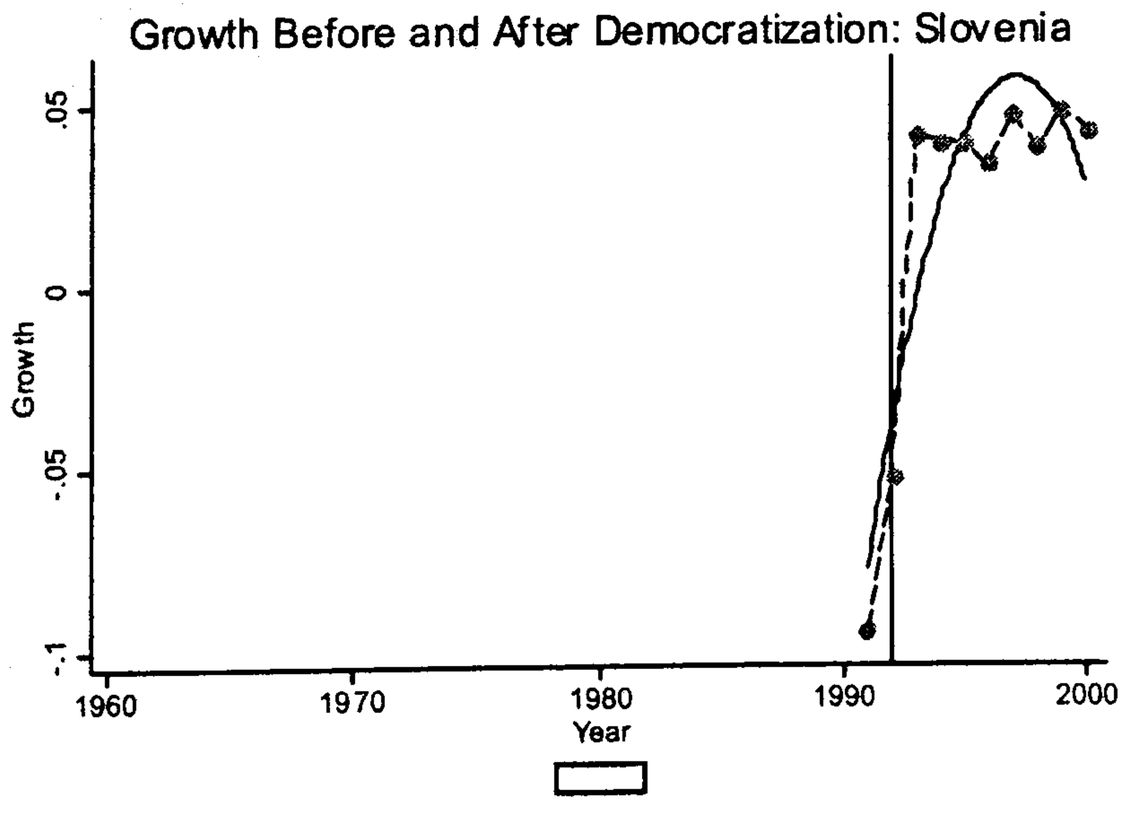

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