Theories of “Bad Policy”

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Recent growth theory fails to provide a convincing account of underdevelopment in terms of economic “fundamentals”. As a result, many accounts cite “bad” government policy (including the failure to support appropriate institutions) as a causal factor behind stagnation. Yet this perspective is hard to understand from the viewpoint of welfare economics. This paper studies theories of endogenous policy which can possibly account for such bad policy. I stress three (interrelated) general intuitions about the causes of bad policy which apply, irrespective of the type of political regime: (1) inability to use transfers to separate efficiency and distribution, (2) inability to commit, (3) the close connection between development and changes in the distribution of political power. I particularly stress the ability (or inability) of these theories to explain cross country differences.

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1. Introduction

“Economic Growth requires reasonably efficient economic institutions. Poorly managed economies, such as those with the absence of secure property rights, autarchic trade policies, convertible currencies and so forth, are unlikely to experience convergence no matter what the underlying production technology or initial level of human capital. Since many, if not all, of the non-convergent countries failed to maintain adequate eco-
In this paper I provide a new perspective on the recent literature on economic growth and development. Much of the literature has converged on a position where bad growth performances and underdevelopment are caused by inappropriate or "bad" government policies. This, in itself, cannot be a satisfactory way of thinking about the issues. While in simple growth models government policy is often treated as exogenous, in reality this is not so. Thus we need to develop coherent theories of "bad policy" (and perhaps more generally "bad" economic (and perhaps social and political) institutions) to understand why apparently bad policies are ever adopted. Such a perspective radically changes the implicit policy implications of the new growth literature (that governments ought to adopt "good" policies). Such advice is, at best, obtuse, without a convincing theory of bad policy (see Basu 1996 and Bhagwati and O'Hare 1994, for perspectives on this point). While there are a number of such models existing in the literature, some of the most popular are clearly at odds with the data, and none help us develop general intuitions which would be useful in giving policy advice. This paper is then a call for more explicit, empirically substantiated theories of bad policy. I argue that this will almost certainly necessitate a more convincing theory of the role and nature of the government and the state than that presently on offer in the economics literature, and in particular, what sort of social groupings or 'cleavages' are important in influencing policy.

The paper proceeds as follows. Section 2 surveys recent theoretical and empirical advances in the growth literature and shows how the emphasis on bad policy has developed. Following this, section 5 proceeds to examine a series of different models which might explain why governments adopt bad policies. Before discussing these theories, sections 3 and 4 reflect on two key issues: how to model the structure and objectives of the state and the precise definition of bad policy (in particular, the sense in which bad policy is inefficient). Section 5 concludes with a preliminary evaluation of the theories. I conduct this evaluation by asking, in the context of the existing models, how did governments which did adopt better policies manage to do this? This is the key to understanding cross country growth experiences from the bad policy perspective and it gives us a good metric for assessing the plausibility of the different models.

In section 6 I try and move beyond existing models and develop what I think are the most general ideas about what causes bad policy. I stress that
it is important to develop intuitions which apply to all regime types (e.g., dictatorships and democracies). While some of these are implicit in existing models, they are either embedded in implausible institutions, or obscured by a focus on other issues or ad hoc extraneous assumptions. In response to this I try to develop a heuristic framework (inspired by work in political science) for structuring our thinking about the political economy of a particular country. Moreover, what I consider to be the most general idea (the third idea below) has not been considered in the literature. The first idea focuses on the inability of governments to commit to policy. The second is the inability of governments to use policy tools to separate distributional and efficiency issues, and the third is the fact that development and changes in political power are closely related. These ideas are interrelated. For example, I argue that the reason that inseparabilities of efficiency and distribution are so prevalent is not for conventional reasons, but because of the effect that efficient policies have on the distribution of political power. I also argue that whether or not commitment is a problem in practice hinges on how quickly development affects political power. In the final section I return to the question implicit in much of the paper: whether or not bad policies are really at the heart of underdevelopment.

This paper complements several recent papers which have addressed the issue of policy reform, in particular, Coate and Morris (1995b,c), Rodrik (1993,1996a), and Tommasi and Velasco (1995). These papers study the nature of policy transitions in developing countries under the maintained assumption that new better policies (free markets and trade, prudent fiscal policy) are replacing old bad policies (inward looking development, populist macroeconomic policy). As a part of this project they propose various models of the initial adoption and subsequent persistence of bad policy. In particular, Coate and Morris, in their attempt to understand the role that conditionality might play in international lending, discuss versions of what in section 5 I call the stupidity and rational ignorance hypotheses.

Nevertheless, my perspective is different from these papers and, while I implicitly treat the issue of policy transition, I focus more closely on what type of theory can explain bad policy. I also try to develop a broader perspective about the types of institutions and policies conducive to development and the political circumstances under which they may arise. Such a perspective raises the distinct possibility that some types of institutional and policy changes have adverse effects on development. For example, Rodrik (1996a) argues that many countries in Latin America have undertaken microeconomic deregulation to a much larger extent than the successful East Asian economies on the basis of misinterpretations of the Asian expe-
rience. My view of institutional and policy change is closer to that of Frieden (1991) when he argues that "institutional changes—especially altered political structures and regimes—are not the result of demands for these changes per se, but rather are "tools" of socioeconomic actors who find they cannot achieve their policy goals within existing institutions." I think that in any sensible positive model of bad policy there can be no presumption of a unidirectional improvement in socio-economic institutions (the literature is too dominated by recent experiences. For example, in the 1970s we would have been writing about political transitions to dictatorship, not democracy, given the experiences of Latin America and Southern Europe.)

2. Recent Developments in the Economic Growth Literature

Beginning with Romer (1986) and Lucas (1988) there has been a resurgence in both growth theory and empirics. I adopt the view that the point of growth theory is to explain cross country differences in the levels and growth rates of per-capita income (this section is not so much a survey as an interpretation of the literature, for more general overviews see Barro and Sala-i-Martin 1995, Mankiw 1995, and Bardhan 1995.) According to the production function approach, differences in per-capita income must stem from differences in technology or differences in per-capita endowments of factors of production. Therefore, to explain cross country income differences we need to explain why countries possess different technologies and different stocks of factors of production. Factors and technology are accumulated, adopted and developed, therefore we need to study the incentives to allocate resources to these tasks. This is what growth theory does. I first consider the type of explanation that recent growth models have provided to these questions. I then show how these answers correspond to the empirical work.

2.1. Theory

"One of the most surprising features of the process of economic growth is the wide cross-country dispersion in average rates of growth. In the postwar period countries like Japan, Brazil and Gabon saw their level of per-capita income expand at a fast pace while other nations experienced no
significant change in their standard of living. This paper studies a class of growth models in which cross-country differences in economic policy can generate this type of heterogeneity in growth experiences." - Sergio Rebelo (1991)

To fix ideas consider the standard one-sector representative agent model of growth (see Jones and Manuelli 1990 and Rebelo 1991). I concentrate on this model although I agree with Grossman and Helpman (1994) and Romer (1994) that the models which formalize growth in terms of the introduction of new goods or improvements in the quality of existing goods are more satisfactory accounts of growth than the accumulation of a larger stock of homogeneous capital. Nevertheless, these models are subject to the same problems which I isolate below in the one sector model and are considerably more complex to exposist.

There is a single produced good which can be consumed or used as capital. The economy consists of a single infinitely lived agent and another "agent" called the government. The representative agent's objective is to maximize the present discounted value of utility. The agent has an endowment of labor time, and owns the entire capital stock. Capital and labor are exchanged in competitive markets. The solution to the intertemporal optimization problem by the agent is characterized by the condition that the marginal rate of substitution between consumption at any two dates should be equal to the marginal rate of transformation. This intertemporal relative price is the after-tax real interest rate adjusted for capital depreciation. In this model, per-capita income grows over time if the agent wants to have an upward sloping consumption path, or if the after tax return to saving can overcome the forces of impatience and diminishing marginal utility. The key endogenous variable in all this is the marginal product of capital. If this is high, then the return to saving offsets the effects of taxation, depreciation and discounting, and the economy accumulates capital. As a result per-capita income rises. In the familiar case where the production function is Cobb-Douglas, \( AK^\alpha L^{1-\alpha} \), the slope of the consumption path and thus the growth rate of the economy is increasing in \( A \), \( \alpha \) and the elasticity of intertemporal substitution, and decreasing in the subjective discount rate, the depreciation rate of capital, and the tax rate on capital (it is also standard to add the rate of population growth to this list, however, since the demographic transition is one of the most robust stylized facts of development it makes little sense to treat this as exogenous, see Robinson and Srinivasan 1997.) Endogenous growth is generated in a model such as this by putting a lower bound under the marginal product of capital. As long as the return to saving stays sufficiently high capital accumulation keeps per-capita income growing forever.
Endogenous growth models provide a class of models where the steady-state growth rate is determined by economic variables (unlike models which posit exogenous technical change). This potentially gives us a way of understanding the differences in cross country growth experiences. How does such an understanding work? The countries with higher steady-state growth rates are those with more patient consumers who are more willing to intertemporally substitute consumption, those with better technology and those with lower tax rates on capital or lower depreciation rates. Here we have a problem. First, it seems impossible that differences in depreciation rates could explain cross country differences in growth. In addition it is hard to see how a plausible theory of development can be based on the notion that people in some countries are more "pecunious" than others or have differing marginal utility functions. Perhaps this is so, but if it is, we need to propose a deeper theory which involves endogenizing preferences. What about technology? Undoubtedly technology is different between countries, yet if it were really the case that the difference between the income levels of, say Switzerland and Peru, could be explained by them having different values of A then we would need to explain why Peruvians did not feel it worthwhile to adopt the (clearly superior) Swiss technology.

