Inequality, Development and Economic Correctness

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Introduction

The relation of income distribution to development has been one of the most prominently contested issues in development economics. This is chiefly because it promises to reveal something about the prospects for equitable or "inclusive" economic development, or alternatively about the ineluctable nature of growth/distribution trade-offs. The literature has been characterised by a succession of swings from one "consensus" to another (Kanbur, 1998). Yet, the dominant concern has been till recently to explore the validity and uniformity of some variant of the inverted-U hypothesis. This hypothesis - stemming from an interpretation of Kuznets's (1955) first historical work – held that inequality would rise inevitably in a country as per capita income levels rose, until after some turning-point the distribution of income would just as inevitably start to improve. Despite significant quibbles in some contributions to the literature, the basic idea was quite widely accepted and was deemed by Robinson (1976), for example, to have acquired the force of economic law.

Argument over the inverted-U relationship ultimately descended, however, to an unfortunate pursuit of uniformity (Kanbur, 1998) and to what one observer calls a form of "scholarly ping-pong" (Bowman, 1997) between models and estimations given varying spins of sampling or specification.[1] Only partly for these reasons, general interest in distribution/development dynamics faded rather during the 1980s.

However, during the 1990s the literature on distribution and development was rekindled, and inequality was brought "in from the cold" (Atkinson, 1997).[2] Recent contributions have shifted the analytical focus away from causal effects of growth upon distribution, reorienting the analysis to the growth implications of variations in the size distribution of income. Much of this literature has supported the proposition that a high

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[1] Kanbur (1998) also points out that much of the literature confuses short-run and long-run dynamics.

[2] Interest in inequality is not restricted to development economists; there has also been a concern with the breaking down of the Kuznets inverted-U relationship apparent in industrialised economies such as the USA and the UK, where inequality has risen sharply in recent years (Gottschalk, 1997; Atkinson, 1997). Another focus has been on international rather than intra-national inequality (see, for example, Pritchett, 1997).

degree of (chiefly income) inequality undermines economic growth. If this proposition has not quite acquired the force of economic law, it has certainly developed the momentum of conventional wisdom among many development economists and institutions. Renewed concern with distributional questions in the mainstream is also consistent with the broad interest in "human development", with an intellectual shift in the World Bank away from the narrower focus of the Washington Consensus, and with the possible replacement of this consensus by the "Post-Washington Consensus" (Stiglitz, 1998) and the "Comprehensive Development Framework" (Wolfensohn, 1999).

Most of this recent literature on whether and how inequality harms growth prospects is driven by innovations in economic analysis, principally endogenous growth theory and the "new political economy". This paper critically reviews this literature. The paper questions the empirical robustness, the methodological foundations, and the policy implications that lie behind the extremely appealing conventional wisdom that inequality is bad for growth. The paper first sets out the models that have been constructed and tested, generally with cross-sectional data and on occasion with panel data sets. These models tend to agree on a basic finding that inequality is inversely related to economic growth rates. Models of this kind fall into two broad groups according to how they construct the main channel by which inequality becomes harmful for economic development. One channel involves the influence of political factors as they affect either political stability, and thence the investment rate, or public choice, incentives to save, and thus the investible surplus (all of these flowing from the behaviour of the "median voter"). The other channel connects inequality to lower growth performance via the damage done by inequality to the expansion of human capital in an economy.

Weaker human capital and politically constrained investment rates may, of course, both activate a link between inequality and economic growth: the two channels are not incompatible. They simply reflect different strands in the major analytical innovations within economics: endogenous growth theory and the new political economy. The discussion below focuses on the influence of these innovations on the new consensus concerning the relationship between inequality and development, highlighting possible weaknesses in the assumptions and logic entailed in their application.

The paper also magnifies the analytical small print in much of this recent literature, in two respects. First, despite the appeal to improved, "quality" data sets, there should be strong scepticism of how robust, empirically, is the current conventional wisdom. Secondly, while the apparent finding of an inverse relationship between inequality and development may appeal to a broad range of political or ideological positions, it should be pointed out that the assumptions that feed into the models that generate this finding, and the policy implications that attend it, are firmly neo-classical. However, after reviewing the literature, this paper concludes that the current conventional wisdom does not represent an advance in the analytical core or methodology of the political economy of development. The literature is characterised by obsession with uncovering (probabilistic) event regularity-type “economic laws”, an economistic notion of politics, and an ahistorical functionalism of “connected” variables that replaces social processes and relations with impersonalised structural categories: these features of the literature combine to undermine the value of such work. There are separate, long-standing and individually complex literatures on distribution, on human capital, on democracy and/or political stability, and of course on economic growth. Why is it to be believed that these issues can be mutually connected at much simpler levels of analysis? This paper argues that they cannot be linked through regression analysis and stripped down versions of political analysis, growth theory, and distributional research. Given that the separate literatures have established the heterogeneous causes and effects of economic growth and of inequality, it is likely that outcomes will be still more heterogeneous, rather than standard and predictable, when inequality and growth are combined. This is all the more so when one takes account of diversity in historical and social context. As a result, it is argued that research should concentrate more on the particular, on the sources of heterogeneity in distribution and development relationships, and also on different forms of inequality than those captured by the personal or household size distribution of income.

How might inequality be harmful for growth?

