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The Politics of Redistribution:
Recent Developments and Research Perspectives*

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This note attempts to present the state of theoretical and empirical research on the politics of redistribution and to draw conclusions about future research priorities.

I contrast public choice models with models emphasizing the role of altruistic political attitudes and learning as well as with models stressing the role of party behavior. I also contrast methods of empirical testing based upon regressions between aggregate variables with those based upon data about individual political attitudes.

The poor empirical performance of basic public-choice models suggests the following research agenda. At the theoretical level, we need better theories of party behavior as well as theories that allow to distinguish between different types of redistribution: transfers to the elderly vs the unemployed vs the working poor, fiscal redistribution vs wage policy and price distorsions, "pure" redistribution vs "redistributive investments", etc.. Regarding empirical research, two priorities emerge: we need to construct more dissagregated cross-country data sets about actual redistributive policies, and we also need to shift our attention towards individual-level data by putting together the existing surveys about individual political attitudes in different countries.

I. Introduction.

This note attempts to present the state of theoretical and empirical research on the politics of redistribution and to draw conclusions about future research priorities. Needless to say, this brief survey is not supposed to be exhaustive.

In section II, I contrast public choice models with models emphasizing the role of altruistic political attitudes and learning as well as with models stressing the role of party behavior. In section III, I contrast methods of empirical testing based upon regressions between aggregate variables with those based upon data about individual political attitudes.

I then argue in section IV that the poor empirical performance of basic public-choice models suggests the following research agenda. At the theoretical level, we need better theories of party behavior as well as theories that allow to distinguish between different types of redistribution: transfers to the elderly vs the unemployed vs the working poor, fiscal redistribution vs wage policy and price distorsions, "pure" redistribution vs "redistributive investments", etc.. Regarding empirical research, two priorities emerge. We need to construct more dissagregated cross-country data sets about actual redistributive policies. Next and most importantly, we need to put together the existing surveys about individual political attitudes in different countries.

II. Recent Developments in Theory.

II.1. Public Choice Models I: Voting over the division of the pie.

The canonical model used by economists to think about redistributive politics has first been developed by Roberts(1977) and Meltzer and Richards(1978,1981). Given some initial distribution of income, agents vote over redistributive income tax schedules. Each voter votes for the schedule maximimizing his after-tax income. This voting game has exactly the same structure as a "divide-the-dollar" game, except that the size of the pie decreases with the distortions induced by redistributive taxation (the magnitude of which is assumed to be common knowledge). The majority rule is of course unable to yield a stable equilibrium in such games,[1] so Roberts and the following literature restrict their attention to linear redistributive tax schedules, whereby every pre-tax income y is taxed at a flat rate t and the tax revenue is used to finance a lump-sum transfer. Assuming that political parties are opportunistic, the median voter theorem applies. The equilibrium rate of redistribution is then determined by the most-preferred rate of the voter endowed with the median income. The general theoretical prediction is that if the median income is smaller as compared to the mean income, then the median voter will have more incentives to tax everybody at a high rate so as to finance high transfers, and therefore the equilibrium rate of redistribution should be higher. Countries with a lower median-income/mean-income ration should experience higher tax rates, and tax rates should increase in a country whose median/mean ration is decreasing over time (and conversely).

This model has recently been used as a building block dynamic endogeneous growth models where higher tax rates imply lower accumulation and lower long-run growth.

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[1]The "divide-the-dollar" game with 3 or more players is the canonical example of a game where majority cycles are pervasive. The fact that the size of the pie decreases as the distance to the initial distribution increases puts new limits to the set of possible divisions of the pie, but this does not change the basic dimensionality problem and the associated majority cycles (see Piketty(1993)). One can go beyond the non-existence of a static majority winner by looking for "stable" divisions of the pie in dynamic, forward-looking voting games; see Baron and Ferejohn(1989) and Piketty(1993) for such an analysis in terms of strategic dynamic games, as well as the large literature on cooperative equilibrium concepts. However, these models usually yield a large number of equilibria and do not deliver cler-cut prediction that could have been used by empirical research.

Persson and Tabellini(1994) and Alesina and Rodrick(1994) conclude from these two ingredients that "inequality" (as measured by the median/mean ratio) "is harmful for growth": the theoretical prediction is that countries with more income inequality (in the precise sense of a lower median/mean ration) should experience higher tax rates and transfers, that countries with higher tax rates and transfers should experience lower growth, and therefore that countries with more income inequality should experience lower growth. These predictions have stimulated a lot of empirical work, which will be covered in section III.1. below.