In my view then, the "economic fundamentals" of the standard growth model cannot provide a convincing theory of cross-country growth differences. This leaves government taxation policy. The value of the tax rate can make the difference between a "miracle" and a "disaster". Moreover, taxes are potentially observable. While few lesser developed economies actually have explicit taxes on capital income, the literature conceives of the tax rate as a metaphor for any type of government policy which may adversely affect the return to investment activities. This may include market distortions of various sorts, inflationary finance (perhaps creating uncertainty about returns), corruption and rent seeking, poor enforcement of contracts and property rights, social and political instability. Perhaps in response to the implausibility of "fundamental" explanations of comparative growth experiences, it is these variables which have become the focus of the literature.

There is another possibility about the relationship between economic fundamentals and cross-country growth differences: the idea that multiple equilibria may be a key to understanding heterogeneous growth experiences. The literature now abounds with examples where, for example, externalities and non-convexities can generate multiple steady-state equilibria with the interpretation that countries with identical parameters, but different initial conditions, can converge to different equilibria. These models
are obviously not subject to my critique in the last paragraph. One qualm
with the multiple equilibria literature is that while it is suggestive, there is
thus far little direct empirical support. However, this literature does raise
exactly the issues I continually return to below. Namely, why were some
countries able to escape from low level equilibrium traps when others
apparently cannot? Rather I phrase this question in terms of: why were some
countries able to implement good policies while others were not? In the
conclusion I return to the issue of whether or not this is the most useful way
to think about underdevelopment. As we shall see, both the existing litera-
ture on bad policy and the multiple equilibrium literature, stress the impor-
tance of initial conditions as a key source of divergence in growth perfor-
mance. Note also that models of multiple equilibria typically have the prop-
erty that even temporary government policies can break the economy out
of a trap. Thus if underdevelopment were caused by multiplicity then this
might also be viewed as a sort of policy failure. I do not think it is con-
vincing to argue that once caught in a low-level equilibrium it is not opti-
mal to switch (as in the model of Arthur, 1989) since the evidence suggests
that under the right policies rapid growth quickly ensues.

2.2. Evidence

"So what did we learn...Government policies affect growth. The data, how-
ever, are not fine enough to tell us whether policies of financial repression,
trade distortions or price distortions are causing this correlation. One fact
seems to be clear however: publicly induced disarray is not associated with
large rates of economic growth," - Xavier Sala-i-Martin (1994)

A key problem with the empirical implementation of the above theory
is that most of the explanatory variables are unobservable. In lieu of obser-
vations on key variables predicted by the theory to drive growth rates (pre-
ferences, technology) the recent empirical literature has resorted to using
initial values of endogenous variables, like the rate of human capital accu-
mination (proxied by various school enrollment rates) or the ratio of physi-
cal capital investment to output (I think this is the root of the criticism of
growth theory by Landes (1991) when he argues, "there was no real effort
to probe the sources of the wealth of nations. Rather, the process of growth
was taken as cause and explanation; the how stood in for the why," (ital-
ics in the original).)

These regressions show that countries with high average growth rates
are those with high average investment rates or high rates of accumulation
of human capital. This does not explain why these growth rates were so high. As the recent work on total factor productivity growth in East Asian economies by Young (1994, 1995) demonstrates, saying that the “miracle” was due, not to a high rate of growth of total factor productivity, but rather capital accumulation, does not make it less of a miracle, just a different type of miracle. Thus what we really want to know is why rates of physical and human capital accumulation are different. In response to this the empirical literature has used a large number of other regressors in addition to the variables proxying for physical and human capital accumulation. If these are found to have significant coefficients in regressions then this is interpreted as evidence that they have an independent effect on the growth rate of output holding constant the rate of growth of physical and human capital, presumably because they affect the rate of total factor productivity growth. Exactly what this could mean is unclear since we have so few convincing models of this process. More crucially, it still leaves open the question as to why, for example, East Asian countries managed to sustain such extraordinary rates of capital accumulation over a thirty year period. On this central question the empirical work is silent.

Much of the interpretation has focused on these additional variables and many of these are either government policy instruments or variables which represent aspects of the role of government in society. The common findings are that the growth rate of per capita income is negatively related to various measures of political instability, the ratio of government consumption to output, the rate of inflation, the budget deficit, the degree of “inward orientation” of the economy, the black market exchange rate premium (and other measures of market “distortions” such as “financial repression”) and various types of variables proxying for corruption, the independence of the judiciary etc. Thus the evidence, following the theory, has focused on bad government policy, and the failure of governments to promote or sustain appropriate institutions, as the key issue in explaining cross-country growth experiences.

2.3. Connection to the Development Literature

An attraction of the recent growth literature’s stress on bad policy is that it connects closely with a large, more policy orientated literature, which has put a great emphasis on inappropriate government policy as being at the root of poor development performance in the period since World War II. The absence of a theoretically satisfying growth model had not stopped
empirical investigators discovering connections between policy variables and growth, nor international institutions giving advice based on this. Moreover, numerous famous case studies of underdevelopment (prominent examples being Bates (1981), Frimpong-Ansah (1991), Klitgaard (1990), de Soto (1988)) have pointed the finger at bad policy and wildly dysfunctional economic and political institutions. One of the most articulate advocates of this view has been Krueger (1993). In her account of post-war underdevelopment, governments adopted inappropriate economic policies in the 1940’s and 1950’s with the best of intentions (and in the light of the prevailing academic consensus) but these policies then created vested interests which kept them in place long after their inefficiency became apparent. The difference between the best writing in this tradition and the recent growth literature is that it recognized that policies which appeared to be economically irrational might be politically rational and thus understood the need for a theory of bad policy (on this see Bates (1990)).

3. Which Model of the State

The theories of bad policy I now discuss can usefully be categorized by focusing on the nature of the state or government they assume. There is a basic distinction in the literature between models where the government is a veil, in the sense that it passively implements policy decided by other agents in the economy, which I shall refer to as the Plant State, and those where it is not. In the latter type of models the government has some independence and is to some extent autonomous. These models I shall call the Autonomous State, a key subclass of which is the Predatory State.

Models of the Plant State can be split into four types. The first is the positive model of government behavior implicit in classical welfare economics, this is the Benevolent Social Planner. In this model a normative social welfare maximization exercise characterizes efficient allocations, and if decentralized allocations deviate from this, policy is designed to correct "market failures." It is then assumed that the government adopts the efficient policy. The second model assumes that policy is set by direct voting by the polity and that the government passively implements the voters preferred policy. The salient model here is the Median Voter Model. The third model in this subsection is where society is comprised of interacting interest groups and policy emerges as a result of their joint choices (such as lobbying or rent seeking). In this model usually only the interest groups are
modeled and the institutions of the state are condensed into the black box linking lobbying to outcomes (such as transfers). This is the Interest Group Model. The final model is where the state is modeled more explicitly and where competing political parties represent different sets of interests in society. In this case policy is the outcome of political competition. I refer to this model as the Political Party Competition Model. In these models the state is and is not a veil in the sense that agency issues are often considered (parties may be agents but not perfect agents of a particular coalition of interests in society).

When the state is assumed to take on a more autonomous form there are two models: the so called Predatory State model where a dictator, autocracy or elite sets policy directly (perhaps subject to a “revolution constraint”), or alternatively an extension of the interest group model where the state is itself a separate interest group in competition with others. Since the issues which arise with the interest group model are identical, whether or not the state is itself one of the groups, I shall use a single terminology.

A major research topic ought to be trying to decide which of these model is the most descriptive, and to the extent that some emerge out of others (as democracy emerged in Western Europe and the United States in the 19th century from far less representative systems), exactly how this process occurs and how it matters for policy and development. Most of the models I examine below exogenously assume some form or structure for the government and deduce the consequences of this.

For the central issues of development the consensus opinion is that the Predatory State and Interest Group Model are the most interesting. To believe that the Benevolent Social Planner model is descriptive requires an extraordinary belief in the irrelevance of institutions, as does the Median Voter Model. With respect to the latter model, there seem few economies where the process of policy making can sensibly be described in this way. In reality, we have representative (not participatory) democracies with a host of institutions to which citizens delegate policymaking. It seems implausible that the policies which emerge from such systems passively reflect the preferences of the median voter. Moreover, while there are enduring democracies amongst the developing world (such as India and Colombia) the idea that the preferences of all individuals count equally in the political system seems farfetched.

To my knowledge there is only a single application of the Political Party Competition Model in the development context (though this is probably more descriptive of the democratic systems in the cases of India and Colombia), which is Roemer (1994). This model is now the salient model
for thinking about developed economies and seems like a very important area for future development.