Recent literature on the relationship between inequality and economic growth stresses two main mechanisms by which a higher degree of inequality might damage development. One is the influence exerted by inequality on political behaviour and decisions that, in turn, affect prospects for economic growth. The other is the influence of inequality on human capital formation and, again, the effects of this on growth. Although some contributions emphasise one channel more than the other, there is nothing necessarily exclusive about either mechanism. In fact, in some models the two interact quite explicitly.[3]

Models that endogenise political behaviour fall into two types. Firstly, there are those that draw on a political theory to explain how inequality logically feeds into public choice decisions that are anti-growth. Secondly, some models draw less on theories of political equilibrium and instead focus on political instability or outright conflict as the link from inequality to economic under-performance. Within each type there are variations in the specification of models but across both types of model there are certain underlying themes and methods in common.

One assumption, then, is that greater inequality brings about an adjustment to a new “politico-economic equilibrium” (e.g. Persson and Tabellini, 1994, 600), with implications for the growth path of the economy. This idea is presented especially clearly, for example, in Alesina and Rodrik (1994). There, higher inequality leads logically to political pressure on the government to raise the rate of taxation on capital. But since capital accumulation is the key to growth, and a higher capital tax disturbs incentives to accumulate capital, this will necessarily mean that the economy grows at a less than optimal rate.[4] (This argument seems ill-fitted to the taxation structures of many developing countries, and indeed to the common observation that, for example, in some Latin American countries with relatively high Gini coefficients tax evasion is pervasive.[5]) Individuals differ in their endowments of capital and labour. Growth is principally determined by individual decisions to save, generating an investible surplus. These individual decisions are likely to be maximised, in the aggregate, where there is a relatively even distribution of wealth such that many people own at least some capital. This is because owners of capital are likely to vote for taxation policies that accentuate the incentives to save. If capital is distributed very unevenly, then government will be unable to resist the pressure to raise capital taxation to levels that may facilitate redistribution (basically to unskilled labour) but that will definitely act as a deterrent to save among capital-rich people and therefore as a break on potential growth. It is suggested, with some empirical support, that inequality of income and wealth go hand in hand.

In this kind of model there is room for some subtlety concerning taxation and its effects on economic development. Thus, Alesina and Rodrik (1994) distinguish their own model from others on this account. In this work, there is an inverted U-shaped curve relating tax to growth at different levels of per capita income, consistent with Barro (1991). At low income levels, taxation is a public good. Its benefits accrue to everyone and it is growth promoting. But beyond a certain level (adding to the tradition of rather elusive turning-points in this literature), capital taxation distorts the economy. In other models, this is either not specified or, as in Persson and Tabellini (1994), taxation is purely redistributive and distortionary.

At the core of this kind of model is the political theory of the “median voter” or, even more tellingly, the median individual. Public choice or government policy is determined, in this theory, by the preferences of the median individual and his/her voting patterns. Public policy reflects adjustments to the apparently rational and knowing individual preferences aggregated in the expressions of this character, allowing the society to find political equilibrium. This notion of political equilibrium may be subjected to comparative statics or dynamic analysis, though it is acknowledged in certain cases that a genuinely dynamic

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[3] Birdsall, Ross and Sabot (1994) do acknowledge other mechanisms by which inequality might harm development, such as the effect on aggregate demand and undermining x-efficiency of the poor.
[4] This argument is well-rooted in the endogenous growth theory argument that a higher savings rate raises the warranted growth rate and productivity.
[5] C.f. Pio (1994).

analysis of the relationship between median individual voting, inequality, capital taxation and growth is close to impossible (Alesina and Rodrik, 1994). The preferences of the median voter are naturally influenced by the distribution of major economic properties likely to determine voting, assuming voting behaviour to be straightforwardly a function of differences in such properties or attributes. Bertola (1991), for example, distinguishes between accumulated and non-accumulated factors of production, and dramatises a model in which people with different income sources come into conflict over the chosen growth rate. Thus, where wealth or income is concentrated in the hands of a minority, the median voter will want, so the argument goes, the government to impose higher rates of taxation on that minority. While the intent behind this kind of voting pattern is to secure redistribution, these models assert that the resulting taxation will reduce the investible surplus and thereby curtail growth prospects.

However, it may be that politico-economic equilibrium is disturbed by political instability arising from the uneven distribution of income (and/or wealth). An economic link, in this case, might arise not only from blunted savings incentives but also from a deterrent to investment (assuming prior saving). This argument is contained in models constructed and tested by Alesina and Perotti (1993; 1994). In this kind of model inequality causes instability, which is picked up in regressions correlating measures of inequality such as the Gini coefficient with indices of political instability. Instability may be measured by indices of unrest, summarising indicators of protest or violence, such as the number of political assassinations. Or instability may be measured by the rate of turnover of the executive.

A brief review of earlier attempts to capture instability-growth relationships, via a single socio-political index, shows that these efforts have been rather inconclusive. An alternative might be to estimate the probability of government collapse, using riots and protests as well as economic indicators such as past growth record and inflation. Although it is acknowledged that difficulties of joint endogeneity of political instability and growth, simultaneity and reverse causality reduce the power of such models, there is little eagerness to conclude that one should abandon probabilistic cross-section models.

A number of models have sought to generate predictive findings about the relationship between instability and growth: the results have tended to vary with different specifications of the models. Alesina and Perotti (1994) link inequality to instability and then to growth. It is argued, from empirical testing of the model, that an increase by one in the standard deviation of the middle class share of total income causes a decrease in the socio-political instability index of about 3.3, and that this then causes an increase in the share of investment in GDP of about one percentage point. Note that despite some introductory hedging about the complications of causality in the model, the findings are nevertheless presented in clear-cut causal terms.