II.2. Public Choice Models 2: Voting over redistributive investments.

In the model described in the previous subsection, redistributive taxation has a purely redistributive role and fulfills no efficiency purpose. This is why the "size of the pie", as well as growth in a dynamic setting, can only decrease as redistributive taxation increases.

Benabou(1995,1996) has recently proposed to contrast the predcitions of this model with those obtained in a model where credit constraints imply that redistribution towards the poor can actually increase total output. The typical exemple of such "redistributive investments" consists of educational investments (using taxes to finance the education of the poor), although there are many others. We refer to Benabou(1996) for a detailed presentation of how the literature on schooling and segregation as well as that on credit constraints and capital accumulation fit into this general framework.

The existence of such redistributive investments does not imply that redistributive taxation can be Pareto-improving: the reason why these investments are not carried out in the first place is precisely because the poor must keep a high fraction of the returns in order to have adequete incentives. Therefore taxation can increase total output, but in general the rich will be made worst off. But the important point is that the preferences of the median-income voter over redistributive taxation are now different. The first difference

is obviously that the equilibrium level of taxation will be higher than in the absence of credit constraints. Most importantly, Benabou(1995,1996) shows that this equilibrium level of taxation does not necessarily increase as inequality increases: when inequality beceomes sufficiently high the gains from financing the redistributive investments of the very poor may become less and less internalized by the median voter.

The predictions of this model stand in sharp contrast with those of the pure "division-of-the-pie" model: the relationship between inequality and redistributive taxation and between redistributive taxation and growth can both be non-monotonic. For example, this model predicts that higher inequality can lead to lower growth because higher inequality leads to lower redistributive taxation, which in turns leads to lower growth. Although the predicted relationship between inequality and growth is the same as with the "division-of-the-pie" model, the causality channels work in the exact opposite way.

II.3. Public Choice Models 3: Non-Voting Models.

The models described above describe the political process as a voting process: other channels of political influence are assumed to be negligible.

Other models, such as the pressure-groups model of Becker(1983), describe distributive politics as a game in which competing interest groups spend ressources in order to influence political decisions: some exogeneous functions relate the ressources spent by each group to the equilibrium tax and transfer policies, and the political equilibrium is defined by the Nash equilibrium of this game.

This approach has recently been extended into 2 directions. First, Grossman and Helpman(1994) and Dixit(1995) have modelled endogenous redistribution through commodity taxes and subsidies as a game between lobbyists maximizing the special

interests of the various producers. As compared to voting models focusing exclusively on redistribution between the rich and the poor through redistributive income taxation, these models remind us that an important fraction of actual redistribution follows a different logic.

Another recent extension of these non-voting models of distributive politics is the literature on the "tragedy of the commons". These models are based upon dynamic strategic games between different income groups who can decide at each period to spend ressources to appropriate the returns to others' investment activities.[2] One can then investigate how the existence and efficiency of "cooperative" (low appropriation of others' investment, high accumulation and high growth) and "non-cooperative" (high appropriation and low growth) depends upon the initial distribution of economic ressources between these competing income groups. Benabou(1996) rightly points out that the typical predictions generated do not relate inequality to growth but rather the gap between economic inequality and "political inequality" (as measured by the abilities to grap others' investment) to growth.

Although these non-voting political interactions are probably part of a complete theory of redistributive politics, a problem faced by this approach is that it often fails to yield precise testable predictions for empirical research. The main reason is that the specific channel of political interaction is not very well defined: the concept of "ressources spent to grap others' taxes and investments" is very broad as compared to voting behavior. For example, Benabou(1996) points out that the link between the models and the empirical notions of "political instability" and the "rule of law" is very loose.

II.3. "Altruistic" Models: Learning and the Role of Social Mobility.

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[2]See, e.g., Barbosa, Jovanovic and Spiegel(1995), Benhabib and Rustichini(1996).