While for all of these models we can describe certain types of bad policy, I will stress that, thus far, they have been unable to convincingly generate answers to the question: why have policies have been so different across countries? The models do provide certain answers to these questions. For example, the median voter model in section 5.4.1, argues that the difference between good and bad policy in democracies lies in income distribution. More inequality leads to high taxes and low growth. Unfortunately, most of these answers are inconsistent with other available evidence. Here it is also important to remember that there is no simple empirical relationship between democracy, dictatorship and growth (as Przeworski and Limongi 1993, Hellwell 1995 and Barro 1996 have all recently shown) hence in modelling policy choices of dictators (as well as democracies) we need a framework rich enough to generate good or bad policies in either institutional environment.

The key problem with all of these models however is that they neglect the structure of the government as an institution, and provide only the most stylized representation of the links between state and society. Both of these things may turn out to be the key determinants of whether or not policy is bad. For example, what may be important is not so much the choice of policy, but rather how it is implemented (see Evans 1995 for a convincing exposition of this view). Moreover, the lack of a model of the structure of state-society relations means that the models are polar extremes. Either the state is completely autonomous (as in the Predatory State) or it is a complete epiphenomena (as in Median Voter Model). We do not know if the results from these models are a good approximation to something more realistic. In section 6 when I try and move beyond existing models I propose what I think is a more general way of trying to think about some of these issues.

4. Bad or Inefficient

How should we define bad policy? How do we know when a particular policy is bad or not? Implicit within economic theory is one distinction between good and bad policy: good policy is policy which is Pareto efficient and bad policy is one that is Pareto inefficient. Is "apparently" bad policy simply a misinterpretation of a socially optimal policy under the Pareto criterion?
It seems hard to imagine that it is the case that, under any sensible criterion of optimality, one cannot make comparisons of the average welfare of citizens in say, Haiti and Sweden. If it is, it would imply that it was optimal for millions to live in poverty without access to basic amenities such as clean water and waste disposal. While development seems to represent a massive potential Pareto improvement, it is easy to imagine models where underdevelopment could be part of an optimal growth path if optimality was restricted to the implementation of actual Pareto improvements, particularly if the government has a restricted set of fiscal instruments (taxes and transfers) at its disposal. Practically no change in the economy or any policy that you can imagine is a Pareto improvement outside of the world of representative agent growth theory.

Nevertheless, I feel it to be much more productive to think about the existence of potential Pareto improvements and why they are not exploited. Adopting too severe a definition of constrained efficiency seems unlikely to be fruitful. As we shall see in section 5.4, a sensible definition of what is bad policy is often consistent with Pareto optimality. A key part of this way of thinking is precisely to ask why the government is not able to use fiscal instruments to make potential Pareto improvements actual, or put another way, why the Second Fundamental Theorem of Welfare Economics fails to hold. This is a deep and under-researched question in political economy to which I return in section 6. This perspective throws light on the standard argument implicit in political science about bad policy. This is that bad policy may be more politically expedient than good policy. Politically expedient usually means that the policy fulfills some distribu-
tional goal of the political decisionmaker. However, usually there is no well articulated theory of why distribution cannot be separated from efficiency (as in the Second Welfare Theorem). With this in mind I shall think of a good policy as one which maximizes total surplus and the movement between bad and good as being one of potential Pareto improvement. This allows a bad policy to be Pareto optimal.

This is the general relationship. In reality there is considerable controversy about whether or not a particular policy is bad or good. There is almost complete consensus here about macroeconomics (such as inflation and budget deficits) but much less about microeconomics. For example, in the East Asian context the government of South Korea deliberately provided subsidized credit to chosen firms. Was this a good policy? This is an empirical issue which I try and sidestep in this paper. Whether or not a specific policy is bad or not is not crucial to understanding the general issues discussed here.
With these issues in mind I now turn to the theories. After expositing them and drawing out what appear to be the most important issues, I focus on the way they explain cross-country differences and their plausibility.

5. Theories of Bad Policy

The first two types of theories apply to all possible models of the state, they are also of particular interest because, as I shall argue later, they are the salient ideas as to why a benevolent social planner might change policy.

5.1. Theory 1: "It’s the Economy, Stupid!"

“It makes considerable sense for the World Bank…to push very hard for liberal policies in developing countries, given their demonstrated tendencies to engage in economically irrational interventions.” - Paul Krugman (1992)

It seems that Paul Krugman is an advocate of Theory #1. The first hypothesis that one could isolate, and one which underlies much policy advice based on traditional conceptions of normative welfare economics, is what I shall call the stupidity hypothesis. It could be the case that governments which adopt bad policies are simply ignorant of the First Welfare Theorem, or the effects of distortions in the economy on its growth rate. This view is widely held. To take another example, Dornbusch and Edwards (1995) overviewsing the results of papers reflecting on stabilization experiences remark, “populist regimes have historically tried to deal with income inequality problems through the use of overly expansive macroeconomic policies. These policies, which have relied on deficit financing, generalized controls, and a disregard for basic economic principles." If this were true, the comparative wealth of nations could easily be explained by rich countries being more informed or perhaps more intelligent. While this is a possibility I move onto more “rational” theories which assume that all individuals know the structure of the model they operate in.

5.2. Theory 2: Incorrect (but Bayesian Rational) Equilibrium Beliefs

"A huge amount of government money goes into subsidizing the price of
fertilizer in India. Attempts to remove such a subsidy have turned out to be politically impossible and the majority of those opposing the removal do so not because they are themselves adversely affected by it (as public choice theory suggests) but because they believe that removing the subsidy will be bad for the economy.” - Kaushik Basu (1992)

The structure of the economy is uncertain. In policy matters people advocate different mappings from policies to outcomes not just for self-serving reasons, but also, as Basu notes, because they differ in their beliefs about how the economy works or what the values of key parameters are. Different policies across nations can therefore be adopted because of differences in the government’s prior beliefs about their effectiveness. I call this the rational ignorance hypothesis. Could this explain bad policy? Imagine in the 1950’s that you had read Prebisch and Rosenstein-Rodan and were convinced that inward looking industrialization and import substitution were optimal policies (in the meantime economists have developed models of coordination failures and endogenous comparative advantage showing precisely how policy intervention to distort resource allocation and impede capital flows can be optimal, thus deepening the mystery of the failure of these policies (see Murphy, Vishny and Shleifer 1989, Matsuyama 1992 and Chamley 1992.)) Once the policies are adopted, you observe data generated by the economy. The question is: do you learn the true effectiveness of the policy over time. The answer: not unless the information you observe is sufficiently informative.

The simplest example of this is the one-armed bandit problem of Rothschild (1974). Given some prior belief about the expected profitability of playing a one armed bandit you decide on an optimal strategy. If your beliefs are sufficiently pessimistic to start with, you never play the bandit and hence never generate any information which is inconsistent with your initial belief. One can think of this in terms of Basu’s example. Politicians think that removing the subsidy to fertilizer will have disastrous effects on the incentives of farmers. Therefore the subsidy is not removed and in equilibrium you observe no information that is inconsistent with your belief that if the subsidy were removed it would be a disaster (Basu’s interpretation of the Indian fertilizer subsidy is not uncontroversial, see Srinivasan 1985.)

This is an extreme form of learning where learning is costly and exogenous (to learn about the actual effects of changing the fertilizer policy you have to remove it and this could cause huge welfare losses.) The key point is that, when learning is costly, it is rational to be uninformed. A perhaps more plausible model is one where some information actually accrues in
equilibrium. The government observes productivity growth in agriculture, for example. Can such information lead the government to understand the true effects of the policy? Again, the answer is possibly but not necessarily. I extend Basu, inspired by Piketty (1995).

The government starts with some prior belief about the vector of parameters which it cannot observe and updates it using Bayes rule. Does it learn the truth about policy? Even if the government observes total factor productivity growth this in itself may not be informative enough to allow the full information optimal policy to be deduced. If total factor productivity growth is low it could be that the policy is bad, on the other hand it could be because you were unlucky (since there are many shocks to an economy which might influence technical progress). You might argue that one particular country could gain information about the effectiveness of learning by doing by observing what other countries were doing. But here again there are many interpretations of the evidence (just think of the controversy surrounding the extent to which industry policy was an integral part of the East Asian Miracle.) To what extent can you believe other people’s opinions if they begin with different prior beliefs and hence interpret the data in different ways?

This model raises the question: how do we know that we have the right model? Notice that in some respects this model, if used as a basis for policy advice, is closely related to Theory 1. How is it that the countries giving policy advice know the true model?

