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Caught in a Poverty Trap? Testing for Single vs. Multiple Equilibrium Models of Growth

Abstract: We look for permanent effects to per capita GDP from exogenous, temporary shocks. Our shocks are temporary changes to the export revenues of small, open economies. We find no evidence that even the largest of these temporary shocks, in excess of 9.7% of GDP, produce permanent effects to the growth path of per capita GDP. The inability to reject a single-equilibrium world with shocks of this magnitude suggests that multiple-equilibria, if they exist, are too widely separated to be policy-relevant. Current aid initiatives, which are of a similar magnitude, are not likely to deliver transition to a higher growth path.

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1 Introduction

Why do poor countries remain poor? Is it because they have chosen poor policies – which might be amended – or because their poverty is self-perpetuating in a manner beyond their control?

Neo-classical growth models predict conditional convergence implying that countries with different policies and habits should expect to have different long-run steady state growth paths. In these cases, poor countries should adopt sound macroeconomic policies, encourage cultural change toward adoption of more promising habits, and mitigate unfavorable geography as much as possible. Eventually, as countries manage to adjust policies and habits and enjoy a run of good luck, they will converge to the rich countries' level of per capita output.

On the other hand, poverty trap models postulate that many of the behaviors and policies which accompany poverty are self-perpetuating, hence the problem cannot be fixed from within. This story is fundamentally different, suggesting that poor countries remain poor not because they are doing something wrong, but because of circumstances beyond their control. These models have thus frequently been used to justify the necessity of foreign aid to help countries escape self-perpetuating poverty.

Determining the extent to which external aid can deliver permanent, transformative benefits is a critical step in illuminating viable paths to development. In a single-equilibrium world, external aid that does not reform the basic policies, habits, and parameters of the economy will not change the balanced growth path and will thus have no permanent effects. On the contrary, in a world of multiple equilibria and poverty traps, aid can lift an economy onto a higher balanced growth path, resulting in permanent improvement. Unfortunately, as we discuss below, convincing tests that address the wide variety of potential poverty traps are elusive.

In this paper, we test the single-equilibrium world against the alternative hypothesis of multiple equilibria. Our test borrows from work by Davis and Weinstein (2002, 2004) on economic geography. The central insight is simple. Assume an economy starts in steady state and apply a large, temporary shock. If, long after the shock has dissipated, the economy has failed to return to its steady state, then we may conclude that the economy has been pushed into a new basin of attraction and found a new steady state thus implying a world with multiple steady states. if, long after the shock has dissipated, the economy has returned to the original steady state, then the shock was insufficiently large to push the economy beyond the current basin of attraction. This cannot be taken as proof of a single equilibrium world – the shock may simply not have been large enough to reach the basin of attraction of the nearest alternate steady state – but the size of the shock may be taken as a lower bound on the proximity of alternate steady states.

We use temporary changes to the export revenues of small economies as the exogenous shock. The validity of the identification is discussed in detail in section 4.5. Our approach has the advantage that it does not resort to taking a stand on a particular model of how poverty traps are delivered – savings, industrial spillovers, institutions, etc. To preview our results: we find no evidence that temporary shocks have permanent effects on the steady state growth path of an economy. This is true even if we study only the very largest shocks, those most likely to push an economy into a neighboring basin of attraction (see section 4.4). while not conclusive – see section 5 for a discussion of caveats – the lack of discernable permanent effects from temporary shocks of a plausible magnitude calls into question the policy-relevance of the multiple steady states that are at the heart of many poverty traps models.

Our results also call into question the ability of development assistance to lift recipient nations out of a poverty trap and onto a self-sustaining, higher growth path. According to the World Bank World Development Indicators, worldwide total official development assistance (ODA) in 2010 was 114.2 billion, or ~0.37%

1 The World Bank classifies countries into high, upper-middle, middle, lower-middle, and lower income groups. Our use of these terms refers to aggregate data for these World Bank groups.

of high income countries' GDP.[1] Official development assistance is not equally distributed across countries. In 2010, low income countries received aid averaging 11.7% of GDP while lower-middle, middle, and upper-middle income countries received aid averaging 1.4%, 0.4%, and 0.1% of GDP respectively (source: World Bank World Development Indicators). The ambitious Monterrey Consensus of 2002, reiterated in the Doha Declaration of 2008, called for an increase in aid devoted to official development assistance (ODA) to 0.7% of rich countries' GDP. If this increase in aid were to materialize and were similarly concentrated on low income countries, it might constitute an additional 10% of GDP of low income countries. Unfortunately, our results indicate that even the largest export revenues shocks in our data – those in excess of 9.7% of GDP – have no permanent effect on a country's growth path. As the magnitude of our largest shocks are comparable to the magnitude of the largest aid flows, our results cast doubt on the existence of a nearby higher growth path that can be reached via foreign aid.

The paper proceeds as follows. Section 2 reviews prior work testing for single vs. multiple equilibria. Section 3 presents a simple model to motivate our empirical specification. Section 4 presents the data, empirical work, key results, and robustness checks. Section 5 summarizes and concludes.

2 Prior Work

A large theoretical literature has proposed a wide variety of possible self-reinforcing mechanisms which can be broadly categorized into two classes. Models of the first class stipulate a non-convexity in the aggregate production function due to scale effects in the production of knowledge (Romer 1990) or spillovers between firms or industries (Murphy et al. 1989; Boldrin 1992; Durlauf 1993).[2] As a result, two countries with identical parameters – defined rather broadly to include everything from savings rates and population growth rates to geography and even policy – may have different long-run steady-state growth paths. As Krugman (1991) explains, this may happen either because countries had different initial conditions – meaning they had different levels of income at the time when technological change introduced the non-convexity – or because the actors within the countries coordinated on different expectations about the future growth path.[3]

2 See Matsuyama (1995) and Azariadis and Stachurski (2005) for excellent surveys.

3 Krugman demonstrates that the relative importance of history and expectations depends on (i) the cost of adjustment of the state variable, (ii) the interest rate at which future payoffs are discounted, (iii) the strength of the increasing returns to scale that provide the positive feedback.

