# 6. Knowledge progress in comparative politics

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## 6.1 INTRODUCTION

A crucial issue for comparative politics is the extent to which knowledge can be – and actually is – systematically accumulated over time, whereby the generation of new knowledge is dependent on previously obtained knowledge. In theory, the vast majority of scholars agree that comparative politics is a progressive discipline that should function according to a developmental logic and produce cumulative knowledge whereby one 'knowledge block' is directly built upon the other, in an additive, complementary or alternative way. Indeed, as a matter of fact, cumulative research programmes generate knowledge that is more valid and more substantially enlightening than non-cumulative programmes, which produce findings that are only locally relevant (Mahoney 2003). Nevertheless, in practice, the objects of comparative politics are sparse and historically contingent, new but transient analytical frameworks constantly emerge and vanish, and empirical research relies upon a variety of methods which are often seen as 'culturally' different and even incommensurable (Mahoney and Goertz 2006). This leads to a situation where knowledge blocks, such as frameworks, theories, concepts and empirical findings, are mostly placed next to each other without necessarily connecting to each other. The goal of this chapter is to shed some light on the process of knowledge accumulation in comparative politics and to discuss the applicability of some pragmatic solutions. Since the discussion of 'causal findings' provides a hard case to assess the cumulativeness of a research programme (Mahoney 2003), the causal relation

between economic development and democracy is used as an exemplary illustration.

How to conceive this relationship represents a hotly debated topic in comparative political science, at least since Seymour Martin Lipset published his seminal book, *Political Man* (1963). Given its theoretical and substantial relevance, this question inspired a large amount of conceptual and empirical work. Scholars from different research traditions have dealt with the relation between economic development and democracy in markedly different ways, and the discussion seems never to have come to an end (Boix and Stokes 2003; Collier and Gerring 2009; Przeworski et al. 1996; Przeworski et al. 2000; Przeworski and Limongi 1993; Vanhanen 1990; Welzel and Inglehart 2003). From this example, we can derive a number of more general implications, draw some lessons on the main problems, such as the persistency of contradictory results and the failure of building on previous findings, and identify possible solutions to the task of making comparative politics closer to a progressive discipline.

## 6.2 THE PROBLEM OF KNOWLEDGE ACCUMULATION

Any truly scientific endeavour is, in essence, a project that entails formulating theories and examining them in the light of empirical observations. In turn, empirical observations are conditioned by prior theories and should be confronted with them (Lakatos and Musgrave 1970). It should be noted that the ordering of these operations is not given once and for all. On the one hand, our hypotheses can be systematically derived from highly formalized theories, such as rational choice models, and can then be tested by applying an appropriate methodological framework and a set of analytical techniques. On the other hand, other approaches, such as grounded theory, follow a more inductive strategy, based on exploratory analysis and ex-post theorization. In this latter case, data collection and observation are used to generate and refine our hypotheses, and, in turn, our theories. In the practice of research, these two poles are not considered as strictly opposed but they should rather be seen as the two extreme ideal-types along a

continuum of deduction and induction. Indeed, empirical researchers tend to rely implicitly or explicitly on 'abductive' reasoning (Aliseda 2006; Josephson and Josephson 1996). This kind of process consists of the permanent dialogue between ideas and evidence, in which ideas help to make sense of evidence, and evidence is used to extend, revise and test ideas (Ragin 1994).

No matter which of these perspectives is adopted, some influential scholars in the field of philosophy of science consider that scientific progress goes through normal phases of knowledge development punctuated by scientific revolutions. Kuhn (1970) describes normal phases as characterized by 'puzzle-solving' science, whereby previous discoveries and findings accumulate incrementally following a relatively linear path. Revolutionary phases, instead, are not incrementally cumulative. They involve a sudden revision of existing scientific beliefs or practices. Some of the achievements of the preceding phase are discarded, and indeed some of the problems that were considered solved could be put into question again because new systematic foundations are required. In both phases scientific progress occurs when members of a scientific community shift their assumptions about existing theories – either incrementally or radically – due to a process of systematically confronting them with other theories and new observations (Hall 2013).