The traditional approach in political sociology describes the motives behind individual political attitudes in a way that is markedly different from the public-choice models based upon selfish maximization. For exemple, one of the dominant paradigm in electoral research is the "partisan voting" model associated to the Michigan School.[3] According to this theory, one's political attitudes are determined by one's identification to the values promoted by a given political party, and the mapping between individuals and parties is postulated to exhibit a very high degree of inertia. Another important exemple is the sociological theory of the effect of individual mobility experience on political attitudes. According to this theory, the socialization/resocialization experience of mobile individuals implies that they identify themselves both to their class of origin and to their class of destination, so that their political attitudes are intermediate between those of these 2 income groups.[4] It is important to distinguish this theory from the theory of the effect of one's income mobility prospects on political attitudes, which one can easily get in a generalized, forward-looking version of the "division-of-pie" model.[5]

These theories have generated a very large body of empirical research (see section III below). For lack of a better term, we will refer to these models as "altruistic models" so as to emphasize that in contrast to selfish models they describe political attitudes as the expression of values and psychological identifications.

From an economist's viewpoint, the "altruistic" models inherited from political sociology are unsatisfactory for 2 reasons. First, they do not describe very precisely the objectives

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[3]See, e.g., Converse et al.(1960) and Niemi and Weisberg(1993) for standard references.

[4]See, e.g., Blau(1956).

[5]This is not saying that both theories cannot operate at the same time. For exemple, the early attempts by Tocqueville and Marx to relate the presumed high mobility rates and the absence of any strong socialist movement in the US were refering both to the effect of past mobility experience and to the effet of mobility prospects.

individuals are promoting when "choosing" their political attitude, which makes it very difficult to perform any efficiency analysis. Next, they do not lead to very precise predictions about possible relationships between observable economic variables (like individual income or aggregate income inequality) and observable political variables (like the level of taxes and transfers), which does not facilitate empirical research.

One attempt to overcome some of these difficulties can be found in Piketty(1995a,1995b,1996). My basic premisse in this research is that individuals might well share the same "values" as far as distributive justice is concerned, but that they disagree about the way actual inequality between individuals is generated. It follows that they disagree about the magnitude of the incentive costs of redistributive taxation, and therefore that they end up supporting different policies. The next step is then to write down a learning model describing how individual agents form these beliefs as a function of their socio-economic history, so as to generate testable predictions between observable economic variables and observable political outcomes.

In Piketty(1995a), I consider the following simple model: individuals can become poor (income y_0) or rich (income y_1), the probability of becoming rich as a function of indivudual effort e_i is given by $\theta+\tilde{\theta}e_i$, and individuals have different subjective beliefs about the marginal product of effort $\tilde{\theta}$ as compared to the importance of luck and predetermined factors as measured by θ. Higher $\tilde{\theta}$s imply higher incentive costs of redistributive taxation from the rich to the poor, and therefore lower most-preferred rates of redistribution $\hat{\tau}$, for given, common social welfare objectives. At each period, agents choose some effort level based on their initial beliefs, they observe whether they become rich or poor, they update their beliefs accordingly by applying Bayes' rule, and finally they vote over next period's tax rates. One important theoretical implication is that whether agents update upwards their estimate of $\tilde{\theta}$ when they become rich depends on their initial beliefs: agents putting little effort (because they believe $\tilde{\theta}$ is low) will interpret a positive income shock as the proof that effort is not that important, and therefore will update downwards their estimate of $\tilde{\theta}$, while agents putting high effort will react the opposite way.

Therefore the theory predicts that the effect of one's mobility experience on one's political attitudes can go both ways. However, the theory also predicts that if one only observes the current income level and the current political attitude, then one should observe a negative correlation: individuals who belive more in effort will end up more often with high incomes (whatever the "truth" may be) and will vote for less redistribution, which generates a spurious relationship between income and policy preferences. For the same reasons, if one only observe the current income level, the current political attitude and the income level of the parents, then one should observe that mobile individuals' political preferences are intermediate between those of the 2 extreme stable income groups: for a given current income group, the fact that one's parents were richer increases the probability that one belongs to a "high-effort dynasty". This is again a spurious relationship, whose sign would become ambiguous if one could also include the parents' political attitudes on the right-hand side of the equation.