The second class of poverty trap models comes from the idea that reforming policy and household behavior is not a simple matter. This approach holds that certain parameters are endogenous because there are powerful feedback loops whereby low income countries find it extremely difficult to engender "good" behaviors and institutions. Many such mechanisms have been advanced. A partial list would include endogenous fertility (Becker et al. 1990); subsistence levels of consumption limiting the savings rates of the poor (Azariadis 1996); the effects of malnutrition on human capital and labor effort (Dasgupta and Ray 1986); financial market imperfections inhibiting the ability of the credit constrained to make investments in physical and/or human capital (Galor and Zeira 1993); the effects of poor health on human capital accumulation; increasing returns to corruption as changes in social norms lower the probability of being caught and the resulting penalties (Murphy et al. 1993; Tirole 1996); a vicious cycle between poverty and conflict (Blomberg et al. 2006).[4]

Unfortunately, poverty traps models are rarely amenable to testing via standard cross-country growth regressions. Bernard and Durlauf (1996) point out that cross-sectional tests of conditional convergence, which have generally rejected the null of no-convergence, cannot distinguish between single and multiple-equilibrium models because the test simply requires that some pairs of countries are converging over time but does not identify which pairs are converging or require that a representative sample of pairs are converging. These tests can thus find evidence of convergence even in a world with convergence clubs born of poverty traps because pairs within a single club do converge.

Generally, models of the first type predict that the mapping from observable parameters to income levels is not a single-valued function. Rather, the mapping of current country-level characteristics to current levels of income depends on which basin of attraction a country is in. In the most extreme case, good and bad equilibria are differentiated only by self-reinforcing expectations about the actions of others in the economy. Testing this theory would require reliable, cross-country comparable measures of households' and firms' expectations about the habits of others along whichever dimension – consumer demand, business investment, fertility, human capital investment – is thought to deliver the multiple equilibria, a monumental task. It is similarly difficult to imagine how one would falsify such a theory. In the less extreme case, a country's steady state growth path is set by historical experience. A poverty trap of this nature would thus imply that the relationship between current observable parameters and current income per capita is a function of initial conditions. A group of papers has

4 For a survey of these models see Bardhan (1997).

used regression tree analysis or threshold estimation to sort countries into groups such that countries within the same group exhibit a common aggregate production function (e.g., Durlauf and Johnson 1995; Hansen 2000; Durlauf et al. 2001; Papageorgiou 2002; Canova 2004; Owen et al. 2009; Sirimaneetham and Temple 2009). These papers have convincingly demonstrated that aggregate production functions differ significantly across groups, and that per capita income and literacy in 1960 can predict which group a country will belong to over the next 40 years. However, as both Durlauf and Johnson (1995) and Owen et al. (2009) admit, the evidence from this line of inquiry is consistent with both models of multiple steady states and models of unified growth and thus cannot constitute a test of the poverty traps hypothesis.[5,6]

Poverty trap models of the second type admit per capita income as a single-valued function of country characteristics, but explicitly hypothesize bidirectional causality. They suggest that per capita income, y, is both caused by and causes a set of variables $\boldsymbol{X}$ capturing the mechanism of the poverty trap. Establishing a poverty trap in this case requires establishing significant feedback in both directions, which necessitates separate instruments for per capita income and each of the mechanisms of interest. Moreover, in order to deliver multiple steady states, it must be that the relationship between y and $\boldsymbol{X}$ is non-linear in at least one direction, complicating IV estimation.

As a result of these challenges, the literature testing for poverty traps at the macroeconomic level is characterized by several indirect approaches. One group of papers compares the observed evolution of the cross-country distribution of income with the evolution predicted by models exhibiting single- and multiple-steady states. Quah (1996), Azariadis and Stachurski (2005), and Bloom et al. (2003) are examples of this approach. Unfortunately, it can be difficult to determine whether twin peaks are driven by multiple steady states or a twin-peaked distribution of the fundamental determinants of growth. A second approach, best exemplified by Graham and Temple (2006) is to calibrate a structural model admitting multiple equilibria. Using a two-sector variable returns-to-scale model,

5 For a survey of unified growth theory, see Galor (2005).

6 Moreover, given the limited data available, these methods are understandably limited in the number of groups they can identify. As a result, Durlauf and Johnson (1995) admit, there might be residual heterogeneity within each of the subgroups. Canova (2004) confirms the suspicion, noting “the dispersion of steady states around each basin of attraction is significant, suggesting that clustering is more prevalent than convergence even within groups” (p. 51). One thus wonders whether lackluster convergence within groups is due to non-convexities at a finer level. Our paper addresses this issue: we find no evidence of barriers to convergence within clubs.

Graham and Temple find that a quarter of their sample of 127 countries are best characterized as in a low equilibrium. However, as they explicitly acknowledge, the validity of such calibration rests heavily on the validity of the structural model. A third approach is to ask whether the cross-sectional and time-series evidence is consistent with poverty traps hypotheses. For example, Rodrik (1998) and Easterly (2006) look at growth accelerations to see whether they are also accompanied by increases of the savings rate which would constitute evidence for a savings-trap hypothesis. Kraay and Raddatz (2007) estimate savings rates as a function of income per capita in a cross-section to determine whether it has the nonlinear form required to deliver a savings-trap. Unfortunately, given the multiplicity of potential poverty trap mechanisms, finding a lack of evidence for one particular mechanism does not disprove the general concept. Indeed, arguing against the existence of poverty traps is a bit like playing whack-a-mole.

Finally, there is a well-developed literature on whether foreign aid (an external though *not* exogenous shock to per capita income) increases growth rates, which might be taken as a sign that aid helps countries escape from a poverty trap. Unfortunately, there is no clear consensus yet in this literature. Burnside and Dollar (2000) conclude that aid helps when the institutional setting is right. Collier and Dehn (2001) and Guillaumont and Chauvet (2001) both find that aid can mitigate negative terms-of-trade shocks. Hansen and Tarp (2000) review previous studies and conclude that, in small amounts, aid can help growth rates. But Easterly (2004) warns that these results are fragile to definitions of growth, aid, and "good" institutions and Werker et al. (2009) argue that most instruments for foreign aid are flawed. Most importantly, these papers look at the increase in the growth rate over a very short period (typically 4 years) concurrent with the increase in aid. As such, they do not test whether aid contributes to permanently higher levels of output from sustained output growth during a transition to a higher steady state. In fact, when Easterly looks at the growth effects over longer periods, the positive effects of aid disappear. Werker et al. similarly find no effect on growth of exogenous increases in foreign aid. On the whole, no conclusive pattern, either of confirmation or rebuttal of poverty trap hypotheses seems to emerge from these studies.