Another version of the inequality-political instability argument concerns outright conflict or civil war. Interest in the economic causes and effects of conflict in developing countries during, especially, the 1990s, has increased. Such conflicts have flourished in the aftermath of the Cold War. Indeed some conflicts that were previously regarded as analytically off-limits (since they were assumed simply to be "proxy" wars fought on distant contested terrain by competing superpowers and their regional allies) have come to be seen more from internal perspectives. Economic models and varieties of political economy analysis of these conflicts have proliferated (Nafziger, 1996; Collier and Hoeffler, 1996; Atkins, 1996). A number of these models isolate inequality as a critical factor, or determining variable, in the origin of conflicts. Binswanger et al (1995) argues that there is a necessary "social cost of delayed [land] reform" (p. 2690), an argument based on a generalisation that the social costs of failing to correct for unequal land distribution have often included peasant uprising and civil war.[6]

In a statistical analysis of "complex humanitarian emergencies", Nafziger (1996) also asserts that inequality is a likely cause of conflict. One specific application of the recent literature, invoking Alesina and Perotti (1994) and Rodrik (1994), is that of Boyce (1995) on El Salvador. One problem here is that inequality assumes universal characteristics: discussion of inequality in El Salvador, by drawing on a generalised notion of the standard causal effects of inequality, in fact dilutes the specificities of the Salvadoran case, replacing historical process, structure and relationships by the categorical effects of a functional connection between inequality and development or instability. There is a separate assumption in most of this literature that civil war necessarily damages development prospects, drawing on the work on the economic costs of conflict of Stewart (1993), among others.[7]

Intriguingly, one economic "explanation" of civil war (Collier and Hoeffler, 1996) develops a model in which greater inequality is likely to <u>reduce</u> the propensity for conflict. The argument in this model is that a higher degree of inequality indicates a wealthy elite whose status depends on government protection: hence, this elite will be willing to accept higher government taxation for military expenditure purposes in the event of a rebellion. The implication is that potential rebels will build this into their cost-benefit calculations and see that any rebellion would face a high-spending military reaction. Given that this reduces the chance of victory and hence diminishes likely utility from an uprising, therefore rebellion is less likely. This model allows us to stress two points. Firstly, a whole range of results,

[6] This claim is backed up by a number of extremely cursory case-studies in which there is no exploration of the complex history of these conflicts. Another example of the assertion that inequality in land holdings causes conflicts is in Myers and West (1996), who also invoke the threat of rural disturbances as a probable consequence of unequal distribution of landholdings in Mozambique, though without any hard evidence.

findings, and arguments has found some support in available empirical data for cross-sectional analysis. Secondly, starting with the basic axioms of methodological individualism and utility maximisation, one may invent a variety of sometimes quite contradictory models of the chain of social and economic causation.[8]

Distribution may also affect economic growth by its impact on human capital formation. This may be relatively direct, as in Chiu (1998); or the effect may operate indirectly, via political mechanisms such as those elaborated above. In either case, the basic causal link is the same. Drawing on Barro-type growth models and invoking Romer (e.g., 1986) and others stressing the endogenous significance of human capital, an argument is then sought for how inequality stunts human capital formation. The indirect argument is simply a variant of the politico-economic equilibrium models discussed above, with greater emphasis in the investment function on investment in human capital as opposed to physical or financial capital accumulation.

The more direct arguments develop the micro-economic logic by which the distributional structure of an economy affects human capital formation. Meade (1964) long ago stressed the reproductive persistence of inequity, the "self-reinforcing influences which help to sustain the good fortune of the fortunate and the bad fortune of the unfortunate" (quoted in Atkinson, 1997). More recently, various economists have taken this idea and formalised the inefficiency caused by inequity (Loury, 1981; Galor and Zeira, 1993; Chiu, 1998). Discussing capital as knowledge, Persson and Tabellini (1994 , 602), state that the owners of capital earn monopoly rents from previous investments in knowledge. For Chiu (1998), rich families, with diminishing marginal utility, find it cheap to send even their untalented offspring to school and higher education colleges; poorer families can only countenance investing in their children's education if the case is economically overwhelming, based on the identification of superior talent. Here, talent is identifiable *a priori* and is distributed randomly, i.e. its distribution allows for stochastic modelling.

Therefore, initial income distribution is essential in determining economic performance over time. With inequality, the rich get advantages, e.g. in the labour market, even over those with greater innate ability, since the latter have been excluded from human capital formation on account of their parents' poverty. Birdsall, Ross and Sabot (1995, 491-493), develop a model in which causality runs both ways – positively in each case – between basic-education enrolment rates and lower levels of income inequality. In this model, low inequality of income introduces positive feedback into a virtuous circle initiated by education

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[7] For alternatives concentrating on the scope for dynamic short- and longer-run effects of conflict, see Collier et al (1994), and Cramer (1998, 1999).
[8] It is even more intriguing that a later version of Collier's model of the utility function for rebellion adjusts the rationale to argue that inequality <u>is</u> associated with conflict, and, again, finds empirical confirmation for this argument.

policies.[9] Low inequality in some countries increases household demand for education and probably increases the supply (492), compared to other countries and presuming similar per capita income levels.[10] On the demand side, budgetary constraints, poor access to capital markets, and basic survival imperatives prevent the poor from investing in education. On the supply side, inequality means that for governments to subsidise generalised basic education there must be a heavy tax burden on the rich, who will try to resist, for example, by urging the government to channel education spending to the higher education sector. (One may note, in passing, that the median voter is not presumed to get his/her way in this story.)