In Piketty(1995b) I extend this basic set-up and get additional predictions by looking at how this learning process reacts to exogeneous shocks to the model parameters. One interesting result is that under certain conditions high inequality leads to less redistribution than low inequality. The reason is that experimentation is costly: for given initial beliefs about effort, a higher income gap $y_1 - y_0$ between the rich and the poor is higher agents will lead agent to put more effort, which may never allow them to learn that low effort and high redistribution is optimal. Conversely, low inequality makes high-effort experimentation relatively more costly and therefore reduces the chance to learn that it is optimal. To put it in a crudest way, the rise of inequality starting in the 70s might have induced a shift in values towards effort and personal success in a way making it very difficult to build strong left-wing persuasion, and conversely for the previous period.

In Piketty(1996), I also extend the basic model to investigate the trade-off between fiscal redistribution and wage pressures. I endogeneize Akerlof(1990)'s "fair wage" by assuming that if low-wage workers get less transfers (and/or pay more taxes) than they think they should (given their beliefs about the economy's parameters), then the fair wage level below

which they put less effort into production is higher than the competitive level. The politically decisive agents might then react by choosing more fiscal redistribution towards low-wage workers than they would otherwise. I derive predictions about possible relationships between the diversity of beliefs and the equilibrium mix between fiscal redistribution and "fair-wage" redistribution.

Other recent attempts to go beyond the pure selfish motives behind the public-choice voter include models emphasizing individual concerns for relative status. For example, Corneo and Gruner(1996) show under what conditions the middle class might decide to vote against some advantageous transfer programs in order to avoid loosing its status by being identified to the lower class.

II.4. Political Science Models: the Role of Party Behavior.

All the models described above focus upon the determinants of the policy preferences of individual agents. The implicit assumption is that the distribution of individual policy preferences gets mechanically translated into some actual policy, either via the median voter theorem in voting models or via some lobbying or appropriation mechanism in non-voting models. This "individualistic" view of politics implicitely dismisses the independant role of corporate actors like political parties, just in the same way as firms were dismissed by traditionnal neoclassical theory. The traditional justification is that if parties are purely office-motivated (opportunistic), then they indeed act as mechanical translators of individual policy preferences.

However, this latter assumption is challenged by numerous works by political scientists analysing the internal evolution and functionning of political parties.[6] One typical example

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[6]This assumption is also challenged by the formal literature on political business cycles, which generally adopts (and find empirical support for) the assumption that parties have distinct policy preferences over the inflation/unemployment trade-off. See, e.g., Alesina

of this approach is the influential book by Kitschelt(1994) on the evolution of European social democracy. Kitshelt starts by arguing that during the past decades social democratic parties have been confronted to the rise of a 2nd dimension of conflict in the distribution of individual policy preferences. As opposed to the traditional dimension of conflict between income groups about redistribution, this 2nd dimension can be described as the "libertarians vs authoritarians" dimension and leads to conflict over environmental policy, participatory democracy, attitudes towards foreign workers,... The challenge for social democratic parties is to build new winning coalitions in this new environment, i.e. to face the trade-off between loosing some authoritarian blue-collars or loosing some libertarian professionals. Kitschelt then argues that whether, when and how left-wing parties make this type of trade-off in various countries is mostly driven by the internal choices of party activists and party organisations, and he goes on to analyze the internal organizational and ideological evolution in various countries.

However, formal theories of redistributive politics with non-trivial party behavior are still in their infancy. Roemer(1995) has recently developed a formal analysis of Kitshelt's trade-off between authoritarian blue-collars and libertarian professionals, in a model with partisan parties and a 2-dimensional distribution of policy preferences. Roemer derives the exact conditions under which the left-wing party will rationally choose to advocate limited redistribution and strong libertarian policies, thereby giving a new argument as to why "the poor do not expropriate the rich in democracies". These conditions on the distribution of preferences can then be tested by using survey studies. Roemer(1994) also developed a model where voters are uncertain about how the economy is working, so that a parties have a role to play in terms of trying to influence voters' beliefs.

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and Rosenthal(1989).

III. Recent Developments in Empirical Testing.

Some of the existing empirical work on the politics of redistribution is deliberately eclectic, in the sense that it combines historical evidence and case studies about changing policies and economic environments, electoral results and party structure in a rather informal way. This is for exemple the case of the voluminous political science literature of the type exemplified above by Kithschelt(1994). This type of approach will always prove to be very valuable, at least as a starting point, but here we choose to focus on methods of empirical testing based upon the analysis of statistical relationships between well-identified economic and political variables.