We know of no other papers taking our approach – looking for permanent effects of temporary shocks – for the macroeconomic study of poverty traps at the economy-wide level. However, there is previous work at the household level that takes a similar approach. McPeak and Barrett (2001) and Lybbert et al. (2004) look at the effects of rainfall variation in east Africa on household assets through herd loss. They document the existence of a poverty trap at the household level as insurance against such shocks is not possible and those who lose their herds have no recourse and fall into poverty. In so doing, they demonstrate that a temporary shock has a permanent effect on household wealth thus concluding in favor of the existence of multiple steady states. Barrett et al. contains a helpful review of related literature on poverty traps at the household level. This is an interesting line of research which has generally found evidence in favor of poverty traps. Because the mechanisms which deliver persistent poverty at the household level need not be the same as those which deliver persistent poverty at the level of societies,[7] and because prior evidence of poverty traps at the macroeconomic level is inconclusive, we feel our work at the macroeconomic level is complementary to work at the household level and remains of interest.

3 Exogenous Shocks: Simple Theory

To motivate the particular form of our regression, we present a variant of the Ramsey growth model. The key difference is that the economy receives an exogenously determined level of export revenues which can be exchanged for imported investment goods. Thus shocks to export revenues have a direct effect on GDP. This enables us to derive a test of the role of temporary export-revenue shocks. While it is well known that temporary shocks to per capita income do not affect the steady state growth path in the Ramsey model, we feel the formal model nonetheless serves two purposes. First, it provides a clean formal argument that the temporary income shock we use as an instrument fails to produce permanent effects.[8] Second, it provides a structural form for our regression, helping to clearly articulate our null hypothesis.

Consumers maximize the following inter-temporal utility function:

$$U=\int_0^\infty e^{-(\rho-n)t}\ln|c_t|\,dt \tag{1}$$

subject to the constraints:

$$y_t=Ak_t^\alpha \tag{2}$$

7 See Barrett and Swallow (2006) for an excellent discussion of *fractal* poverty traps – poverty traps at both micro and macro levels that spring from the same basic mechanism.

8 It has been suggested to us that temporary shocks can have permanent effects in a single-equilibrium world if they are invested in infrastructure. But this is precisely the behavior the Ramsey model refutes by explaining that households will respond so as to keep the economy on the steady state growth path. Werker et al. (2009) provide empirical corroboration by showing that exogenous foreign aid payments go largely toward increasing consumption rather than investment.

$$y_t = c_t + i_t^d \tag{3}$$

$$\dot{k}_t = i_t^d + i_t^m - k_t(\delta + n) \tag{4}$$

$$i_t^m = z_t \tag{5}$$

where lower case letters denote per capita variables (i.e., divided by L_t). The equation of motion for the capital stock includes terms for domestic investment, $i^d_,$ and imported investment goods, $i^m_,$ financed by export revenues, z. We assume exogenous steady state TFP growth at a rate ζ. We then further assume that, in addition to steady state technological growth, productivity can depend on a set of production function shifters, $W_t=\{W_{1,t}, \ldots, W_{n,t}\}$ such as institutions, policies, and structural variables. We assume that there is a given, known dynamic evolution of these variables characterized by steady state levels $W_{ss}=\{W_{1,ss}, \ldots, W_{n,ss}\}$. We assume that productivity depends on these shifters according to:

$$A_t = \left[\prod_{j=1}^{n} \left(W_{j,t}\right)^{\pi_j}\right] \left(A_0 e^{st}\right)^{1-\alpha} \tag{6}$$

So that the production function becomes

$$Y = w_t K^{\alpha} \left(\left(A_0 e^{st}\right) L\right)^{1-\alpha} \tag{7}$$

$$w_t \equiv \left[\prod_{j=1}^{n} \left(W_j\right)^{\pi_j}\right] \tag{8}$$

We are interested in the case where the economy starts at its steady state: $k_0=k_{ss}$ corresponding to a given level of $z_t=z_{ss}$ and is perturbed by a shock to z_t which disappears gradually, in a way that we make precise below.

From the Hamiltonian and the necessary conditions that follow, we can derive the equations of motion for consumption and the capital stock. Letting "hats" denote quantities in terms of effective units of labor, we have:

$$\dot{\hat{c}}_t = \hat{c}_t \left(w_t \alpha \hat{k}_t^{\alpha-1} - \delta - \rho - s\right) \tag{9}$$

$$\dot{\hat{k}}_t = w_t \hat{k}_t^{\alpha} - \hat{k}_t(\delta + n + s) - \hat{c}_t + \tilde{z}_t \tag{10}$$

which, together with the initial and terminal conditions, give us a system of differential equations for the state variables, k and c.

We assume that the data generating process for export revenues is:

$$\tilde{z}_t \equiv \frac{z_t}{k_t} = \tilde{z}_{ss}\left(1+\theta^t\right); \quad \theta<1 \tag{11}$$

Thus describing a process whereby an initial shock decays exponentially.

Because Ramsey models are well understood, we relegate the details of the solution to an appendix available upon request. As we demonstrate, solving the model yields the following evolution of per capita income in response to a temporary shock:

$$\lim_{t\to\infty}\left[\ln y_t - \ln y_1\right] = -\left[\ln y_1 - \ln y_0\right] + \left\{\log w_0\left(e^{-\psi t}-1\right) + P\ln w_0\left(e^{-\psi t} - e^{-\lambda_2 t}\right)\right\} + st \tag{12}$$

Implying that the entire output-effect of a temporary export-revenue shock, (y_1-y_0), is reversed in the long run once TFP growth and institutional change have been taken into account. We take equation (12) as the basis for our empirical work.

4 Exogenous Shocks: Estimation

Our identification strategy relies on the use of temporary shocks to export revenue as forces that could – in a world of multiple steady states – push an economy from one steady state to another. In essence, a temporary shock will cause a one-time change in per capita income from y_t to y_{t+1} .If the economy is characterized by a single steady state, then over the long-run the effect of the shock will evaporate and the economy will, on average, return to its previous growth path. But if multiple steady states exist and the shock is sufficiently large, this initial impulse will have permanent effects which remain long after the temporary shock has dissipated.