Is there madness in their method?

The arguments reviewed above present a morally appealing case, certainly at the level of their basic finding that inequality is harmful for growth. What, though, is the significance of this finding? And how robust is it, at the empirical, theoretical, and methodological levels? A critical focus on some of the empirical and methodological themes raised in this literature may reveal that the finding cannot be considered as a reliable "economic law" or generalisation across all countries and development predicaments. Once the theory and method underpinning the finding of an inverse relationship between inequality and growth, at the cross-country level, are subjected to closer examination, the ideological implications of the recent literature emerge more clearly.

Empirically, the inverse relationship between inequality and growth is not convincing as a generalisation. Briefly, there are three types of problem with the empirical work: unreliability of the data, doubts about the statistical significance of regressions, and lack of strong enough grounds for conflating statistical association with causation. Firstly, there are reasons to be sceptical about the accuracy of the data employed in this literature. Many of the contributions to the literature use different data sets, so that it is not easy to see where exactly it is the specifications of each model that yield variations in findings and significance and where the variations flow from application to different sets of observations. Moreover, much of this literature smoothes over the lesson learnt many times over in the traditional debate stimulated by Kuznets (1955), i.e. that inequality data are notoriously untrustworthy and

[9] Higher education enrolment rates introduce wage differential compression, erasing the scarcity of knowledge and overcoming the contrary compositional tendency commonly assumed since Kuznets (1955) whereby labour shifting into higher productivity sectors necessarily generates inequality (Birdsall, Ross and Sabot, 1995, 493).

[10] A particular comparison is made between Brazil and Malaysia during the 1980s, when Brazil's per capita income was slightly above that in Malaysia but the per capita income of the bottom quintile in Brazil was only 54 per cent of the per capita income of the equivalent quintile in Malaysia. "Given an income elasticity of demand for basic education of 0.50 (a conservative figure), if the distribution of income were as equal in Brazil as in Malaysia, enrolments among poor Brazilian children would be more than 40 percent higher" (493).

frequently incompatible (Moll, 1992).[11] Even the "improved" data sets used in some recent contributions (e.g. UNCTAD, 1997) rest on a range of individual country surveys that do not necessarily stand up to close inspection, especially at the level of comparability.[12] Much of the interest in the possibility of combining, functionally or otherwise, low inequality with rapid growth derives from the experience of the East Asian NICs. But belief in the historical sequencing of the inequality and growth relationship and in the continuing role of low inequality in growth that stayed at high rates until the late 1990s has increasingly been undermined (Moll, 1992; Seguino, 1997).[13]

Some contributions confront directly the uncertain significance of inferences drawn from their econometric testing of large data sets, not so much on account of the underlying poor quality of the data but because of the fragility generated within their models. Birdsall, Ross and Sabot (1995, 496), for example, state that in their inequality-growth model inequality is negative and significant at the 10 per cent level, but that adding in a regional dummy variable cancels the significance of the inequality variable; therefore, as they note, their results are at best suggestive.[14] Persson and Tabellini (1994, 610), directly undermine their own finding while still claiming its importance. They acknowledge that their model – showing inequality to be negatively correlated with growth in post-World War Two economies – may have omitted a variable that was negatively correlated with inequality and that was growth-promoting, such as technological innovation spurred by the experience of war economies. Re-estimating the model to try to avoid this possibility, they find that the fit is much worse. The coefficients on their model's inequality and political enfranchisement variables lose significance. Persson and Tabellini (ibid., 615) also point out that their regressions have estimated reduced form relationships but have not directly addressed the two specific channels identified in the political theory they have drawn on (median voter behaviour aggregating the preferences of isolated maximising individuals): from more equality to less policy-induced redistribution, and from less redistribution to more investment

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[11] See also Paukert et al (1981) for measurement problems in inequality research; and Leontief's comment that: "Incompatible data are useless data" (1971, 6).

[12] Throughout the distribution/development debate, there have occasionally been cautious notes sounded about the need for strict criteria of data reliability and comparability, from Kuznets (1955), to Fields (1989), Deininger and Squire (1996), and Anand and Kanbur (1993).

[13] An interesting recent argument is that rapid growth, especially its manufactured export component, has relied heavily on continuing inequality through labour market institutions that keep female textile workers' wages, for example, artificially low (Seguino, 1997).

[14] Earlier, these authors point out that multicollinearity means that two key variables in their education/growth model are not significant at the 5 per cent level; but they claim that data testing rejects the null hypothesis that the coefficients of the variables are zero. From here, Birdsall, Ross and Sabot assert that, nonetheless, the results "support the contention that the contribution of education to economic growth tends to be greater in countries in which manufactured exports are a higher proportion of GDP" (1995, 489). This is a good example of Mayer's (1993, 135-7) criticisms of the misuse or stretching of significance tests to smother ambiguity with claims of economic significance or inference.

and faster growth. Finally, they accept that the results are "very tentative" and that the hypothesis they are testing requires "better data and a larger sample" (617).