5.3. **Theory 3: Satiation**

“If your mouth is the only mouth on the udder of the cow you do not care if the cow becomes smaller.”—Oginga Odinga (1992)

One very common hypothesis about bad policy is the *satiation hypothesis*. This is that a *Predatory* or *Autonomous State* which is interested only in maximizing its own welfare, is indifferent about policy choice and thus may easily adopt bad policies. This is because, whatever policy it chooses, national wealth is large enough to satisfy it. In this case even if the economy collapses around it there is still plenty to go around. This seems to be the position of Kenyan dissident Oginga Odinga commenting on the economic policies of President Daniel Arap Moi.
5.4. Theory 4: Democratic Politics

5.4.1. Direct Voting over Policies

One central model of bad policy in the literature on the Plant State is the one where individuals own different stocks of capital and there is majority voting over the tax rate. This model, developed independently by Bertola (1993), Alesina and Rodrik (1994) and Persson and Tabellini (1994), has the prediction that the lower the capital stock of the median voter relative to the mean, the higher will be the equilibrium tax rate (since the median voter loses less from taxation compared to the gain from transfers) and hence the lower the growth rate. Here the key variable determining the growth rate is the distribution of capital. This model gained credence since empirical work suggests that initial inequality of land or income has adverse effects on subsequent growth rates (this empirical result was discovered by Alesina-Rodrik and Persson-Tabellini, and has since been verified by, Clarke 1995 and Perotti 1996.) The model predicts that the more unequal a society is, the higher the equilibrium tax rate and the more debilitating redistribution.

There are some key problems with this model. First, the evidence finds the relationship between inequality and growth is independent of the regime type. It holds both in democratic and authoritarian regimes and yet the voting mechanism only applies to democracies. Second, there is little evidence that redistribution is debilitating. Indeed, empirical work robustly finds that the proportion of government transfers in income is positively related to growth (see Sala-i-Martin 1992, Perotti 1994 see also Atkinson 1995.) Third, highly unequal societies, rather than having a lot of redistribution, tend to have very little. In fact, it is developed economies which tend to be both equal and have a large amount of transfers. Thus while the voting model is suggestive, the actual mechanism seems wrong (Saint-Paul 1994 shows that the connection between inequality and the behavior of the median voter asumed by the model is not general since inequality can worsen in sensible ways without a change in the equilibrium tax rate.) Lindert (1991) comments on the voting model by noting that, “the recent idea that pre-fisc income equality promotes growth by bringing…lower taxation fails to fit either the OECD data set or the wider history. It is not true that wider income gaps raise taxation and transfer spending and we have even seen reason to doubt the next link, between greater redistribution and slower growth.”

The question of whether or not median voting induces inefficient policies has been extensively treated in public economics. Bergstrom (1979) shows
that only in a knife edge case will the median voter choose a level of a public good which satisfies Samuelson's condition for efficiency (in the sense of total surplus), but this result really stems from the exogenous restrictions on policy instruments necessitated by the use of the median voter theorem (see Besley and Coate 1995a for a lucid discussion of these issues). Notice that, while this model applied to redistributive taxation explains low growth, it in fact generates a Pareto efficient growth path given the restrictions on the policy instruments. This is so since the policy is chosen to maximize the utility of the median voter and thus any other growth path makes the median voter worse off. However, this path, while Pareto efficient, is not generally total surplus efficient. In reality governments are not so exogenously restricted. To be descriptive such an explanation would really need to propose a theory as to why other fiscal instruments are not available, or if available, not used.

The general message of this model is that for an economy to grow, resources need to be concentrated in the hands of individuals whose private interests coincide with this objective. If policy is determined by others with less of an interest in accumulation, then bad policy occurs since they divert resources away from accumulation. My position, discussed in section 4, is that accumulation is almost certainly in the potential interests of all members of society and from this perspective, by its exogenous restrictions on policy, the median voter model fails to address the key issues.

5.4.2. Other models
The hegemony of the Downsian model of political competition has meant that the development of more satisfactory models of political competition (where parties care about policies as well as whether or not they are elected) has been a relatively recent phenomena. The literature is still very unsettled as to whether or not these types of models predict that policy choice will be efficient or not, and if they are inefficient (or "bad"—recall that these two things may not be the same thing), exactly what aspect of the political system causes this. Moreover, as noted above, these types of models have not been applied to growth models or development issues. Recent work by Besley and Coate (1995a,b) finds that policy is efficient in the static model of representative democracy that they consider (essentially because the policy chosen is that which maximizes the utility of the elected politician and any other policy makes at least that person worse off).

They do find, however, that dynamic considerations may induce various types of inefficiency in policy choice. These stem from the fact that current
policies may affect both the identity and policies of future governments and this may lead the present government to adopt, for example, inefficient policies in an attempt to manipulate these. While the particular model of political competition that Coate and Besley develop is probably not a fruitful framework in which to understand development, their work does embody one aspect of what I feel to be a key intuition: that development and the distribution of political power are correlated. In particular, they show that in a two-period model, a government in the first period may not adopt a productive investment because of its affects on the second period political equilibrium (their results generalize and unify some previous work of Aghion and Bolton 1990 and Milesi-Ferretti and Spolaore 1994.) Specifically, the policy strengthens the desire or ability of political opponents to contest elections, thus reducing the probability that the incumbent will be reelected. I return to this idea in section 6 where I try and draw out some of its wider implications.

The oscillation in policy created by democracy is also a potential source of inefficiency. Howitt and Wintrone (1995) and Piketty (1993) argue that this instability coupled with risk aversion can lead to policy inertia since parties are afraid to raise issues for fear of “opening a can of worms”, and Roemer (1994) has argued that this may generate a desire for policy stability which dictatorship may satisfy. More generally, electoral uncertainty can make it difficult for a democratic government to commit to a policy (an issue I return to in section 6.2.)

A key intuition behind arguments for democratic efficiency is that free entry into the political system generates a presumption towards efficiency. Inefficient policies (like firms) will be weeded out by political entrepreneurs offering better policies. Nevertheless, this assumption of free entry is a strong one. Ades and Verdler (1993) and Ades (1995) have explored the implications of models with costs to entering politics.

5.5. Theory 5: Non-Cooperative Strategic Interaction

Many accounts of bad policies assume this setting and it comes in three basic forms. The first two, Prisoner’s Dilemma models and Encompassing Predators, exploit inefficiency of the classic type associated with non-cooperative Nash equilibria. While many of the issues are the same, the key difference between them is that the second version views the government as a Predatory State who is a Stackelberg leader and introduces the notion of an “encompassing” interest to distinguish between cases where good and
bad policy arises. The final type of model which generates bad policy from non-cooperative behavior focuses on commitment problems (and the fact that policies which arise in other models may be time inconsistent), and how differences in their resolution across countries may explain differences between good and bad policy. Unfortunately, despite the claims of the literature, the Predatory State models which assume commitment do not generate bad policies. That they are thought to do so is a misunderstanding as I shall argue below.

5.5.1. Prisoner’s Dilemma Models

The Prisoner’s Dilemma is the basis for the Interest Group Model of the State where policy emerges from the non-cooperative interaction (“lobbying”) of various interest groups, possibly including the state itself. In a seminal application, Bardhan (1984) argues that inefficient public policy (particularly inability to supply infrastructure and public goods) in India is the main reason for its disappointing growth performance over the past thirty years (this is a standard view of India, see Bhagwati 1992) and that these bad policies emerge from the non-cooperative actions of three main interest groups: a relatively autonomous state, wealthy landowners and the modern industrial sector. The two non-government sectors lobby the state for transfers, which in turn tries to maximize its own welfare. The outcome of this process is inefficiently low supply of public goods, since too much of tax revenues are dissipated in transfers (such as subsidies), or in state consumption.

This model seems to have a large potential for explaining bad policy and frequently appears in formal and informal discussions of policy. It is closely related to the model of rent seeking proposed by Krueger (1974) and Olson (1982), the model of pressure groups developed by Becker (1983,1985), and has also been used to try and understand the microfoundations of social democracy and corporatist institutions in European countries (see Johansen 1979, Przeworski and Wallerstein 1982 and the seminal contribution by Lancaster 1973.)

A general question here is what are interest groups? In particular applications of this model these are usually exogenized, but thought of generally enough, this framework is appealing. We can think of interest groups as particular coalitions of individuals with consonant policy preferences. What sort of interests will form in society? Consider an economy with two sectors, manufacturing and agriculture. In both sectors there are workers
and owners. In attempting to influence policy would we expect to see workers in agriculture form a coalition with owners (so that interest groups form along sectoral lines), or would we expect to see interests form along class lines (so that workers in agriculture find themselves to have more in common with workers in industry than owners in agriculture)? An interesting application of a sectoral approach to understanding the political economy of the Latin American debt crisis is Frieden (1991), while Esping-Andersen (1990) has applied a class analysis to the evolution of European welfare states. I return to the issue of endogenous coalition formation in the next section.

A key question about this model is, why cannot small numbers of coalitions of agents collude to reach an efficient policy? Presumably all agents suffer from the lack of economic development even though at any one time they are choosing optimally. The social irrationality of such non-cooperative equilibria point to the gains from such cooperation. Moreover, if this model is a general paradigm, then we need to understand why other oligarchies did not suffer the same fate. For example, in Japan both the industrial and farm sectors are politically powerful. Why did they not engage in non-cooperative behavior which crippled development?