The model developed in section 2 delivers a specification for this test as well as the hypothesis for a single steady-state world. Equation (12) yields the following specification:

$$\ln y_{i,t+n} - \ln y_{i,t+1} = \beta_1\left(\ln y_{i,t+1} - \ln y_{i,t}\right) + \beta_2 \ln w_{i,0} + st + \varepsilon_{i,t+n} \tag{13}$$

where $\varepsilon_{i,t+n} = \sum_{j=0}^{n} u_{i,t+j}$ is the accumulation of a set of white noise disturbances to income and w_0 is a vector of production shifters. The null hypothesis.

$$H_0 : \beta_1 = -1 \tag{14}$$

corresponds to a world with a single steady-state in which temporary deviations from the steady state growth path do not translate into permanent deviations. The alternate hypothesis is:

$$H_1 : \beta_1 = -1 + \chi \tag{15}$$

where χ is the effect of the temporary shock that remains after 25 years, which we refer to as the "permanent" component. We will test $\beta_1=-1$, implicitly testing $\chi=0$. The precision of our point estimates will define the range of χ consistent with the data.

Are our regressions mis-specified under the alternative that multiple equilibria exist? Given the strong evidence for multiple growth regimes cited in the previous section, there is instinctive suspicion that a linear regression based on a Ramsey model must be mis-specified. The recent literature on growth regimes has demonstrated that countries with different income and literacy levels are characterized by different aggregate production functions. But it should be noted that we are not estimating a growth equation where constraining all countries to have the same coefficients would be obvious folly in light of the recent literature. The logic of our empirical approach – temporary shocks may or may not have permanent effects – is much simpler and explicitly admits a wide variety of production functions, including non-convexities that deliver multiple growth regimes. Our single-equilibrium model simply delivers the null hypothesis. Nonetheless, in section 3.4, we rerun our estimates for sub-samples based on per capita income, the most important splitting variable in the literature on growth regimes, so as to allow for such regime differences. We find no change in our results.

4.1 Instrumenting with Temporary Export Revenue Shocks

The first complication is that this hypothesis describes the evolution of the economy in response to exogenous shocks. If the initial deviation from steady state comes from a change in fundamentals – such as enhanced TFP growth – then there is likely to be a correlation between $(\ln y_{t+1} - \ln y_t)$ and $(\ln y_{t+n} - \ln y_{t+1})$ that will bias the coefficient estimates. To address this problem, we use the change in export revenues as an instrument for $(\ln y_{t+1} - \ln y_t)$. However, the previous argument suggests we are interested in whether *temporary* shocks deliver permanent changes to steady state per-capita output. A permanent decline in export revenues would be expected to deliver a permanent decline in steady state output per capita. So in fact, we are interested in the temporary component of changes to export revenues. Thus we first filter export revenues to isolate their temporary component.[9] Finally because the effects of changes can take a while to show up in GDP figures, we include three lags.[10] Hence our first stage regression is:

$$\ln y_{i,t+1} - \ln y_{i,t} = \alpha_0 + \sum_{k=1}^{3} \alpha_k \left(\ln \bar{z}_{i,t-k} - \ln \bar{z}_{i,t-1-k} \right) + e_{i,t} \tag{16}$$

where $\bar{z}$ is the temporary component of export revenues. Our instruments are the (lagged) percentage change in export revenues that comes from temporary shocks. We estimate (13) and (15) as a system using panel 2SLS with country fixed effects. We use shocks that dissipate within 5 years and a horizon of 25 years.[11] Because we have overlapping histories the error term in the second stage, $\varepsilon_{i,t}$ from equation (13), will be heavily correlated between successive observations from the same country, thus we cluster by country. Finally, we control for initial level of income to account for neo-classical convergence.

4.2 Data Sources

Data on per capita income comes from Penn World Tables 6.2. Data on export revenues were taken from various volumes of the UNCTAD Handbook of International Trade and Development Statistics. These data were then standardized and joined to form a single, consistent time series. The raw data include 130 developing countries with an average of 31 years of coverage per country. Our use of a 25-year horizon (explained in the next section) reduces the number of data points considerably and eliminates many countries that have not yet been observed continuously over a 25-year period. As a result, we have 55 countries with an average

9 We have used the Christiano-Fitzgerald filter with plo=2 and phi=10. Thus the longest shock deemed temporary dissipates in 5 years. In our early work, we compared a variety of different filters including Hodrick-Prescott, Baxter-King, Butterworth, and a simple high-pass filter. Over a variety of cases, the Christiano-Fitzgerald filter seemed to best capture the spirit of a temporary shock to export revenues. The definition of temporary was chosen to be relatively short so as to enable our regression, limited in length by the data set, to look for effects 20 years after the original impulse has dissipated. A table showing that the results are robust to using any of these other common filters is available upon request. See Christiano and Fitzgerald (2003) for a helpful discussion of band pass filters.

10 We have chosen this number of lags to maximize the first stage F statistic.

11 As a robustness check we have also looked at longer-lived shocks – those that dissipate within 10 years – as well as a shorter horizon of only 20 years. The statistical significance of certain coefficients change but the results are broadly upheld: there is no evidence of LR effects of temporary shocks.

of 15.8 data points each for a total of 871 data points. Table A1 lists the countries in the sample and the years in which the export-revenues shocks we follow occurred.

We are somewhat hampered in our ability to implement the full equation by the data on institutional quality. While many fine indicators have been developed, measured, and brought to bear in cross-country growth regressions and other work, panel data are available for very few of them. For the most part, they are either time-invariant or measured only once. While equation (13) requires only a measure at the beginning of the shock, we will be looking at shocks starting at any and all dates in the sample, thus we need a panel measure. In lieu of this, we have for the moment used fixed effects to gather the entire set of country-specific shocks: both those that can be measured with specific institutions and those that cannot. Since we are not explicitly interested in the effects of these institutions, the bundling into a single fixed effect is not a concern. In the one instance where we can find reliable panel data on institutional quality, the measure of democracy from the Polity IV dataset (Marshall and Jaggers 2008), we include it explicitly.