Two further arguments undermine the causal claims of much of the literature. This literature is characterised by a plethora of model specifications. For example, models setting out arguments for how higher inequality might reduce human capital formation differ in whether they assume that individuals know their ability when they make human capital investment decisions (Loury, 1981) or not (Chiu, 1998); whether they assume that individuals have identical innate ability (Galor and Zeira, 1993) or that the distribution of innate ability is uneven and random; or whether or not there is an underlying assumption of the viability and influence of a median voter. Combined with data for cross-country comparisons that do not bear close inspection, with variations in outcome according to changes in individual model specification, and with the tentative hedges around many of the conclusions, this multiplicity of model rationales for an empirical outcome that is positively willed on the available evidence ultimately does not invite much trust in the conclusions reached. Much of the literature falls into a common trap, moreover, of conflating statistical (probabilistic) association with actual causality. At best, the contributions acknowledge the limitations on causal statements (Birdsall, Ross and Sabot, 1995). Elsewhere there is a more defiant statement of causality. One of the several technical and theoretical difficulties involved is the insistence on linear relationships imposed by the style of modelling and data analysis, when there is evidence to suggest that real, underlying relationships between inequality and development outcomes not only vary but are often episodic rather than linear (Bowman, 1997; Atkinson, 1997).

The roots of these empirical problems lie not just in technical details and the constant search for improved data and/or more observations, but in the methods adopted in the literature and the theory implied in those methods. On the whole, the method is a matter of lining up a series of variables and subjecting them to cross-country regressions (sometimes of as many as 90 countries). Alternatively the data are subjected not to statistical mapping but to logical/mathematical "proof" (e.g. Chiu, 1998). Two factors sustain the idea of causality sought in this work. One is a belief that economics is best regarded as the quest to pin down constant conjunctions of events, event regularities of a probabilistic form such that whenever one event occurs, then under a certain set of conditions another predicted event shall take place.[15] According to this belief, the scientific method of economics seeks to establish that, for example, as inequality rises, so - in a democracy (perhaps) where capital markets are imperfect, innate abilities are randomly distributed (perhaps), policy is redistributive (again, perhaps not in another specification) and is formed chiefly by response to the preferences

[15] See Dow (1997) and Lawson (1997) for a discussion of the positivism of mainstream methodology.

aggregated in the behaviour of the median voter - then there is a significant chance that potential growth will be curbed. The second factor underlying causality claims is a form of analytical *trompe l'oeil* by which the focus switches between micro-economic abstract theory and grand aggregates of cross-section data. The actual links between the micro-theory and the macro-economic and macro-social statistics introduced to confirm the conclusions deduced from it are rarely made explicit.[16] This is chiefly because such analyses hardly ever discuss real social and economic processes. Further, this method joins deductive micro theories of human action reduced to utility maximisation, with an almost arbitrary range of limiting attributes (inter-generational altruism, identical distribution of innate talent, random distribution of such ability, and so on) with over-aggregated data in discrete categories of variables that are not always robustly defined, even, at the quantifiable level.[17]

The literature that posits an inverse relationship between inequality and growth represents a highly sophisticated attempt to cope with the challenge posed by the predictive shortcomings of previous mainstream methodology (dominated by econometrics).[18] Responses to predictive weakness in econometric modelling may branch in two directions. One is to endogenise more and more variables until no potentially influential variable has been omitted. The other is to dig deeper for a logical explanatory rationale for the phenomena one is trying to understand. A further response is to acknowledge that populations are heterogeneous and explicitly to model variations in attributes.[19]

Much of the recent literature on inequality and development involves two or more of these responses. First, contributions such as Alesina and Perotti (1994) explicitly stake their claim on working at the interface of new political economy and endogenous (or "new") growth theory. In other words they are hoping to reach firmer empirical conclusions by means of including a substantially greater range of independent and endogenously related variables in their model than has been typical of the traditional literature. The same is true of models combining endogenous growth theories derived from Barro and Romer, among others, with models of political equilibrium typically based on the median voter theory (e.g. Persson and Tabellini, 1994). Second, the plethora of specifications mentioned above is evidence of a search for intrinsic closure, for a tighter logic in the micro-foundations of the argument.

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[16] On this as a general feature of endogenous growth theory, see Fine (1998a).
[17] For example, political instability indices where, unlike, say, prices or volumes of goods and services, the phenomenon is not by nature defined in scale terms. There are many other examples in related literature, for example the attempts to argue or presume a functional relationship between a "free press" and development (see World Bank, 1997, page 108), or the difficulty in defining corruption in quantifiable and comparable terms (see Khan, 1995).
[18] In this the literature is one example of a range of analytical exercises branching out from the insights of endogenous growth theory and other innovations such as the asymmetric information paradigm that have prompted a growing confidence in the micro-economic foundations for virtually any social phenomenon (Fine, 1998a and 1999).
[19] For a formal critique of the methodology of econometrics, see Lawson (1997).

Differences among the models reflect attempts to refine the explanatory rationale driving the behaviour of the individuals observed in their aggregation (either their political behaviour or their behaviour in deciding whether or not to invest in human capital accumulation). Hence, the literature engages in debates over whether an inter-generational two-period model should presume identical allocations of innate ability or stochastic properties of ability among the population (see, for example, Chiu, 1998). Thirdly, of course, the whole purpose of this literature is explicitly to put the influence of heterogeneity, in the form of the size distribution of income, at the forefront of analysis.