III.1. Aggregate-Level Testing: the "Growth" Framework.

The papers by Persson and Tabellini(1994) and Alesina and Rodrick(1994) have stimulated a lot of empirical work using the same methodology as Barro's cross-country growth regressions. That is, data about growth rates, inequality (typically the income share of the middle quintile), redistribution (generally the total share of transfers in GNP), as well as usual controls in growth regressions, is put together for a large number of countries and a long period of time, and multiple linear regressions are conducted between these different variables.

The basic result is that inequality seems to be harmful for growth. The regression of the average growth rate over some long period of time on initial inequality and several controls (like initial income and human capital stock) has now been run over a varitety of data sets and periods with many different inequality measures. We refer to Benabou(1996) for a survey of 21 such studies. Most studies confirm the initial finding of Persson-Tabellini and Alesina-Rodrick: the effect of initial inequality on long-run growth is

statistically significant and negative.[7]

However, this first finding does not tell us anything about the exact causality channel through which inequality is detrimental to growth. Perotti(1992,1996)'s methodical testing of the main explanatory theories brings no support to the political mechanism proposed by the theoretical literature. His findings, which have been confirmed by other works, are that the effect of inequality on the size of transfers is never statistically significant, and that the effect of the size of transfers on growth tends to be positive rather than negative! The absence of any systematic relationship between inequality and redistribution is confirmed by regressions using other measures of redistribution like average or marginal tax rates or education expenditures.

Perotti identifies other channels through which inequality seems to be detrimental to growth. Higher inequality seems to lead to lower investment when the credit market is less developed, which brings some support to theories based upon credit constraints. Higher inequality also seems to lead to higher "political instability", as measured by some index of coups, political violence,.., which itself leads to lower investment and lower growth. The lattest finding could be interpreted as support to non-voting models on inequality and appropriation games, except that the exact predictions of these models are only loosely related to the empirical measure of "political instability".

Another interpretation of these cross-country regressions is proposed by Benabou(1995,1996). Benabou starts by arguing that the basic premisse of "division-of-the-pie"-type models is completely contradictory with the absence of any negative effect of transfers on growth. Benabou points out that the positive effect of redistribution on growth could be capturing the efficiency effects of transfers in a credit-constrained economy, especially since when government consumption is used instead of transfers the effect turn

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[7]Whether this effect becomes insignificant once a Latin America dummy is added is more controversial.

negative. One could then interpret the absence of any significant relationship between inequality and the size of transfers as evidence for the non-monotonic relationship predicted by Benabou's theory of redistributive politics in presence of redistributive investments. The idea that at high inequality levels higher inequality leads decisive voters to internalize a lower fraction of redistributive investments seems also plausible at an anedoctal level (cf. US vs Europe), so it is fair to say this theory looks more promising than other public-choice models. On the other hand other theories could generate the same type of relationship (such as the learning model desceibed in section II.4.), and it seems virtually impossible to tell them apart without using data about individual political behavior, to which we now turn.

III.2. Individual-Level Testing: Electoral Data and Social Surveys.

Another way to test theories of redistributive politics is to use data about individual political attitudes towards redistributive policies. The absence of any systematic cross-country relationship between income inequality and the size of transfers suggests that the interaction between individual political characteristics, individual political attitudes and aggregate political outcome is more complex than posited in simple public-choice models, so that testing directly the reduced-form relationship between aggregate variables might be prematurate. Individual-level testing allows to make progress on the micro-determinants of political attitudes, and ideally this should allow us to come back to the more "grandiose" questions at some point.

Different types of individual-level data and associated tests have been used in the literature. For example, the purpose of traditional electoral research on partisan voting has been to show that responses to questions like "Generally speaking, would you consider yourself as a Republican, a Democrat or an Independant?" were by far the best predictors

of actual voting behavior.[8] Similar type of electoral data has also been used to test retrospective voting models.[9]

Sociologists have also been designing for a long time surveys asking to a large number individuals which party they vote for, their occupation (and/or income) and the occupation (and/or income) of their parents. The common finding across many countries and time periods is summarized on Table 1: higher income groups vote less often for left-wing parties, but the income groups of the parents also has a stong explanatory effect - of the same sign and approximately of the same size. This also holds when more dissagregated occupational data is used, as well as with direct questions about redistribution. This seems hard to reconcile with the postulate of selfish maximizing behavior, and at first sight this brings support to the sociological theory of the political consequences of personal mobility experience. At the same time as sociologists were finding these results (in the 50s-60s), they discovered that actual mobility rates did not seem to vary very much across western countries, from which many concluded that it must be the perceptions about mobility prospects that differ across countries.[10]