There are several ways in which the ability of different groups to support cooperative agreements might differ between countries and may change over time. These might give us a handle on the cross country differences. The standard approach to sustaining cooperative agreements in such models is to argue that, if relationships are long-lived and individuals are patient enough then the threat of punishment can sustain efficient agreements. Typically, what is crucial here is the discount rate of agents. Nevertheless, as I discuss in section 5.5.3, it is hard to believe that this is important.

The ability to support cooperation depends on the relative costs and benefits and these are not a constant but may depend on the degree of development of the economy (as embodied in the level of the capital stock for example) (this idea has been developed formally by Benhabib and Rustichini, 1996). Thus an economy which started out with relatively good fundamentals could move onto a high growth path because this allows for cooperation amongst different factions in society. Another interesting possibility is that the status quo distribution of wealth, income or access to political power affects the ability to support cooperation. This is so since it determines the disagreement point in cooperative agreements and the ability to influence decisions and hence the part of the Pareto frontier which is feasible to support with cooperation. This in itself might not be a problem
if an arbitrary efficient point could be implemented, but this is not necessarily so. Inducing economic development involves creating a set of institutions in society which feature large indivisibilities and irreversibilities. These will influence the distribution of income in ways which cannot be controlled. Thus the initial distribution of resources or power may determine the possibilities for reaching cooperative solutions.

Despite these ideas, I think the most useful approach is to understand exactly what types of groups will form and which gain political power. This seems to have more explanatory power than focusing on the parameters which determine whether or not cooperation is feasible. I move to this issue in section 6.

5.5.2. Encompassing predators

It might seem that a Predatory State would be able to mimic the policy of a social planner and, given a rich enough set of instruments, extract all rents for itself, thus adopting good policy (just as a discriminating monopolist would.) In fact much of the literature on this topic, for example Findlay and Wilson (1987), Findlay (1990), Olson (1982), McGuire and Olson (1996) and Lal and Mynt (1996), concentrates on a particular set of conditions under which this is not so. In these models the state is viewed as a revenue maximizing Stackelberg leader with respect to the private sector, whose fiscal instruments are restricted so that the conditions for the maximization of autocratic revenues typically are interpreted to imply that there is a socially inefficient undersupply of public goods, and a high tax rate. The predator is further assumed to be able to credibly commit to any tax rate. However, if a state is large enough and has an “encompassing interest” in society then it will have the incentive to adopt good policy (not wishing to “kill the goose that laid the golden egg”).

What is the intuition behind the adoption of bad policies in these models? In them the predator chooses tax rates and the supply of public goods subject to technological constraints and possibly an exit technology for citizens (perhaps a “revolution constraint” or the possibility of moving into the informal sector.) In most models, the Predator’s program is to maximize his utility subject to these constraints. The allocations are then compared to some benchmark to assert their inefficiency. Here is the problem. Findlay and Wilson (1987) and Findlay (1990) both calculate the Predatory equilibrium assuming the tax rate is exogenously fixed and show the predator undersupplies public goods relative to a situation where the public goods
are chosen to maximize output. Grossman and Noh (1994), McGuire and Olson (1996) and Lal and Myint (1996), all compare the predatory equilibrium to one where policy is chosen to maximize the utility of citizens with the government budget balanced. But this is the wrong benchmark since, if the predator is a citizen, then his utility ought to be taken into account in the social planning problem. In fact, allocations in predatory state models are really just those from a particular type of social planning problem with a large weight given to the utility of the predator. Recall that Pareto optimal allocations can be characterized by maximizing the utility of one agent subject to feasibility constraints, and subject to the utility of other agents being at least at some level. In predatory state models the exit technology implicitly delivers the last constraint, and hence inefficiency can only be stemming from exogenous restrictions on instruments. But this is only inefficiency with respect to the first-best allocation. A social planner subject to the same set of instruments would pick the same allocation. There is therefore an isomorphism between predatory state models and social planning problems. In the cases (McGuire and Olson 1996, Lal and Myint 1996) where predatory equilibria are claimed to be inefficient, this is due to either the predator being forgotten when the social planning problem is chosen, or not realizing that inefficiency stems from exogenous restrictions on instruments which would also constraint any other political mechanism (and in particular would have to be used in making transfers to the Predator consistent with his weight in the social welfare function.) Thus these models do not actually generate any useful intuitions which connect predatory policy with inefficiency or bad policy.

The idea behind having an encompassing interest seems, at one level, intuitively correct. As defined in McGuire and Olson, the degree of encompassing is measured by the proportion of factor income accruing to the predator. The larger the "stake" of the state in society, the better the incentives to provide socially beneficial public goods. Unfortunately, history seem full of dictators who, under any reasonable definition, have had an encompassing interest in the economy and yet presided over the collapse or destruction of the economy. To illustrate this point let me consider two (of many) examples drawn from Robinson (1996b). Between 1885 and 1908 the Congo (modern Zaire) was not a colony of Belgium but rather the personal possession of King Leopold (see Gann and Duigman 1979.) Thus King Leopold had a perfect encompassing interest. Nevertheless, during this period his policy toward the Congo has been characterized as "based on the violent exploitation of natural and human resources" with a consequent "destruction of economic and social life, (and) the dismemberment of polit-
ical structures” (see Jewsiewicki 1983.) None of this behavior seems consistent with the idea that an encompassing interest provides autocrats with the incentives to adopt socially optimal public investments. An equally enlightening example is that of the dictatorship of Rafael Trujillo in the Dominican Republic between 1930 and 1961. Wiarda (1968) observes that the Dominican government under Trujillo “could be summarized by the single word ‘grab’. During his time in power Trujillo expropriated much of the land and businesses of the country so that he eventually directly controlled about 85% of the economy (see Wiarda 1968 and Vedovato 1986) and owned 60% of all land. Nevertheless this policy did not result in the rapid economic development of the Dominican Republic nor in efficient public investment. Indeed, by the 1950’s 50% of all government expenditure was on the military (Lundahl and Vedovato 1989.)

The problem with the encompassing story is that it assumes that efficient “policy choice” by the government (such as providing public goods) can be manipulated without influencing the ability of the predator to extract resources from society. In fact, in order for the economy to prosper the key issue seems to be the creation of well defined and enforced property rights, an independent judiciary and system of law enforcement and a host of other institutions. These are not the same as a dictator choosing the level of a homogeneous public good. Moreover, few dictators actually raise revenues through explicit taxes and the creation of the institutions required to get the economy growing clearly erode the ability of dictators to extract rents from other agents in society (for example by using the army). Thus the institutional prerequisites to development are essentially antagonistic, indeed practically orthogonal, to the maintenance of the power of a dictator. The most direct evidence establishing this is the mass of empirical work showing that the incidence of democracy is positively correlated with the level of development. Here we see another example of the interrelationships between development, the distribution of political power and political equilibrium, which arose in section 5.4.2 and to which I return in section 6.

Of course, if development is a potential Pareto improvement, then the dictator can be compensated ex post, but after institutional changes have altered the power structure in society the political system will be unable to commit itself to compensate former dictators. Hence, independent of the degree of encompassing interest, we rarely observe dictators adopting efficient policy. The situations where they do, as for example in South Korea, do not seem to be differentiated along these lines. In fact, in reality the connection between the degree to which the autocrat is encompassing and
economic development is probably the opposite of that which this literature postulates. In Robinson (1996a) I study an explicit model of institutional transition, namely democratization. Dictators cede political power if forced by the threat of political instability and social conflict and the threat of these is increased by capital accumulation. Hence, while accumulation may increase total income it may induce institutional transition which is unfavorable to the autocrat. If a dictator loses political power then he does not gain from development and will oppose it. Thus a dictator may wish to slow accumulation. Other things equal, the more encompassing the dictator initially, the greater his ability to control accumulation and the less likely is development. In this model, and contrary to the literature (for example Levi 1988) the more the dictator values the future, the worse is policy. The reason for this is that the more the dictator cares about the future the greater his losses from transition and the greater his incentives to oppose it.

5.5.3. Time Inconsistency of Policy

The Stackelberg model also endows a dictator with an ability that in reality they do not possess: the ability to commit to a policy. In a dynamic setting optimal policies are typically time inconsistent and cannot be implemented without commitment. Standard arguments in game theory suggest possible ways that commitment may be attained. For example, good policy can be sustained by punishment strategies. Private sector agents can threaten to punish the predator if he deviates from good policies, and if the predator is sufficiently patient such a mechanism can work. Grossman and Noh (1994) show in a related model how such reputational equilibria interact with an endogenous discount rate (which they interpret as the dictator’s survival probability.) If the survival probability depends on the efficiency of his current policy, then the desire to stay in power can improve the efficiency of policy. However, this is only so if the dependence is not too large, because the tax rate has to be sufficiently high to stop the dictator reneging on commitments. If survival is very sensitively related to policy then this sensitivity undermines the reputational equilibria and makes policy worse.

Unfortunately, establishing a reputation rarely seems to be a substitute for the ability to commit. As with encompassing, there seem clear examples of dictators who were established for a sufficient time, had dynastic pretensions, and who were successful in removing all effective opposition for reputation to reap large benefits (again I draw on Robinson, 1996a,b.) For
example, the Duvalier dynasty of Francois and Jean-Claude ruled practically unchallenged in Haiti from 1957 to 1981 (see Lundahl, 1984). During this period South Korea increased its per capita income by a factor of seven while the Haitian economy stagnated. In Nicaragua, the Somoza family ruled from 1938 to 1979, and moreover, were fairly encompassing (it is estimated that they owned one third of the economy at the beginning of the Sandinista revolution, see Dunkerley, 1988, and Close, 1988.)