There can be few other examples in economics of the attempt so completely to respond to the difficulties previously encountered in econometrics and in mainstream economics more generally. (However, this is never stated explicitly in the literature as a response to failure, but rather is presented as vanguard analysis.) However, the results are indeterminate. This is a signal that uncertainty and contingency in political, social and economic life simply cannot be subjugated to econometric practice, however sophisticated and elegant. The reason is that the project of econometrics is the pursuit of closure, while the reality of lived political economy is social openness. As Dow (1997, 87) suggests: "Logical certainty is limited because in open systems the full range of relevant variables is not known: empirical certainty is limited because in open systems evolutionary processes and discontinuities limit the incidence of replicable events". This openness of social relations does not preclude trends, though their repetition across divergent contexts cannot be relied upon. There are tendencies, and these tendencies presumably derive from structural relationships. But neither the actual outcome, nor the intensity and significance of the outcome, can be predicted, for two particular reasons. One is that there are multiple and infinitely variable combinations of such tendencies and their individual causal strengths: observable outcomes are likely to depend on specific combinations of structural tendencies, facts, relations. The other is that outcomes cannot be read off from empirical or structural "laws" because of the influence of agency; and this human agency is not reducible to the dull creature of methodological individualism, a supposedly always choosing character that in fact is only a passive reflector of external signals, and whose utility maximisation may be refined by more complex elaboration of attributes but who remains a kind of isolated automaton of society.[20]

A further attraction of this work is its explicit focus on distribution, not just as a complication but as a central feature of economies. However, distribution is admitted only on

[20] The argument here draws especially on Bhaskar (1989), Lawson (1997), and Bourdieu and Wacquant (1992). Here it is interesting that Atkinson (1997) suggests, specifically with regard to relationships between inequality and economic growth, that economists have much to learn from other social sciences and from historians. However, the tendency has, arguably, been for economists to draw in political science and sociology but chiefly by taming them with its paradigm of methodological individualism and functional relations (Fine, 1998a, b).

terms of functionality. Given a certain distribution structure of attributes such as land, wealth, income, individuals will – the literature expects – behave consistently over time with respect to human capital investment, political activity, choices made by the median voter.

However, distribution may alternatively be thought to have variable political and economic significance in different social and political contexts. Inequality is not simply a descriptive concept with a significance strictly proportional to its empirical intensity, but is a subjective concept perceived in multiple ways. Further, the reduced individual of the mainstream methodology involves an "assumption of antecedent uniformity" (Sen, 1992, xi); that in turn ignores the fact that human diversity "is no secondary complication…it is a fundamental aspect of our interest in equality" (ibid.). The argument in this paper rests on the premise that socio-economic outcomes spring from social relations rather than the aggregation of individual reflexes. These relations are complex, they are influenced by history, by the form that the development of capitalism takes, and by the exercise of and contest for power. Cramer (1998), for example, shows how hard it has been to isolate a functional relationship between inequality and conflict and that where inequality has been a significant feature of conflict this is inseparable from specific social and historical relations as well as particular economic policies.[21] Underlying the multiple possible social effects of inequality, Hirschman (1981) suggests the idea of a "tunnel effect". An individual or group observing sharp inequality might regard this as a signal of possible future income flows to themselves. In this case people might tolerate inequality. There is a suspension of envy. There may also be institutional mechanisms at local or other levels that are designed to mitigate the divisive effects of inequality opening up. Hirschman cites micro-level research looking for an envy response to national growth and finding – e.g. in Brazilian *favelas* – the opposite, i.e. this tunnel effect. Here, inequality would be tolerated so long as the tunnel effect (certainly impossible to quantify) does hold.[22]

What is missing?

Romer (1993) argues that the critical obstacle to economic development is not an *object* gap (such as physical capital), but an *idea* gap (i.e. technology). Much recent economic theory has focused precisely on the generation of innovation, most typically in a variety of

[21] Nairn (1998) develops an image of the complex conditions required for social catastrophes such as the genocides of Nazi Germany, Yugoslavia and Rwanda to come about, in the same way that thermo-nuclear fission depends on a highly sensitive set of conditions. Despite the mechanistic implications of such an image, Nairn does also highlight the unpredictability of these outcomes and their dependence on specific social relations. See also Cramer and Weeks (2000) for a discussion of the relationship between structural adjustment and conflict.
[22] An opposite idea is that low inequality, by signalling scope for social mobility, might stimulate the x-efficiency of workers (Birdsall, Ross and Sabot, 1995).

endogenous growth models.[23] Nonetheless, beyond objects and ideas there may be a further obstacle to development that is an *ideology* gap (reflected in politics). If there is, indeed, an ideology gap or tension in late late industrialising countries, there is also an ideology gap in mainstream economic literature (representing both a lack of awareness of the role of ideology and politics and a strong but implicit ideological presence).

The median voter theory, for example, presumes a liberal state responding to the directives of a separate civil society;[24] and it is a political corollary of the aggregation of isolated individuals employed in economic theory. The critical fact is that neither this nor the other methodological tools used in the recent mainstream literature on inequality and development capture the significance of social and political relations or the influence of power, making these relations uneven and frequently exploitative. Hirschman's notion of expectational calculus (1981, 47) may imply that the "tunnel effect" overwhelms the behaviour expected of the median voter; but what is important is that this expectational calculus is formed socially, through specific relations and ideology. A good example might be the way in which tolerance of sharply rising inequality in Britain and the USA during the 1980s was influenced by the populist rhetoric of the Reagan and Thatcher administrations, whose emphasis on individual incentives amplified a Hirschman-like tunnel effect and deflected attention from the shift in the balance of social relations.[25]