Some of this conventional wisdom about the effect of mobility experience on redistributive politics has been questionned by some recent empirical work by Kelley(1992). Unlike most previous studies, Kelley also has data about the parents' political attitudes. The very neat finding is then that the effect of mobility experience on voting behavior becomes insignificant once parents' voting behavior is introduced on the right-hand side of the regression equation. This contradicts strongly the sociological theory of Blau(1956): individuals with poorers parents are not more left-wing because they were exposed to poor people but because there is a higher chance that their parents were left-wingers. On the other hand, this is consistent with my learning theory: once initial beliefs

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[8]See the references in section II.4.

[9]See, e.g., Markus(1988).

[10]See Piketty(1995) for references.

are controlled for, the learning effects of mobility become ambiguous.

Another type of individual-level data that has been used comes from social surveys asking all sorts of questions about policy preferences and opinions together with usual set of socio-economic characteristics. One interesting finding from the International Social Survey Program (ISSP) on Attitudes Towards Social Inequality is that responses to abstract questions about inequality, personal responsability and distributive justice are substantially more similar across countries and social groups than responses to "practical" questions like those about the actual mobility opportunites of individuals.[11] This confirms the finding of a large body of psychological research showing that very different social groups share common principles of distributive justice ("talent is an irrelevant basis for desert unless it is seen as a result of previous effort",etc..) but that they strongly disagree about the relative importance of effort and predetermined factors for the actual generation of inequality.[12] This is consistent with the basic premisses of my theory.

In Piketty(1995b) I use data from the General Social Survey to investigate further this issue of conflict of interests vs conflict of beliefs. I use data over the 1972-1994 period, with more than 30000 individual responses altogether. In addition to questions about income and voting behavior, I use questions like "Do you think effort or hard work is the most important factor to get ahead?" and "Do you think we are spending to much on welfare?". One interesting finding is that although these qualitative answers are highly imperfect substitutes for the beliefs of the the theoretical model (answers can take only 3 values), almost 40% of the statistical effect that one would (wongly) attribute to income in a simple regression of voting behavior on income disappears once we include these answers on the right-hand side of the equations. This confirms the idea that distributive conflict is not a pure conflict of interest, and that one way to better understand cross-

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[11]See Smith(1989) and Evans(1993).

[12]See Miller(1992).

country and time-series variations in redistribution would be to better understand variations in these beliefs. At this stage, I can also show that a switch in "learning regime" seems to have occured between the 1970s and the 1980s: the regression of the beliefs about the importance of effort yields a coefficient on positive income shocks that is twice as large in the 80s than in the 70s, as was predicted by the theory.

IV. Research Agenda: Some Suggestions.

IV.1. Theory: Getting better theories of different types of redistribution and party behavior.

Existing theories of redistributive politics do not usually distinguish between different type of transfers and redistributive policy tools: most models view redistribution as a flat tax on all income financing a lump-sum transfer distributed to everybody. Actual redistributive policies are more complex in a way that is very relevant for the theory. For example, the huge rise of transfers observed in most western countries during the past 30 years has been mostly a rise of transfers to the old-aged population (both through pensions and health care) and not a rise of transfers (and/or decline of relative tax burden) to the low-income, working-age population. This is important if one wants to understand why in the current low-wage workers in western countries do not feel at all like the low-income agents of our models after a huge expension of redistribution. Another example is that most "vertical" redistribution is directed towards the unemployed in continental european countries, whereas a substantial fraction is directed towards the working poor in the US (through the Earned Income Tax Credit) and in the UK (through the Family Credit). This obviously reflects huge differences in prevailing views about the labor market, but the point is that "redistribution" can take very different forms that a one-dimensional model is

unable to capture. The important distinction between purely redistributive transfers and redistributive investments was already referred to in section II.2..

In other words, redistribution is multidimensional and we need theories that distinguish between these different dimensions. This could also prove to be useful in order to generate predictions allowing to better distinguish between selfish and altruistic motives behind political attitudes. The limited empirical success of the "encompassing" models of redistribution also suggest that more progress can be made in this direction.