Reputation then is not easily established. One reason, discussed above, may be that these economies were so poor that it was simply not worthwhile to establish credibility. Another reason may simply be that reputational equilibria, of the type intensively studied in game theory, may simply be an inaccurate description of human interaction, perhaps because any feasible punishment is not renegotiation proof, or because there are significant irreversibilities about cooperative equilibria which rule out reversion to inefficient outcomes being used as threats.

Note that even if reputation can be established, this does not solve the problem discussed above, that if efficient policies change the structure of political power in society they may not be adopted.

5.6 Evaluation of the Theories: What Generates Good Policy

So what did we learn? The Stupidity Hypothesis, while implicit in much discussion of economic policy seems unconvincing. While there is obviously deep uncertainty about the true structure of the economy, there are enough well known empirical regularities about the relationships between different institutional structures and government policies and economic outcomes, that the failure to adopt good policy cannot be systematically due to stupidity. Consider for example the frustration which surfaces in the World Bank's report Adjustment in Africa (World Bank, 1994.) Huge efforts to induce African governments to change policies have been less than fully successful despite the evidence that better policies produce better economic results.

I think that this is probably also true about the Rational Ignorance Hypothesis, though there do seem to be examples where this model is useful. To be convincing this theory would really need to provide some ideas about where initial prior beliefs came from and how learning took place in a way which systematically explains the evidence across countries. For example, what piece of evidence caused the change in South Korean policy away from import substitution towards openness in the 1960's? I think
it is unlikely that this change was caused by the fact that the government learned something they did not already know. On the other hand, the early post-war success of Japan seems to have been very influential in determining policy orientation in South Korea and Taiwan (see Vogel, 1991), just as these countries experiences seem to have had subsequent ripple effects on Indonesia, Malaysia and Thailand. Learning seems to have been important here. A sensible interpretation of the model also explains why South Korean success, for example, should have had little impact in Africa or Latin America. The historical and cultural endowments of these regions are so different that a Latin American country could plausibly reason that South Korea’s success rested to factors which did not apply elsewhere. Here, what is crucial for learning is policy change by a neighbor. In the Latin American case Chile seems to be the main interesting example.

An interesting perspective on this model comes from asking the question: if one believed that policy was chosen by a social planner, why would policy change? Assuming the stupidity hypothesis is implausible, then efficiently chosen policies could change, either because the structure of the economy was non-stationary, or because of uncertainty and learning. The first explanation does not seem relevant to the issues in this paper and the second turns out to have an implausible implication, namely that policy change and political change are uncorrelated. For example, if supply-side economics was implemented because we learned that high tax rates had a disastrous impact on efficiency then it would have been just as likely that it could have been adopted by Jimmy Carter. Similarly, the Bolshevik party could have removed central planning and introduced a market economy without Yeltsin and democratic transition in the former Soviet Union. I think this argument is evidence against the learning hypothesis.

I think the evidence is also against the Satiation Hypothesis as a plausible source of variation in policy. Historical evidence suggests that the avarice of most rulers is, to a first approximation, unbounded. Greater national wealth typically provides many things which rulers enjoy: prestige, a large army, international status and power. I think it is sensible to think that all of these would be desired by rulers if it were feasible to accomplish them in a way consistent with other goals (such as maintaining power.)

The Median Voter Model seems to have little to recommend it as a descriptive model of policy. However, practitioners of this model have at least attempted to use it to explain cross-country differences. Unfortunately, as noted above, the predictions here are wrong. Recent work (by Saint-Paul and Verdier, 1993, and Bénabou, 1995) has adapted these models in ways which let government redistribution have positive economic effects on
investment and growth (because of externalities or imperfect capital markets) and these results tend to strengthen the intuition that redistribution per se is not a bad policy. In fact, lack of redistribution may be indicative of really bad policy. Thus this analysis turns the original models on their heads. Inequality generates bad policy because it does not generate redistribution.

The problem with this story is that there seem to be prominent examples where redistribution was not intrinsic to development. The British state began to redistribute through education and labor market intervention in the 1870's, and with a welfare state in the first decade of this century. The beginning of serious redistribution in the United States was in the 1930's. While these policies may subsequently have been either good or bad, they do not seem to have been the crucial determinant of development. Indeed, East Asian states were not distinguished from others by continual fiscal redistribution, what they did do was to initially redistribute assets directly, or resources which allowed people to invest in assets, the value of which was increased by development.

Other models of democratic politics are not as yet well adapted to examine development issues and we thus have little intuition about what aspects of, for example, electoral competition, can help us explain observed cross-country differences in policies. Presumably, the institutions through which parties interact will be important but so will behavior interacting with these, in particular, as I argue in the next section, the types of groups which form and become politically powerful.

Models of non-cooperative strategic behavior do seem to capture some key issues but, so far, they are not convincingly able to account for cross-country differences or the costs and benefits to different political systems of different institutional structures and policies. Conventional arguments as to how non-cooperative inefficiencies (generating bad policy) can be overcome have thus far not been able to explain cross-country differences (indeed there seems to have been no attempt to even think this through.) What I do think is important is the way these models have thought of policy as determined via the interaction of various coalitions. In the next section I conjecture that within this framework it is perhaps not prisoner dilemma type interactions which cause bad policies, but other general issues which can also be applied to democratic and predatory state models.

Models of the Predatory State need better microfoundations. While the general methodology embodied in these models seems sound, a better integration of economic and political constraints is required before we have a convincing model of policy choice (I further develop this theme in the
next section.) As currently formalized, these models provide little that is useful for understanding bad policy choices.

There seem to be many other aspects of politics that are missing from existing models, particularly, as I stressed in section 3, the internal structure or "capability" of the state, and the structure of state-society relationships. Moreover, they do not satisfactorily integrate political institutions with the nature of public discourse and the cultural and social system in which they are embedded. Kuran (1988, 1995) has shown how these wider issues may be of central importance in understanding policy choice.

6. Understanding Bad Policy

On the one hand we seem to have a plethora of potential models of bad policy, on the other hand, put under the magnifying glass, these turn out to generate very few robust predictions about what might determine whether or not policy is good or bad. In this section I want to propose a general framework for thinking about the determinants of political power and then use this to think through what the general intuitions about bad policy might be. I finally turn to the sorts of variables which may help us understand the comparative international evidence.

I think one useful approach to understanding policy determination is to think in terms of a model where individuals in society can form into coalitions or groups depending on their induced preferences over policy. As discussed above, these groups can form on sectoral lines, along class lines, perhaps along gender, religious or ethnic lines. Another important distinction might be between the public and private sectors, and this gives us a way of incorporating ideas about the autonomy of the state. The types of cleavages that form in society are the fundamental determinant of political activity. All of the above models can be seen in this light. Obviously the Interest Group Model reflects this approach. Crucially, however, interest groups are not endogenous and I think it is this which ultimately leads the model to be thus far empirically empty. Certainly different types of groups have differing abilities to engage in collective action, but it is not this that differs across countries, but rather which groups form to start with. The Predatory State also falls into this category. Models of democratic politics can also be thought of in this way. The Median Voter Model is really a model of rich versus poor with the actual policy being determined by the individual on the margin of these two groups. Similarly, models of Political
Party Competition tend to end up deriving the policy preferences of parties from the core constituencies which support them. In approaching a particular situation of bad policy the first thing would be to assess which types of coalitions are important. This model does not overcome one of my previous criticisms, namely that existing models have not satisfactorily treated political institutions. I think that as an initial step this is defensible because, while evidence in some directions exists about their importance (see for example Garrett and Lange 1995), the development literature has not been able to discover systematic relationships between bad policy and regime type or political institutions.

With this framework in mind what are the general intuitions about what could generate bad policy? Here I think there are three ideas.

6.1. Inability to Separate Efficiency from Distribution

"The early phase of transition to the modern industrial economy is characterized by great internal strains and conflicts, consequences of the shifts in relative economic position and power of various groups affected differently by the increase in numbers and by the opportunities of the new technology. These [phenomena] appear, when viewed statistically, as rather placid movements of steady climbing lines. But under the surface there are major shifts among social groups which may involve serious strains on the pre-existing framework of society geared to a much slower rate of growth."—Simon Kuznets (1968)

A, perhaps the, central theme of existing ideas about bad policy is that bad policies occur because they happen to benefit some politically powerful group and that the Second Welfare Theorem fails to hold. Consider the median voter growth model. In this model the median voter trades off the higher transfers from capital taxes against the lower future wage and transfers caused by lower capital accumulation. The root cause of bad policy is that the Second Welfare Theorem does not hold in this model in the sense that non-distortional transfers cannot be made by assumption. The optimal way to redistribute would be to raise revenues by levying lump-sum taxes which would not distort the marginal incentives to accumulate (modulo income effects.) Hence the exogenously posited restriction on policy instruments is crucial to the result.