The problem is that social sciences, and comparative politics more specifically, face recurrent criticisms of being adverse to the systematic accumulation of scientific knowledge. The unfeasibility of progressing through a coherent pattern of development is to some extent inherent in the nature of the social sciences. In that regard, James B. Rule (1997) has provokingly observed that theoretical innovations in the social sciences are often developed by scholars to get a prominent place in the 'marketplace of theoretical ideas' rather than for promoting a genuine theoretical advancement. In his view, this happens because the outputs and practical implications of social science research are less visible and less assessable by external observers (such as policy-makers, journalists, target groups, etc.) than those of 'harder' sciences, and eventually they matter little to outsiders. A corollary of this argument mentioned by Rule is that the 'obscure and self-referential jargon' that is adopted in some subdisciplines can be considered functional in creating new market niches that can survive and thrive precisely because of their

isolation. Following a similar line of reasoning, Shapiro (2009:2) quotes as examples of this 'flight from reality' the cases of 'the linguistic turn in philosophy and developments in literary hermeneutics [, whereby] practitioners in these fields often see themselves as engaged in interpretation rather than explanation, thereby perpetuating a false dichotomy.' Using Rule's terminology, these research programmes typically 'turn in upon themselves', in which they fail in answering their basic research questions, producing instead a number of 'problematiques' that are relevant for insiders only, whereas substantial progress should instead concern the broader scientific public through 'the development of analytical tools that subsequent thinkers "cannot afford to do without," regardless of their identification (or lack of it) with the theoretical programme that gave rise to them' (Rule 1997: 5) Examples of these productive analytical tools that are beneficial to scientific progress in the social sciences are Herbert A. Simon's bounded rationality framework, which provides foundational insights on modelling human agency in political science, economics and related disciplines, according to which bounded rational agents experience limits in formulating and solving complex problems and in processing (i.e. in receiving, storing, retrieving, transmitting) information (Simon 1957; 1982); and Mancur Olson's collective action theory, showing among other crucial insights that individuals in large groups have a tendency to 'free ride' on the efforts of others if the group does not provide selective incentives to produce collective goods, so that large groups are less able to act in their common interest than small ones (Olson 1971). This framework has been widely applied to several issue areas, by scholars working on economics, government and administrative policies, the welfare state, public choice, the sociology of organizations, industrial organization and management.

To be sure, theoretical pluralism and a certain level of heterogeneity are highly beneficial for a vigorous debate in the social sciences. However, reinventing the wheel several times, or even reinventing it square, can be hardly considered fruitful. It is a waste of intellectual energy and material resources. That is why (some more) cumulativeness is desirable.

Following Sjöblom (1977), the characteristics of a cumulative discipline are:

- the existence of a consensus about the fundamental underpinnings of the discipline;
- scientific discussions about specific problems rather than broad philosophical issues;
- the fact that several problems are regarded as solved;
- the systematic concatenation of subfields;
- a relative consensus on what constitutes controversy;
- the representation of the advances of the discipline in interrelated theories on which terms new problems are formulated;
- demand for the codification of existing knowledge.

As Anckar (1997) noted, however, this is not an adequate description of the state of political science, where the foundations of the discipline are much debated, basic philosophical issues are still disputed, problems are rarely considered as definitely solved, subfields frequently behave as close subsystems, and the codification of the discipline is erratic and scarcely systematic. These problems are even magnified for comparative politics. Although there is no doubt that a few very influential pieces of research have built upon one another and have inspired a multitude of other studies – such as foundational comparative historical works on democratic and authoritarian regimes (Linz 1978; Moore 1966; O'Donnell 1973) – this positive assessment cannot be generalized to the discipline as a whole (Mahoney 2003). Macro-comparative research typically struggles to reconcile its aim of attaining a certain level of cross-case generalization with the necessity of focusing on a non-random, quite heterogeneous, relatively small set of countries as units of analysis (Bollen, Entwisle and Alderson 1993). The empirical answers to crucial questions – the dynamics of war and peace, the emergence of social inequality, the explanation of specific institutional forms, the consequences of economic crises, and so on and so forth – are indeed both 'cacophonic' and highly case-specific (Rule 1997). Some generic reasons for the difficulties in accumulating knowledge in comparative politics have already been mentioned and are related to the structure of the academic field: there is a 'market for ideas' for which there exists a large supply of 'local innovations' that are required to get a tenure, a promotion or a grant.