Persson and Tabellini (1994) stress the role of institutions in mediating the link between income distribution and public policy (and hence, via fiscal policy, investment and the growth rate of the economy). Again, institutions are significant but this approach makes them devoid of historical content and abstracts them from the specific balance of class forces and political groups that will profoundly influence institutional performance and economic outcomes in a particular society.[26] Gramsci's conception of state/society relations makes more sense, where "the State is the entire complex of practical and theoretical activities with which the ruling class not only justifies and maintains its dominance, but manages to win the active consent of those over whom it rules…" (Gramsci, 1986, 244). This contrasts with the

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[23] This interest is not confined, obviously, to endogenous growth theory. One recent contribution is Amsden (1997), arguing for the historical experience in successfully industrialising countries of states rigging prices and working with firms to construct market imperfections that have enabled firms to build up knowledge-based "competitive assets".

[24] Even where there is clearly not a liberal state, some of this literature still has high hopes of the propensity of the median voter to make its aggregated voice count (see World Bank, 1997, 108, for example).

[25] Atkinson (1997) also criticises the over-reliance of recent inequality-growth literature on the median voter theory, on grounds of this theory not being at all standard and because it ignores ideology, interest group pressure, bureaucratic control, etc. The freedom of the press is typically assumed to go hand in hand with the exercise of the median voter's will; but of course press "freedom" may be tied closely to property rights rather than the range of opinion. In this scenario, socio-political consensus is "manufactured" (Wood, 1995; Herman and Chomsky, 1994).

[26] On the relationship between the "political settlement" and the performance of institutions, see Khan (1995).

implicitly populist expectations of spontaneous uprising against inequality in, for example, Atkins (1996), Binswanger et al (1995), etc.

Brief reference to South Africa allows an illustration of some of these themes. In South Africa during apartheid, extreme inequality of wealth and income did not lead to redistributive taxation that curbed personal savings incentives; this of course was because of the particular institution of apartheid. Nonetheless, investment in physical and human capital was remarkably low over a long period. Although this was related to some white South Africans' fears of political instability (or simply political change), it was also encouraged by a state that allowed capital leakage via transfer-pricing and that implemented highly restrictive, monetarist macroeconomic policies from the early 1980s onwards. These features of policy were, in turn, related to the political settlement in South Africa and its contradictions. It is also interesting that since the removal of apartheid legislation and an ostensible shift in the political settlement, the balance of class forces in South Africa has changed little. Further, there is no evidence whatsoever of the clamour of the median voter for radical redistribution measures that might have been predicted from a national Gini coefficient of some 0.68 (see, for example, Marais, 1998). In the same vein, Pio (1994, 296) points out that in recently reborn Latin American democracies neither income nor capital taxation are major sources of revenue and that recent tax reforms in Argentina and Mexico have involved introducing regressive value added taxes. "What seems to be happening, therefore, is that the link between the interest (real or perceived) of the median voter and the policies implemented by his [sic] elected representatives are at best weak" (ibid.). What is missing from the median voter approach, of course, is the role of economic power.

While inequality is a critical issue, the recent interest in its relationship to development has "cleaned" class and other social relations, and exploitation, out of the analysis of unequal societies (Wood, 1995). The methodological retreat from social relations and historical processes, towards the interplay of quantified variables, suggests that the literature itself is ideological.[27] To borrow phrasing used in some of the literature reviewed here, the mainstream literature on inequality and development at least has the virtue of being consistent with an ideological stance characterised by neo-liberal policies, including unfettered integration into the world economy, combined with the institutions of a supposedly liberal democracy.[28] From this perspective, East Asian economic success is boiled down to a simple formula: export-orientation based on comparative advantage, combined with

[27] A related argument is that of Keen (1994) that a focus on the category of the "landless", i.e. in terms of an impersonal attribute or lack of it, needs to be replaced by focus on the "dispossessed", capturing social relations and processes of exploitation that are crucial to the incidence of famine in Africa.
[28] Another example of mainstream methodology in the service of a shifting focus and "finding" that suited the political/ideological interests of the time in, especially, the USA is the history of the military expenditure-economic growth relationship (Cramer, 1994).

equalising human capital investments (Birdsall, Ross and Sabot, 1995). The history and extent of state intervention in the economies of countries such as South Korea is ironed out of this analysis.[29] Much of the literature is fairly explicit with respect to policy conclusions: development can be achieved by combining neo-liberal economic policy with some state expenditure on education and health to raise the productivity of the poor. The critical implication, an explicit one, is that as little as possible should be done to disturb the private incentives to save on behalf of capitalists and, while greater equality is thought to be a good thing, on no account must this be achieved by over-hasty resource transfers that would distort market signals (Alesina and Rodrik, 1994; Bardhan, 1996).