The other area where theoretical advances are badly needed is the theory of political parties. It seems futile to try to understand the comparative history of transfers between, say, Sweden and Italy, without a better understanding of why one observe a strong social democratic party on the one hand and divided socialist and communist parties on the other hand. The question "what is a party?" is probably even more complex than the question "what is a firm?", and opening this black box is one of main challenges faced by formal modelling.

IV.2. Empirics I: Getting better measures of redistribution.

If we want to be able at some point to test in a satisfactory way the existing formal theories of redistributive politics, we need to be able to use better data sets.

First, we need better measures of redistribution. The existing empirical literature uses aggregate indicators like the total share of transfers in GNP. For reasons mentionned above, we need to have more dissagregated measures of transfers if we want to perform meaningfull empirical testing. For example, how can we interpret the regressions telling us that growth is positively related to the share of transfers in GNP if we don't know the nature of these transfers? Ideally, we would need to know, for a large number of countries, the size of transfers to the unemployed, to working families, to the old-aged population, to

the kids (via educational finance), etc.. Some of this data is already available in the existing cross-country data sets put together by the OECD, the World Bank or other institutions, but these data sets are often not precise enough and were not assembled from the perspective of redistributive politics. A major effort is therefore needed to construct new data sets, with a stronger relationship between theoretical predictions and empirical concepts.

IV.3. Empirics II: Putting together electoral and social surveys.

The poor empirical performance of aggregate-level regressions suggests that future research should rely heavily on individual-level data. All theories have very precise predictions about the relationship between individual economic characteristics and individual policy preferences, and we should first try to understand how these predictions fit the data across countries and over time. A lot of data about individual political attitudes has been collected in all developed countries and many LDCs over the past few decades, and this data could be used much more intensively in light of the theoretical predictions. In particular, cross-country comparisons with large data sets seem necessary to make progress on the key questions regarding the effect of income mobility on political attitudes and the relative importance of selfish and altrusitic motives.

However this data is typically dispersed across many institutions, some of it is publicly available and some of it is not, etc.. It would be very useful to put together these different data sets so that scholars working on redistributive politics can do serious quantitative research. Three broad catagories of data can be distinguished, in decreasing order of priority:

1-Longitudinal Social Surveys. These are are large national surveys, conducted each year with representative cross-section of individuals, asking both a large set of questions about socioeconomic characteristics and about policy preferences and opinions, as well as voting behavior for the latest election. The policy questions that are relevant for redistributive politics usually include questions about the level of tranfers to the poor, to the old-aged,

the level of unemployment benefits, questions about why people become and/or remain poor, questions about the level of taxes on different income groups, etc..

The first large longitudinal social survey was developed in the US in 1972 (General Social Survey) and now provides us with nearly 25 years of data. These data sets are unique in that they ask the same policy questions over log periods of time, which is a necessary requirement for this type of qualitative data to make sense. Many other countries now have their own longitudinal social surveys, including the UK, Germany, Italy,... In addition, the designers of these national surveys regularly conduct international special modules under the International Social Survey Program, including one on "Social Inequality" in 1987 and "The Role of Government" in 1985 and 1990. These special modules of questions allow a comparison of the relationship between individual characteristics and policy preferences over more than 20 countries.

This data has been completely underused from the perspective of redistributive politics and should be the first priority.

2-Post-Election Surveys. In each country one or several institute run some survey immediately after each election asking a large number of individual characteristics together with questions about the current and past votes. These surveys usually do not include detailled questions about policy preferences, but they tend to be a far better empirical source about actual voting behavior than longitunal social surveys, since the latter are rarely conducted immediately after the election, so that the answers to be strongly biased (such as for the US 1980 presiential election). Thus this data could be used as a complement to the longitudinal social surveys. Moreover these post-election surveys cover a larger number of countries over longer time periods.

3-Opinion Polls. Finally, in each country many polling institutes (Gallup,..) ask questions to reprensative national samples about all sorts of things. The major problem from a scientific viewpoint is that questions keep changing all the time. One can also include in this category the EuroBarometer Survey that have been conducted in European Union countries for the past 15 years, but which ask rarely the same questions more than once (except about European integration...). Sometime these are the only available sources and they should be regarded as such from our perspective.

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