4.3 Results

The results using export revenues as the instrument are presented in column (1) of Table 1. The first stage passes both Craig-Donald tests at the highest confidence level mitigating concerns about weak instruments. Column 2 adds the polity score, of the few indicators of institutional quality with sufficient variation to stand out from the country fixed effects.[12] It does not change the character of the results. To address concerns that our sparse specification may omit relevant covariates, we can also include standard Solovian growth factors: primary and secondary schooling rates, population growth rates, net national savings rates. We can further add summary measures of macroeconomic policy: general government final consumption expenditure as a share of GDP and the consumer price inflation rate. None of these controls, singly or as a group, change the results and interpretation that follow.

The results cannot disprove the null hypothesis. The point estimate is virtually identical to –1, suggesting that the per-capita income effect of a temporary shock to export revenues completely dissipates over the 25-year horizon in ques-

12 Unfortunately, while there are many, many indicators of institutional quality which have been targeted in growth regressions, time-varying measures are available for very few of them. Most of them – such as rule of law – are measured only very infrequently and exhibit very little intertemporal variation and are thus collinear with our country fixed effects.

Table 1 Full Sample.

$\ln y_{t+25} - \ln y_{t+1}$		**(0)**	**(1)**	**(2)**
Instrument		Terms of trade	Export revenues	
t	(ς)	0.005	0.006	0.005
		(0.0021)**	(0.0013)***	(0.0013)***
$\ln y_{t+1} - \ln y_{t+0}$	(β_1)	−0.818	−1.006	−0.994
		(0.297)***	(0.126)***	(0.122)***
Polity	(β_2)			−0.002
				(0.0012)
N		898	898	871
Second stage within-R^2		0.617	0.617	0.630
Craig-Donald First-stage Wald F-statistic		1.871	22.12	22.31
Shea partial R^2		0.013	0.073	0.076
Sargan overidentification		0.698	0.171	0.434
χ^2 p value		0.983	0.918	0.805
χ^2 test (β_1=−1)		0.37	0.00	0.00
Prob>χ^2		0.541	0.959	0.963

Standard errors reported.
Significance levels: *10%, **5%, ***1%.
Unreported controls: $\ln y_{t+0}$.
We estimate equation (13) with two instruments. First we use shocks to TOT. Unfortunately, this is a weak instrument whose results are likely biased toward 0. Thus we focus on shocks to export revenues for the remainder of the paper. The point estimates are very close to 1, suggesting that the entirety of the resulting change in GDP is reversed over the next 25 years.

tion. It is also estimated with reasonable precision thanks to the strength of the instrument. In our basic specification (Table 1, column 2), the 95% confidence interval is [−0.76, −1.25]. A true coefficient in the interval (0, −1) represents a mix of temporary and permanent effects. So our confidence interval does admit a range of parameters in which temporary shocks have permanent effects. Nonetheless, while the results do not preclude all permanent effects, the narrow confidence band seems to rule out a shift to a dramatically different basin of attraction.

The reader may wonder why we have chosen export revenues as our instrument rather than terms of trade, which would capture shifts in both import and export prices. We have reported results using terms of trade in column 0 of Table 1. Unfortunately, as you can see from the first stage F-statistic, the temporary component of terms of trade is simply too weak an instrument for changes in output per capita. Hahn and Hausman (2002) show that the sign of the weak instrument bias in 2SLS regressions is the sign of the correlation between the endogenous variable and the error term in the OLS specification. In our case, that would be positive, offering a possible explanation for why the coefficient estimate using the weak instrument is less negative (closer to zero). In any case, the results remain similar: the statistic is strongly distinguished from zero and indistinguishable from –1. Hence even this much weaker instrument supports the conclusion that temporary shocks do not result in discernable permanent changes in income per capita.

4.4 Robustness: Large Shocks Only and Differentiating Between Rich and Poor

We feel two critiques are especially relevant and address them directly with additional regressions.

Big shocks only: In the long run, an economy remains near its steady state. The theoretical argument was that, in a world of multiple equilibria, an exogenous shock may push an economy from one basin of attraction to another and thus the long run will be characterized by a different steady state. But this can happen only if the shock is sufficiently large. Shocks that are below the threshold result in no long-run effect, even in a world of multiple equilibria. The current strategy is biased against finding an effect because the ineffective small shocks are included along with the large shocks. Therefore, we ought to focus only on the largest shocks, to see whether they have long-term effects.

In order to isolate the largest shocks, we calculate the cumulative 3-year shocks to export revenues, take the absolute value, rank them, and then take the largest 1/n of these shocks. Choosing the value of n determines how large the shock must be to be considered. For example, if n=2, then we look only at the effects of half of the shocks. We use n=2, 5, and 10. Table 2 gives the percentiles of the absolute value of the three-year cumulative export shocks. Thus for n=2, we are using shocks that are at least 11.1% of exports, for n=10, we are using shocks that are at least 43.4% of export revenues. How big are these shocks in terms of GDP? Exports average just over 30% of GDP in our sample, so the effect on GDP is typically smaller by a factor of 3 or 4. Table 2 also reports percentiles for the one-year shocks to GDP.

In order to test the hypothesis that only large shocks have an effect, we generate a new instrument, η, which equals the cumulative 3-year shock to export revenues if this cumulative shock is large enough to make our cutoff but is set to zero if the shock is too small to pass the cutoff.[13]

13 The switch from using the last three periods as separate instruments to summing them and using the cumulative change as the instrument comes because our method of zeroing out the small shocks leads to excessive collinearity between the instruments if used separately (they all have zeros in the same place).

Table 2 Size of the Shocks.

	Cumulative % change in export revenues over past 3 years (absolute value) $\left\|\sum_{k=0}^{2}(\ln\bar{z}_{t-k}-\ln\bar{z}_{t-k-1})\right\|$	Total change in GDP/capita $\|\ln y_{t+1}-\ln y_t\|$
25th Percentile	4.6%	1.47%
50th Percentile	11.1%	3.31%
75th Percentile	25.0%	6.02%
90th Percentile	43.4%	9.71%
95th Percentile	61.7%	13.01%

Shows the distribution of the export revenue shocks. The left-hand column shows the total size of the shock: e.g., the median shock is 11.1% of exports. The right-hand column shows the size of change in output associated with the export revenue shock. Here the median shock to export revenues produces a 3.3% shock to GDP.

$$\eta = D_{\text{large}} * \sum_{k=1}^{3}\left(\ln\bar{z}_{i,t-k}-\ln\bar{z}_{i,t-k-1}\right) \tag{17}$$

$$D_{\text{large}} = \begin{cases} 1 & \text{if} \quad \sum_{k=1}^{3}\left(\ln\bar{z}_{i,t-k}-\ln\bar{z}_{i,t-1-k}\right) \geq cutoff \\ 0 & else \end{cases}$$

The results are reported in Table 3. The results confirm the previous pattern: we cannot reject the hypothesis that temporary shocks to export revenues have no permanent effects on per capita income. This is true for even the largest shocks, those between 0.4% and 2% of GDP. The first-stage F-statistics show this modified instrument is much stronger than the original and the standard errors and confidence interval are correspondingly smaller.