However, following Blyth (2006), another important reason is endogenous to the theories we use in comparative politics. In his

view, human beings live in a very uncertain world, where relative stability is created by human agency through a long-term process of institution-building. In our theories however, we tend to assume much more stability than what is warranted. Nonetheless, large-scale change may happen, and quite unpredictably. This is why the development of comparative political science is punctuated by crises, through which existing models, theories, concepts and assumptions are discredited and pushed aside, as reactions to the failure of predicting crucial events, such as the rise of the Third Reich, the fall of the USSR or the so-called Arab Spring. These failures in turn lead to dramatic changes in the discipline – from structuralism, to behaviouralism, to rational choice, to hermeneutics, etc. The immediate implication of this state of affairs is that scientific progress in comparative politics cannot go through the search for general laws or grand theories, which are unlikely in the social sciences and especially improbable in comparative politics, as political phenomena and events are historically contingent and only imperfectly comparable across large numbers of entities. However, limiting the scope of the analysis to the non-generalizable attributes of the few cases under investigation is likewise unhelpful for knowledge progress. The scientific ambition of research in comparative politics, taken as a whole, should neither be of developing law-like wide-ranging theories nor of developing extremely narrow case studies. Rather, the 'middle-range' (Merton 1968) theorizing that is today largely dominant in the discipline could be taken even more seriously by explicitly specifying the context and the delimitation of our theories with respect to other existing theories, in order to make the most of existing pieces of research and to make clear the innovation potential of new studies. In other words, instead of creating artificial research designs and treating their theoretical arguments in isolation, comparativists should make explicit the conditions for the applicability of their arguments with respect to other studies that share commonalities.

This implies working at a conceptual level to situate each theory in a firm relationship to existing ones, from an ontological, epistemological and methodological perspective. For instance, Skocpol's theory of social revolutions (Skocpol 1979), which is based on deterministic foundations, an analysis of necessary and sufficient conditions, and a small N comparative strategy, can

provide lessons about the need for considering causal configurations when focusing on similarly complex phenomena, but cannot be directly applied to studies that postulate a net effect of a given factor on the unconditional probability of social revolutions in a large number of cases. This could perhaps limit the potential for innovation to what is actually needed. To do so, we have to deal with a number of challenges. The next section illustrates these challenges with the example of the debate on economic development and democracy. Afterwards, we will come back to some strategies for connecting the dots.

## 6.3 THE EXAMPLE OF THE DEBATE ON ECONOMIC DEVELOPMENT AND DEMOCRACY

The study of the complex relations between economic development and democracy constitutes one of the classic areas of research in comparative political science.

### 6.3.1 The Effect of Economic Development on Democratization

Since the early 1960s, there has been an intense and persisting debate about the validity of the so-called modernization theory (Lipset 1959) according to which economic development leads to stable democracy. Lipset's well-known argument is that economic development increases wealth and, through a coherent pattern, it raises education, communication, and in turn equality, which is associated with the presence of a large, moderate middle class supporting democracy. Hence, the central question is, whether a certain level of economic development can be identified which constitutes a sufficient condition to predict a transition from autocracy to democracy. However, there are unrelenting disagreements among scholars with reference to: concept formation (e.g. about the conceptual foundations of the theory and the definitions of democracy and dictatorship); research design and the empirical analysis (e.g. about the suitable measurement techniques and the reliability of existing data); and the conclusions to be drawn (e.g. about the

interpretation of results). To simplify matters, three distinct positions exist.

First of all, Przeworski et al. proposed, in an influential series of publications, to amend the classic version of the modernization theory by introducing a conceptual distinction between an endogenous and an exogenous theory of democratization (Przeworski et al. 1996; Przeworski et al. 2000; Przeworski and Limongi 1993; Przeworski and Limongi 1997). The endogenous explanation follows the modernization logic: through a gradual process of differentiation and specialization of social and political structures, economic development increases the likelihood that autocratic countries will undergo a transition to democracy. According to the exogenous theory, instead, democracy is established independently of economic development, but once in place it is more likely to survive in developed countries. Similarly, they maintain, autocracies are not likely to develop more rapidly than democracies. Przeworski et al. present empirical evidence for which the exogenous theory holds while the endogenous one fails. They have been able to identify a threshold of annual per capita income predicting that once established, democracy will endure regardless of its performances and all exogenous conditions. No democratic regime indeed has ever fallen in a country where per capita income exceeds a given amount (it was $6000 at the time of Przeworski et al.'s study; today it would be around $11 000). If their data are reliable, one can derive that this level of development, in absolute terms, may constitute an individual sufficient condition for the stability of democracy within the population of democratic countries. Instead, they show that among the 'initially poor' autocracies (those with less than $2000 annual per capita income) by 1959, among 98 countries only 26 had grown to $2000 by the exit year, 15 to $3000, 7 to $4000, and 4 to $5000. The only significant outliers are South Korea and Taiwan. The theory behind this empirical finding is however still incomplete. It remains a structural explanation. What are the mechanisms at work?