This position would seem to reinforce recent shifts within the World Bank, for example, that advertise a greater acknowledgement of the role of the state in development and a greater interest in market failure, social and environmental issues, and participation, but that are dominated by neo-classical assumptions and methodological individualism - adjusted only for the implications of asymmetric information (Stiglitz, 1998).[30] This current ideology of development might be called an ideology of "economic correctness". An alternative argument might be that capitalist development, and especially the development of capitalism, is not possible without acute tensions and contradictions, that these reflect social, including class, relations that are often (in the case of class necessarily) exploitative: indeed, that instability is a fundamental feature of such development (Warren, 1980; Wood, 1995; Vogel, 1996). It may be argued that instability is not necessarily damaging to the longer run prospects for capitalist development. Instability may, for example, involve confrontations that disturb entrenched classes and interests impeding further development (Bowman, 1997; Cramer, 1998).[31]

Further, the literature reviewed above tends to assume that quantifiable income or asset inequality is the only form of inequality that is socially significant to economic growth and development. This denies the relevance of Sen's (1992) question: inequality of what? Even income inequality will, as has been suggested, have varying implications for political and economic development in different societies characterised by specific political settlements and combinations of social, economic and political underlying tendencies. Then again, as Hirschman (1995) argues, new forms of inequality are bound to arise constantly

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[29] In the World Bank's (1993) East Asian Miracle report, the state is explained away on grounds that the state achieved little that the market would not have achieved if left more to itself.

[30] The shift in the Bank's thinking has been led by Joseph Stiglitz's attempt to usher in a "post-Washington consensus" in which some of the monetarist fundamentalism of the high period of structural adjustment is toned down and there is greater attention to correcting for market failures while stressing competition as opposed to ownership in productive structures (Stiglitz, 1998).

[31] Instability may in the short-run provide benefits to dominant classes. Continued low-level warfare in northern Uganda, for example, may illustrate a "paradox of instability" in that it enables the Kampala-based government to keep the military entertained and to offer them economic opportunities in the north, thus actually helping to preserve a regime that has, at least, presided over higher growth than the country had previously enjoyed for decades.

during a society's continued development: some form of inequality appears to be inescapable, and this may even prove a strength to development rather than a constant brake.

A fantasy of development by addition of desirables – free trade, liberal democracy, efficient markets and modest states, participation, and equality – is likely to underplay the need to understand real social and economic contests and tensions. Such understanding is a prerequisite for the resolution of such tensions and the development of institutional mechanisms enabling the management of conflicts. In other words, high inequality may be a repugnant feature of a given country, and the need for greater investment in basic education and health services pressing; but the idea that this will be enough to generate prolonged economic growth and political stability is a diversion. Here, the statistical methodology of mainstream development economics, by over-aggregation and the quest for the closest fit, itself mimics an ideal of harmonious development models transferable across multiple contexts.

Conclusion

The argument that inequality is harmful to economic development is morally appealing. There are convincing rationales and plausible scenarios in which, indeed, high inequality may well inhibit growth. Even in such situations, though, inequality would not somehow act alone but would require or would more likely reflect particular social relations.[32] However, the manner in which a statistical finding and the suggestion of an economic law has been advanced is misleading. The mainstream econometric literature generating this finding is excessively deductivist. It is unduly reductionist in its insistence on micro-foundations. It fails to explore how the shift from micro to macro takes place, other than through the aggregated choice of the median voter, him/herself a naïve creation. The data do not convincingly support the desired finding, even on the admission of those claiming the finding. The policy implications of the models may very well themselves be "harmful for growth" in LDCs.[33] For these reasons, the finding is theoretically, empirically, and ideologically questionable.

Much of the recent literature arguing that inequality is harmful for growth highlights another current theme of development economics. For the literature proclaims its dependence on "new" theories such as new growth theory or new political economy. However, this

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[32] And indeed relations between relations (see Bhaskar, 1989): for example, between historically formed identity relations and class relations.

[33] In fact, the models clearly give centre stage, in pushing economic growth, to investment and the private savings incentives that are alleged to propel investment; and some, such as Alesina and Rodrik (1994) pay lip-service to the constructive role of public expenditure at very low income levels; but the literature is narrow in its appreciation of the determinants of investment, and generally ignores the increasingly acknowledged (potential) complementarity of public and private investment (UNCTAD, 1998). Further, as Pio (1994, 296) points out, these models do not address the role of trade effects on growth, for example, via specialisation, in line with perceived comparative advantage, in exports of goods with low human capital inputs perpetuating slow growth.

discussion has shown how the literature and its uncertain findings do little to add greater certainty than previous exercises in cross-country econometric efforts to establish law-like constant conjunctions of events.[34] Further, the theories underlying these exercises are, of course, not especially new even if their configuration is novel.

Contrary to the persistent quest for uniform relationships between inequality and development throughout the past fifty or so years of development economics, there is no uniform relation at the aggregate level. There are multiple possible relations, including those produced by the tendencies identified by Kuznets (1955), and Lewis (1983) amongst others and indeed the tendencies partially identified by the recent literature examined in this paper. These sets of tendencies or possible trajectories are not, however, exhaustive. The most important influences on real outcomes in the dynamic relationships between distribution and development will lie in specific historical, political and economic structures, contingencies and events. Above all, these influences lie in the social relations that generate distributional patterns and development processes.

In common with other critiques of the inequality and development literature, one implication of the arguments in this paper is that research into distribution must focus far more on longitudinal single country experiences (Moll, 1992; Kanbur, 1998). Arguably, this research should also focus on particular urban and/or rural localities, and on sectoral experiences. Finally, inequality research could usefully shift away from the personal or household size distribution and towards other analytical frameworks, including regions and identity groups.[35] Even here, however, it would be a mistake to expect uniform relations between inequality and development across different contexts at, for example, the sectoral level.[36]

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[34] For a similar critique of endogenous growth theory and its weak empirical results, see for example Pack (1994).

[35] c.f Stewart (1998) on "horizontal inequality".

[36] On the complexity of inequality and agricultural development, see, for example, Ghosh and Sen (1993), Matson and Selden (1992), Bhardan (1989).

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