Rich vs. Poor: Multiple steady-states arise because of a non-convexity in the Solow diagram. But it does not necessarily follow that non-convexities exist near every country's steady state. Rather, different countries may be in different equilibria with different basins of attraction and thus the shock required to escape the current basin of attraction may be of different sizes. Depending on the mechanism generating the non-convexities and thus the multiple steady states, the shape of the basin of attraction, and thus the size of the shock required to trigger a permanent transition to a new steady state, might depend on many country-specific variables. Canova (2004) states "[a]mong the indicators suggested by theory, the distribution of income per capita at the beginning of the sample seems to be

Table 3 Large Shocks Only.

$ln\ y_{t+25} - \ln y_{t+1}$		(3)	(4)	(5)
Fraction of shocks included		1/2	1/5	1/10
Smallest shock included (% of X-revenues)		11.1%	29.4%	43.4%
(% of GDP)		3.3%	6.6%	9.7%
t	(ς)	0.004	0.004	0.004
		(0.0013)***	(0.0012)***	(0.0012)***
$\ln y_{t+1} - \ln y_{t+0}$	(β_1)	−1.034	−0.974	−1.019
		(0.097)***	(0.069)***	(0.065)***
Polity	(β_2)	−0.002	−0.002	−0.002
		(0.0012)	(0.0012)	(0.0012)
N		871	871	871
Second stage within-R^2		0.629	0.631	0.628
Craig-Donald First-stage Wald F-statistic		37.12	82.61	101.44
Shea partial R^2		0.121	0.234	0.273
Sargan overidentification		1.819	0.970	0.451
χ^2 p value		0.403	0.616	0.798
χ^2 test (β_1=−1)		0.12	0.14	0.09
Prob>χ^2		0.728	0.711	0.7678

Standard errors reported.
Significance levels: *10%, **5%, ***1%.
Unreported controls: $\ln y_{t+0}$.
Only shocks large enough to push a country into an adjacent basin of attraction should have long-term effects. Thus we re-estimate equation 13 with a dependent variable that is non-zero only the largest shocks. Shocks not meeting a threshold are set to zero, as per equation 16. Each column represents a different threshold: the first column zeros the smallest 50% of shocks so as to concentrate on the largest 50%. The second and third columns allow only the top 20% and 10% of shocks through respectively. In all cases, the point estimate remains close to 1, suggesting the effect of the shock is fully reversed within 25 years.

the one with the highest information content" regarding which convergence club the country will join (p. 65). Thus we split our sample into low- and high-income countries and re-run the regressions separately within each sub-sample. We have produced the split in two different ways: first by considering whether per-capita income was in the top or bottom half of the entire sample; second by considering whether per-capita income was in the top or bottom half for the particular year in question.

The results are reported in Table 4. Not surprisingly, the instrument is weaker for the upper half of the income distribution. Nonetheless, for the poor countries, for which the instrument is sufficiently strong, we remain unable to reject the null of no permanent effects and our confidence interval is similarly tight, once again implying that if permanent effects do exist, they are quite moderate.

Table 4 By Income.

$\ln y_{t+25} - \ln y_{t+1}$		(11)	(12)	(13)	(14)
		Upper and lower halves of entire dataset		Upper and lower halves by year	
(Range for y: real 2000 USD)		330–2000	2000–28,800	330–2350	1770–28,800
t	(ς)	0.003	0.011	0.004	0.105
		(0.0017)*	(0.0022)***	(0.0016)**	(0.022)***
$\ln y_{t+1} - \ln y_{t+0}$	(β_1)	−1.025	−1.068	−1.032	−0.968
		(0.111)***	(0.509)**	(0.111)***	(0.535)*
N		444	452	437	454
Second stage within-R^2		0.687	0.574	0.695	0.559
Craig-Donald First-stage Wald F-statistic		16.81	4.83	16.60	4.52
Shea partial R^2		0.110	0.034	0.110	0.031
Sargan overidentification		0.743	0.663	0.301	0.480
χ^2 p value		0.690	0.718	0.860	0.787
χ^2 test (β_1=−1)		0.05	0.02	0.08	0.00
Prob>χ^2		0.820	0.893	0.771	0.952

Standard errors reported.
Significance levels: *10%, **5%, ***1%.
Unreported controls: $\ln y_{t+0}$.
Splitting the sample in half by income to test whether export shocks are equally impotent for low and high income countries. Country-years may be classified relative to the entire dataset (left) or relative to the year in which they occur (right). Notice that the instrument is much stronger for poor countries than for rich. Nonetheless, the point estimates remain close to 1 for both groups.

4.5 Is the Instrument Valid?

The strength of the first stage F-statistics, generally passing the Craig-Donald critical values at the highest levels, suggests that the instrument is not weak. However, one might worry as to whether the instruments from equation (15) are correlated with the error from equation (13). For the small economies under discussion, it is commonly agreed that commodity prices are exogenous (see Collier and Gunning 1999). Nonetheless, there are complex feedbacks between export revenues and total output and, not surprisingly given the complex time-series behavior of GDP, there is some doubt as to the direction of Granger causality between the two.

There are two important aspects of our empirical approach that combine to guard against reverse causality. The first is the use of lagged changes to export revenues. The second is the use of the temporary component of export-revenues as the instrument for GDP. Here are three examples illustrating the robustness of the technique.