The importance of this issue can be seen from the thinking of Kuznets quoted above and has been recently developed at length by Mokyr (1990,1992.) If growth is not Pareto improving, and if market participants
have political power to oppose changes which adversely affect them, development may not occur. Without political institutions to generate compensation, coalitions with a vested interest in old technologies, types of capital, or institutions, will attempt to block change (Krusell and Rios-Rull 1996 provide an interesting formalization of this idea.)

This perspective focuses attention on why efficient policies cannot be chosen along with a set of taxes and transfers which independently implement the distributional preferences of the government. It also suggests that we may be able to explain cross-country differences if we can understand why some countries can generate compensation, while others cannot. This issue has received some, though perhaps not enough attention in the literature. In an influential paper, Fernandez and Rodrik (1991) argue that even though a particular policy is known to be efficient it may not be adopted under majority voting because there is uncertainty about who gains and loses. Since gainers cannot commit to compensate losers ex post the policy is not chosen. This model has the problem that in many policy decisions it is all too obvious who will gain and who will lose. The issue is rather the political power or salience of these different groups (such as their relative degree of concentration). The idea that compensation may not be feasible ex ante and not credible ex post appears widely in the literature (see Dixit and Londregan 1995.) Another important argument is due to Coate and Morris (1995.) They argue that inefficient policies may be used because of their information properties. Taxes and transfers are very obvious ways of redistribution and if a government wishes to redistribute, but hide its distributional preferences from key voters, then it may want to redistribute covertly. One way of doing this may be to adopt policies which, though they are inefficient, nevertheless have the desired distributional impact while revealing far less information. While their model focuses on democratic politics, their ideas apply quite generally. For example, it is plausible that even a dictator may want to conceal the fact that he favors certain groups.

It seems that a general lesson may be that adopting good policies and creating the right incentive and institutional structures have significant distributional impacts which, in the absence of compensating transfers, create trade-offs between distribution and efficiency. Since political institutions and players have distributional preferences (recall that I am urging a view of political economy where society is decomposed into coalitions or groups each of who aims to maximize their own benefits from policy choices) bad policies get chosen because of their distributional effects.

It is hard to argue with this view, however there is a sense in which it
is severely incomplete. For example, while the above models are very interesting, we get no solid intuitions from them which might help us understand why and how the trade-off between efficiency and distribution varies across nations and just what aspects of the political organization of society are key to the inability to generate compensation. In fact neither predators nor democrats seem to be able to compensate efficiently. Indeed, in the last section I argued that the evidence does not suggest that actual redistribution is important for development. I conclude therefore that the standard interpretation of this cannot be crucial for understanding bad policy since countries which seem to have adopted good policy do not seem to have done so by significantly ‘buying off’ vested interests.

How could this be? I think this is surely because good policy arises when those groups which have political power also benefit from good policy. In this case it is irrelevant that others lose since they are insufficiently powerful politically to oppose good policy. Why are cases where the politically powerful lose from good policy so intractable? It ought to be the case that such situations could generate good policies because even though good policies might change distribution disadvantageously to the politically powerful, they can compensate for this through their political control. I think there are two answers to this. The first is a conventional one, an inability to commit. The second is new, and I think possibly more important.

6.2. Inability to Commit

One problem for the politically powerful is that having too much power can be a bad thing since it makes it hard to credibly commit to a policy. For example, implementing good policies to encourage investment may not stimulate the economy if the politically powerless are afraid that in future the returns to these investments will be expropriated. The undiluted power of Predatory States is the problem here, while the electoral uncertainty in democracies can lead to similar problems. Theoretically, reputation building can alleviate this problem, however, I have already argued against the practical importance of this idea. Another possibility seems to be institution building. North and Weingast (1989) and North (1993) have argued that the evolution of parliamentary institutions in Britain was crucial in forcing the government to stick to policy commitments. The mechanism through which this might happen has been developed by Acemoglu and Robinson (1996a,b) who argue that democratization occurs as a way of inducing a credible commitment to redistribution. It achieves this because it alters the
subsequent costs and benefits of collective action. Similarly Robinson (1995) proposes a model where setting up a bureaucracy allows the government to commit to a policy since it creates an effective vested interest in the continuation of the policy. Institutions, also effect distribution (see the next section). Thus even if commitment is possible it may not be privately rational for a policy maker.

Inability to commit may explain bad policy in situations where political power is concentrated in the hands of those who lose from development. Good policy will not stimulate accumulation and growth in situations where the politically powerless fear future policy reversals.

6.3. Development and Political Equilibrium

"Extracting a larger share from a shrinking pie is not the optimal way to maximize revenues, but it may be the only way consistent with the survival of predatory states. The disorganization of civil society is the sine qua non of political survival for predatory rulers. Generating an entrepreneurial class with an interest in industrial transformation would be almost as dangerous as promoting the political organization of civil society. For predatory states, "low-level equilibrium traps" are not something to be escaped; they are something to be cherished."—Peter Evans (1995)

This leads into the third general idea. Theories of bad policy hinge on the interaction of the economy and the polity. Political institutions are created to supply collective goods but they also distribute the costs and benefits of providing these. These affect not just individual payoffs, but also the nature of the future political equilibrium and therefore the future identity of who is to determine the distribution of costs and benefits. It is then important that there is a consonance between the types of institutions and policies which are crucial for development (what I am calling good policies) and their effect on the future political equilibrium. If adopting good policies affects the future distribution of political power and the resulting political equilibrium in ways which are adverse to the present governing coalition, then this will alter their incentives to adopt such policies.

This topic, which surfaced in sections 5.4 and 5.5, seems to be a key source of underdevelopment. While Besley and Coate (1995b) examine these types of issues in a particular representation of a democratic environment, they seem just as pertinent for other types of models of politics. Perotti (1993) shows how the effect of current policy choice by the median voter is influenced by how the policies will effect the future identity of
the median voter. In Robinson (1996b,c) I build several models of good policy and study how induced development alters the nature of political equilibrium. One, discussed above, is that capital accumulation may induce social conflict, perhaps because it induces rising inequality. Another is that development directly affects the structure of civil society (through urbanization, improvements in communication etc.) and the ability to organize collective action. Thirdly, development, by raising the wealth of different groups, also increases their political power (many models have this implication). I show that in these different contexts good policy may not be adopted because of the effects it has on the future distribution of political power. These papers also stress that typically these effects of development cannot be undone by lump-sum taxes (how can one use taxes to offset the impact of groups to engage in collective action caused by development?) Thus distribution and efficiency are inexorably intertwined.

6.4. Potential Explanatory Variables

What do we know about the preconditions in an economy which are likely to be conducive to the adoption of good policy and how do these relate to the empirical findings? In other words, what variables are out there which might help us understand differential policy choices? I now think these through with particular reference to the above general intuitions: for example, what variables could help us understand what coalitions form and are politically powerful (perhaps the key issue)?

6.4.1. Factor Endowments

The obvious starting point in thinking about what types of coalitions have political power and what their policy preferences are, is to think in terms of factor endowments. This approach is taken in the seminal work by Rogowski (1989) and recently applied by Lal and Myint (1996). Thinking about relative factor endowments in general equilibrium allows you to think through what types of initial distribution of income you might observe. One usually then associates income with political power. Then one can study effects of development on distribution and who might have the incentive to oppose change. This is particularly revealing since starting from different initial factor endowments countries will take different paths towards the development of industry and a modern economy. Such paths have different implications for the dynamics of factor prices and income
distribution.

Factor endowments have emerged as important determinants of bad policy in the literature on the "Generalized Dutch Disease". It has been suggested by several writers (for example Karl 1995 and Lal 1995) that resource-rich societies may generate political systems which focus on redistribution at the expense of growth (Lane and Tornell 1995 formalize an aspect of this issue.) Thus South Korea, for example, was blessed with having few natural resources and this aided its development of comparative advantage in manufacturing, whereas the economies of Nigeria and Venezuela collapsed as agents fought for the oil rents.

This approach seems very attractive. For example, relative factor endowments and technology may give us a handle on whether cleavages occur along class or sectoral lines. The main problem seems to be that so far there is no empirical work, mainly anecdotes. Given the rich empirical literature on the Heckscher-Ohlin model this should not be insurmountable. A very promising line of empirical research is to try and tie particular types of bad policies to particular factor endowments.

6.4.2. Institutions

What one treats as endogenous is a modeling choice. One useful approach is to think of institutions as exogenous and consider policy variation within institutions. The spirit of this paper pushes this train of thought towards political institutions. Could these be important? I partially argued against this on the grounds that there is no robust relationship between democracy, dictatorship and development. However, this may just suggest that we have to look at the finer structure of political institutions and their social context. It may not be whether or not the overall system is a democracy or not that matters. It may rather be, for example, whether or not there is a meritocratic bureaucracy, and any type of regime may be capable of sustaining this. This approach is in the spirit of Evans (1995) and cries out for empirical work (Campos and Nugent 1995) have made some recent progress in this direction.)

6.4.3. Inequality

A common theme of many works is that inequality matters. The empirical evidence shows that equality has a significant positive impact on growth. In a famous comparison Lucas (1993) argues that the paradox of compara-
tive growth in South Korea (a miracle) and the Philippines (a disaster) cannot be simply explained by economic fundamentals. Yet one fundamental which he ignores is income distribution. For example, Fields and Jakubsen (1994) report that the Gini coefficient in South Korea was 0.34 in 1965 while in the Philippines in the same year it was 0.47. Could the comparative equality of South Korea be an intrinsic part of its success? Many authors believe so. Rodrik (1994,1996), for example, has argued that the relative equality of land and income distribution in East Asian countries may have played a key role in their ability to adopt good policies and institutions.

I also noted in section 5 that distribution may be important for understanding the extent to which cooperation can overcome intrinsically non-cooperative situations. Moreover, the more equal a society is, the less likely are the benefits of industrialization to be skewed towards one group or away from another, and other things equal, the less likely a blocking coalition can emerge. Campos and Root (1996) argue that it was precisely the equality of East Asian countries which defused potential opponents of growth and allowed most to invest in assets whose value was increased by growth. In fact, recent interpretations of the relationship between inequality and growth have suggested that inequality may affect growth adversely particularly through the channel of increased social conflict (see Bénabou, 1997.) The stress on equality therefore seems justified.

6.4.4. Shocks and Intrinsic Dynamics

Many of the variables which might explain differences are initial conditions. This view brings us back to the models of multiple equilibrium discussed in section 2.1 where differences in initial conditions are central to explaining cross-country growth experiences (this view raises some interesting possibilities, such as the role of colonialism in the subsequent growth of Africa.) Nevertheless, it seems hard to imagine that the path of growth is so immutable once the “die is cast”. The development of Japan following the Meiji restoration, or that of South Korea following the transition between the Rhee and Park regimes in the 1960’s, belies this view.

One way we can reconcile the significance of initial conditions with policy transitions is to recognize that the dynamics of accumulation themselves can induce change and alter the costs and benefits of bad policy (which depend on the values of state variables—one could easily imagine that the opportunity cost of adopting bad policies is increasing with the level of income for example.) This idea is present in Benhabib and Rustichini (1996)
discussed in section 5.5.1. Moreover, shocks (wars, external threats, harvest failures, depressions) have historically been very important sources of policy change (see Gourevitch, 1986; Tornell (1995) and Velasco (1993,1994) provide models where policy choices are inefficient because of non-cooperative influence activities and show how changes in the environment in which this activity takes place can change the nature of the equilibrium.

In the East Asian context the external threat of communism (inducing, for example, peasant uprisings in the 1940’s in South Korea) and invasion could have played a key role in making asset redistribution (land reform) politically feasible and could have been an important spur to growth. Haggard (1990) has stressed the external impetus for growth in these economies. The history of Latin America suggests that, absent these external shocks, land reform is usually not part of the political equilibrium. There are many other examples of shocks inducing development: Russian industrialization after the Crimean war, the Meiji restoration in Japan and Turkish industrialization under Atatürk in the 1920’s (see Trimberger, 1978.) Thus a low-level trap, perhaps induced by the unwillingness of the government to adopt developmental policies which would disturb the political equilibrium, can be broken when the regime is forced to grow or perish (Acemoglu and Robinson 1996a have recently stressed that, thought of in this type of light, political instability can be good for development since it can force beneficial institutional transition.)

The social structure of East Asian countries was also disrupted by the War. In particular the landowning class was destroyed either by previous Japanese colonial occupation, or, in the case of Taiwan, deliberately undermined by government policy. Datta-Chaudhuri (1990, 1981) argues that the vested interests of landowners in the Philippines can explain why the economy did not industrialize and this is a standard explanation for relative stagnation of Latin America (see for example Furtado, 1976.) While this is plausible, we do not understand why the landed elites who controlled the political system in Britain at the start of the 19th century did not attempt to block industrialization (as the Filipino elites are postulated to have done.) The landed aristocracy certainly ended up losing their political power, which in turn led to policy changes which were unfavorable to them (in particular the repeal of the Corn Laws.) One explanation for this may be the process of development occurs much faster for latecomers. The growth rates on East Asian countries have far exceeded any growth rate observed during the first or second industrial revolutions. This is important since the faster the growth rate, the faster the change in the political equilibrium. Hence, elites may oppose rapid change while gradual change leaves them with enough time
to enjoy political power to benefit from development. In the British case, because of its long history of mercantile expansion, there had also developed a strong class of merchants with an interest in wealth creation. By the time the industrial revolution occurred, this class had already severely circumscribed the political power of landed interests.

6.4.5. Social Structure and Cultural Endowments

While economists are used to thinking about factors as endowments, it may also be important to consider social endowments more broadly. There is a long tradition of considering the effects of wider aspects of the culture, social norms and value system of a society on development (Jones 1988 and Mokyr 1990 provide nice discussions) yet so far this has not been integrated into the way we think about development.

In an extraordinary empirical study, Putnam (1993) finds that the efficiency of regional government in Italy is closely related to what (following Loury 1977 and Coleman 1990) he calls “social capital.” By this he means productive “features of social organization, such as trust, norms and networks, that can improve the efficiency of society by facilitating cooperative actions.” Putnam shows that in regions where social capital is high (as measured by participation in civic institutions and voluntary societies) government is efficient, whereas when social capital is low, it is not. The criticism that economists have ignored the fact that the economy is only one aspect of a mutually interrelated social fabric is not a new one (see Granovetter 1985 for a recent statement). Putnam shows empirically that these things may matter quantitatively in significant ways. This seems a very promising area of research. Putnam’s theory is relatively pessimistic. Social capital is determined by initial conditions and determines subsequent political development (in this it is reminiscent of Evans’s (1995) ideas about how the nature of the bureaucracy affects whether or not a state is developmental. He traces the success of East Asian governments in adopting good policy to initial conditions which gave then a strong and meritocratic administration with a developmental outlook.)

It is also interesting to consider the provocative work of Easterly and Levine (1996). They find that the degree of ethnic fragmentation in a society has a negative effect on growth rates and they speculate that this is because of its effects on conflict and redistribution. While this finding is controversial and open to different interpretations, it marks the start of an exciting attempt to assess empirically these factors.
Conclusions

In this paper I have argued that the basic approach of growth theory has failed to provide convincing hypotheses about comparative development. The inability of differences in tastes and technology to provide plausible causal theories has led many economists to stress differences in policies and many historians, following North (1981), to stress the nature of the structure of political and social institutions necessary for development (Harding, 1989, and Lin and Nugent 1995.) However, several authors have realized that one has to go beyond this and view policy and institutions as endogenous. In this paper I have tried not only to evaluate existing research on this topic under the rubric of 'bad policy' but I have also tried to direct attention at what I feel to be the most promising ideas. In my estimation these are very under-researched and the empirical task of linking policy choices to explanatory variables hardly begun.

Let me re-cap on the key theme which emerged. Development is not Pareto improving and those who lose have an incentive to oppose it. If they have political power it might be thought that they could be 'bought off' so that development can continue. However, we rarely seem to observe this happening. Instead development proceeds when those who benefit from it have sufficient political power. Why is it so difficult to generate compensation? One plausible reason seems to be that those with enough political power to extract compensation cannot commit not to keep extracting resources in the future and this may adversely affect investment incentives. This is intertwined with one of the main innovations of the paper, my stress on the fact that development alters the distribution of political power. In this case, agents with political power who do not benefit from development still oppose development because it changes the structure of political power in a way which adversely affects their interests. This represents a fundamental inseparability between efficiency and distribution. This suggests that even the ability to commit may not be sufficient to guarantee good policies (though it may be necessary) and whether or not it is sufficient depends on the time horizon over which political change occurs and how fast growth proceeds.

What one would really like to know are the conditions under which the winning coalition's interests are consonant with the goals of development. This seems a very contingent issue and so far we have few solid generalizations.
Is underdevelopment best thought of in terms of bad policy (interestingly, accepting this idea implies accepting the centrality of the state in facilitating development, something stressed by many historians, for example Polyani (1944) and more recently Easterlin (1996), but typically resisted by economists)? There are many issues of importance which are not obviously captured by this phrase. In an important sense, however, it is: the institutions and policies which seem to be central to promoting development are devised and sustained intentionally by human societies. We can therefore pose the question as to why it is that some societies have managed collectively to adopt good or bad institutions and policies. Thought of in general enough terms, this seems a very fruitful approach. Unfortunately, so far too much of the growth literature has failed to understand the need for such an approach, and those writers who have, have focused on unconvincing models of the political process, or on an overly narrow definition of what bad policy is all about. The literature also needs to connect more with the rich microeconomic literature on development, which has intensively studied the nature of contracts, capital markets, and the incentives to adopt new technology and the literatures in economic history, political science and sociology which have though long and hard (and fruitfully) about the nature of institutional change. In particular, some of the ideas I have suggested above are closely related to the theories of democracy proposed by Moore (1966) and industrialization due to Brenner (1976), both of which stressed the importance of what types of coalitions formed and attained political power in understanding the subsequent evolution of politics and economics.

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