Scenario A: *An adverse productivity shock in the home-country's export industry. For example, a major export industry is nationalized leading to poor management and poor investment decisions.*

Shocks to export revenues – which then may or may not spread to GDP – are precisely the channel of interest. The only way this scenario causes endogeneity problems is if the productivity shock is caused by prior changes to GDP. For example, GDP growth is poor leading to the election of a government which then nationalizes export industries causing productivity to decline. It has been shown that leftist governments are less likely to respect existing property rights (Vaaler et al. 2005). To test this channel, we estimated a probit of the probability that a left-wing government is elected as a function of the past 4 years of GDP growth and the identity of the previous government.[14] We repeated the exercise for both the executive and legislative branches. The results, reported in Table 5, show no systematic relationship between past GDP growth and the partisan affiliation of government: the eight coefficients show no consistent sign and only one is significant at even the 10% level. Thus, while this channel this is possible, we do not think it sufficiently common to endanger our approach.

Scenario B: *An increase in home country GDP (perhaps due to an exogenous increase in productivity in non-tradable sectors) leads to an increase in domestic demand and thereby a decline in the excess-supply of the export-good and thus a decline in exports.*

This looks to be a genuine case of reverse causality: an effect starts in GDP and moves to export revenues. First, note that if this is a permanent shock, then we have a permanent decline in export revenues which does not get past our filter. But if it is a temporary shock, it is true that we will measure the reversal of the shock as a decline in GDP akin to that of the return to steady-state in a single-equilibrium world. However, because the instrument is lagged, the instrumented value of $(y_{t+1}-y_1)$ will not pick up the change to export revenues caused by this shock. Hence the decline in $(y_{t+25}-y_{t+1})$ will simply be part of the error term.

Scenario C: *A common shock that affects GDP in both the country in question and its trade partners. For instance, FDI dries up for an entire region due to investor*

14 We have no reason to believe left-wing governments are either better or worse for TFP growth. Governments are classified as either left or right and thus this is a binary variable.

Table 5 Income Growth and Partisan Government.

	[15]	[16]	[17]	[18]
Method of estimation: panel probit				
Dependent variable: prob(government elected is left-wing)				
Government in question	**Executive**	**Largest governing party in legislature**	**Executive**	**Largest governing party in legislature**
Previous government was left-wing	0.82	1.85	0.88	1.84
	(5.81)***	(10.57)***	(5.86)***	(10.51)***
GDP growth year *t*–1	2.36	0.51	3.09	0.51
	(0.72)	(0.30)	(0.93)	(0.30)
GDP growth year *t*–2	0.35	0.73	1.15	0.94
	(0.15)	(0.44)	(0.48)	(0.56)
GDP growth year *t*–3	−2.46	−2.72	−1.47	−2.53
	(0.93)	(1.67)*	(0.54)	(1.55)
GDP growth year *t*–4	−0.59	0.52	−0.56	0.46
	(0.30)	(0.35)	(0.29)	(0.32)
Dummy (Hyper-Inflation in year *t*)			0.49	0.18
			(1.49)	(0.86)
Constant	−2.08	−1.30	−2.32	−1.30
	(6.03)***	(11.70)***	(5.90)***	(11.70)***
Observations	132	353	132	353
Countries	42	74	42	74
Pseudo R^2	0.24	0.32		0.32

z-Statistics reported: ***1%, **5%, *10%.
Looking for a systematic relationship between past GDP growth and the election of left-wing governments, controlling for the partisan affiliation of the previous government. Data on the partisanship of government and the identification of election years for the relevant branch of government come from the 2007 Database of Political Institutions (Beck et al. 2001). Hyperinflation is defined as an annualized rate of inflation above 40%. While inference is somewhat hampered by large standard errors, it is clear that there is no consistent pattern of signs.

herding and poor differentiation by foreign investors among the various countries in the region.

If this were a permanent shock, then the change to export revenues would not pass our filter and, as with scenario B, it would simply enter the error term. However, if this were a temporary shock, we could see a temporary effect on GDP and a temporary effect on export revenues, both caused by the external event. This event would deliver spurious correlation if the decline in foreign country GDP occurred prior to the decline in home country GDP, thus giving a pattern whereby export revenues decline first, and then home country GDP follows, but is not caused by the decline in export revenues.

To investigate further the role of fluctuations in trade-partner GDP, we constructed a trade-weighted measure of trade-partner GDP and extracted the temporary component to such fluctuations, as per the filtering method described above. We then estimated equation (13) using this measure as the instrument for shocks to home-country GDP instead of shocks to export revenues. It turns out this measure is an unbelievably weak instrument, too weak for reliable results. At first blush this would seem to be at odds with the much-replicated results from the gravity models of trade. Upon further reflection, it is in fact evidence regarding a different frequency of fluctuation. Gravity models are estimated on cross-sectional data and thus constitute evidence for long-term stable relationships. Our exercise was evidence of the high-frequency correlation between trade partners' GDP. This weakness of the high frequency correlation suggests that we need not worry too much about endogeneity from scenario C.

To reiterate, because our instrument is both filtered and lagged, it is difficult to tell a story of a type of shock which is likely to deliver reverse causality with the requisite lag structure. Such stories are not impossible, but they seem neither likely nor commonplace.[15] It is also worth reiterating that weak instrument bias and endogeneity bias are likely to bias the coefficient upwards and thus closer to 0 rather than –1.

4.6 How Big are these Shocks?

Our estimates fail to find discernable permanent effects of temporary shocks to per-capita income due to changes in export-revenue. The implication is that there is no alternate steady-state nearby into which a shock may jostle an economy.

15 Perhaps the most frequent comment we receive is that shocks to export revenues do not occur ceteris paribus but may themselves cause changes in the economy. For example, a jump in the world price of natural resources may lead, via the voracity effect (Tornell and Lane 1999) to a jump in taxes and transfers thereby inhibiting future investment causing a productivity slowdown. Might concomitant effects then obscure or offset the transition between equilibria, confounding our empirical strategy? Of course such channels exist: the strength of these effects is precisely what we attempt to measure. We have tried to argue in this section that our identification strategy remains valid. In essence, such comments suggest there might exist several feedback loops, some that amplify shocks and deliver multiple equilibria, others that dampen or reverse shocks. It is true that we cannot separately identify the various channels. We feel it is more relevant to measure and address the mix that exists in practice.

Nonetheless, because our shocks are of limited size, we cannot rule out the possibility that an alternate basin of attraction exists beyond the range of these shocks. Thus, rather than a repudiation of multiple steady-states, these results deliver a lower bound on the size of the basin of attraction in which an economy currently rests. Table 2 shows that these shocks are not inconsiderable. For our basic specification using the whole sample (Table 1), the median shock is a cumulative change of 11.1% of export revenues (3.3% of GDP) over 3 years. When we limit the sample to the largest shocks (Table 3), we are, by the end, looking only at shocks in excess of 43.4% of exports (9.7% of GDP). Thus we have evidence that shocks with considerable impact on current GDP have no discernable permanent effect on the steady state growth path.

It is true that while these largest shocks represent considerable fractions of domestic GDP, they are nonetheless small compared to the cross-country differences in income that models of multiple equilibria seek to explain. We cannot yet rule out multiple equilibria as an explanation for convergence clubs. However, such models are clearly less interesting from a policy perspective if shocks that are empirically plausible fail to shift an economy from one equilibrium to another.

In particular, the case for foreign aid frequently invokes the idea of providing sufficient extra resources to push an economy from stagnation to sustained growth. Kraay and Raddatz (2007) note "recent calls for across-the-board debt relief and a major scaling up of aid to help poor countries achieve the Millennium Development Goals have been significantly influenced by the idea that these countries are stuck in poverty traps and that major pushes are required to break free of these traps" (pp. 315–316). As we explain in the introduction, low income countries received aid averaging 11.7% of GDP in 2010. The Monterrey Consensus calls for increasing official development assistance from the current 0.37% of high countries GDP to 0.7%, an increase of 89%. If similarly concentrated on low-income countries, this would produce additional aid averaging 10.4% of low income countries' GDP. Unfortunately, our results show that shocks of this magnitude do not produce permanent effects (Table 3, column 5) casting doubt on the existence of growth paths which are sufficiently proximate to be reachable through foreign aid.[16] While many justifications for foreign aid remain, our results suggest that enabling escape from a self-sustaining poverty trap is not one of them.

16 It is sometimes noted that aid is not a temporary shock, but a permanent flow. Even so, if a movement of this magnitude does not produce an escape from the poverty trap, there will be no additional effect. Standards of living will be increased by the amount of foreign aid, for as long as it is sustained, but there will be no sustainable take-off to sustained growth and convergence.

5 Conclusion

There are a variety of theoretical mechanisms capable of generating self-reinforcing poverty. Traditional macroeconomic tests of poverty traps often have trouble distinguishing between a world characterized by multiple steady states and a world characterized by twin peaks in the distribution of parameters. We focus on one clear distinction between these two worlds: large, temporary, exogenous shocks to per capita income ought to lead to permanent effects if alternate steady states exist nearby.

We construct a modified Ramsey model including technological growth to inform our specification. Motivated by the model, and by the widespread view that export revenues are exogenous to small open economies, we use export revenues as our shock and look for permanent effects on the growth path of per capita GDP. We check that the effect is robust to non-linearity in the size of the shocks. We also check that the effect is robust to countries in different growth regimes as per recent literature. In all specifications, we are unable to reject the null hypothesis of no permanent effects suggesting a world with a single proximate steady state.

Several caveats are in order. First, while our point estimates are very nearly –1, our 95% confidence interval typically stretches from −0.76 to –1.25 (see Table 1, column 2). This range admits a number of coefficients that are consistent with some small permanent effect. However, it is clearly sufficiently narrow to rule out the dramatic shift to a new basin of attraction envisioned by poverty traps models. Shocks of plausible magnitude are unlikely to deliver a take-off (or collapse) resulting in a change in a country's convergence club membership. Second, one might reasonably claim that these export-revenue shocks are not large enough to conclusively demonstrate the non-existence of multiple steady-states given the huge range of variation in income across countries. We agree. Nonetheless, our results constitute evidence that even shocks larger than 9.7% of GDP leave no discernable permanent impact on a country's steady state growth path. Third, it is possible that short-run fluctuations in export revenue are not relevant to certain feedback loops. For example, suppose the quality of a country's legal institutions tend to improve with per capita income. Are institutional reforms likely to make great strides in response to a 3-year period of greater export revenues? Perhaps. A government with full coffers has a greater capacity to, for example, pay judges and policemen and combat judicial corruption. However, it is reasonable to suppose that short-lived revenue influxes do little to systematically improve institutions. As a result, our test may be a less convincing rebuttal of feedback mechanisms displaying considerable inertia.

Despite these caveats, the lack of discernable evidence of long-lived growth effects from short-run shocks of empirically plausible magnitudes raises the question of whether development models of multiple equilibria are relevant to decisions available to policymakers.

Appendix

Table A1 The Sample of Countries and the Years of the Initial Shocks.

Country	Years of Initial Shock	Freq.	Country	Years of Initial Shock	Freq.
Algeria	1962–78	17	Malawi	1964–79	16
Argentina	1962–79	18	Malaysia	1962–78	17
Bangladesh	1972–78	7	Mali	1962–79	18
Benin	1962–78	17	Mauritania	1970–78	9
Bolivia	1962–78	17	Mauritius	1968–79	12
Brazil	1962–78	17	Mexico	1962–79	18
Burkina Faso	1962–79	18	Morocco	1962–78	17
Cameroon	1962–78	17	Mozambique	1975–78	4
C. Afr. Rep.	1970–78	9	Nicaragua	1962–79	18
Chile	1962–79	18	Niger	1962–79	18
Colombia	1962–78	17	Nigeria	1962–79	18
Congo	1962–78	17	Pakistan	1962–79	18
Costa Rica	1962–79	18	Paraguay	1962–78	17
Côte d'Ivoire	1962–78	17	Peru	1962–78	17
Dom. Rep.	1962–78	17	Philippines	1962–79	18
Ecuador	1962–79	18	Rwanda	1962–78	17
Egypt	1962–78	17	Senegal	1962–78	17
El Salvador	1962–78	17	Sierra Leone	1970–79	10
Gabon	1962–79	18	Somalia	1970–79	10
Gambia	1965–78	14	Sri Lanka	1962–71	11
Ghana	1962–78	17	Thailand	1962–78	17
Guatemala	1962–78	17	Togo	1962–79	18
Honduras	1962–79	18	Trin. & Tob.	1962–65	4
India	1962–78	17	Tunisia	1962–79	18
Jordan	1962–78	17	Uruguay	1962–79	18
Kenya	1963–78	16	Venezuela	1962–79	18
Korea, Rep.	1962–79	18	Zambia	1964–78	15
Madagascar	1962–79	18			

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