One line of reasoning suggests that the intensity of distributional conflicts is lower at higher income levels (Lipset 1963). Another hypothesis is that developed countries might be able to adopt superior institutional frameworks (Diamond 1994; Diamond, Linz and Lipset 1990). A radically alternative story suggests that the development of industrial capitalism fosters the organization of the

working class, making it more difficult for the elite to exclude these groups from political participation (Rueschemeyer, Huber Stephens and Stephens 1992).

According to the second position, conversely, the endogenous theory stands up well, too. Boix and Stokes argue that Przeworski et al. are drawing false inference against modernization from the fact that few autocracies (19 out of 123 in their dataset) did develop over time, became wealthy and eventually turned into democracies (Boix and Stokes 2003). The existence of few autocracies at a high level of development may be due to the fact that lower levels have already helped former autocracies to turn into democracies. Therefore, according to the authors, what deserves special attention is not the fact that the number of earlier developed autocracies that experienced a democratic transition is small, but the puzzle that some rich autocracies can survive at all. Of course, a possible answer could be that the explanation of democratic transitions is not monocausal, but multiple conditions are jointly necessary to produce the outcome. For instance, one missing variable hindering democratic transitions could refer to the immobility of capital assets, which is a characteristic of oil-producing countries (Boix and Stokes 2003). In addition, given that even discarding those quite peculiar cases, some dictatorships have lasted for many years in a number of developed countries (such as: Singapore, East Germany, Taiwan, USSR, Spain, Bulgaria, Argentina and Mexico), it is plausible that other conditions should concomitantly occur, for example international pressures (McGarry 1998; McGarry and O'Leary 2006).

The criticisms coming from Epstein and colleagues are similar but even more fundamental, as they crucially challenge the validity of the empirical analysis of Przeworski et al. by focusing on the interpretation of their findings (Epstein et al. 2006). According to the authors, Przeworski et al. failed to make correct sense of the coefficients reported in their model, so that they 'erroneously' reported the impact of gross domestic product (GDP) on democratization as insignificant. What is more, the authors formulate a key conceptual formation criticism by emphasizing the need for introducing a novel category, partial democracies, to fully capture the effect of development on democratization. This category represents a 'political grey zone', where countries have some attributes of democratic political life, such as regular elections, democratic

constitutions, and a certain space for opposition parties and at least embryonic civil society; yet they suffer from serious democratic deficits concerning poor representation, low political participation, law abuse, elections of uncertain legitimacy, scarce confidence in political institutions and poor institutional performance (Carothers 2002). The majority of the so-called third wave countries (Huntington 1991) fall into this category. Carothers mentions for instance, Nicaragua, Ecuador, Guatemala, Panama, Honduras, Bolivia, Bangladesh, Mongolia, Thailand, Madagascar, Guinea-Bissau and Sierra Leone as cases displaying a state of 'ineffectual pluralism'; while Cameroon, Burkina Faso, Equatorial Guinea, Tanzania, Gabon, Kenya, Mauritania, Morocco, Jordan, Algeria, Egypt, Iran, Yemen, Armenia, Azerbaijan, Georgia, Kyrgyzstan, Kazakhstan, and the major cases of Russia and South Africa are examples of 'dominant-power' politics. Accordingly, following Epstein et al.'s model, a high level of economic development both enhances the probability of consolidating existing democracies and of promoting the transition from autocratic to democratic regimes, whereas in the case of partial democracies the tendencies are erratic and quite unpredictable. Partial democracies represent a sort of residual, vague category that should be duly considered, as is the case with more recent conceptual and empirical literature on defective democracies, competitive authoritarianisms, etc.

A third argument contests any (direct) causal relationships between economic development and democratization (Robinson 2006). The correlations between the two variables are believed to stem from a set of omitted variables. For instance, 'good' economic institutions, such as secure property rights, the rule of law, etc., are both expected to sustain economic development and to influence democratic consolidation, if consolidation is simply defined as 'the persistence of political institutions typically associated with democracy' (Schedler 1998). Specifically, according to Robinson, the pattern of correlation identified by Przeworski et al. is merely generated by a cross-sectional effect, while it is interpreted by them as a time-series variation. In other terms, Przeworski et al.'s dataset only demonstrates that 'richer countries are more democratic', but not that 'as countries become richer, they become more democratic'. Using historical data from the beginning of the early modern period, the author finds that countries whose income per capita increases do tend to become (and remain) more democratic:

that is why there is a cross-sectional positive effect today. As said, the principal interpretation offered by Robinson is that there is no causal effect, but over very long periods of time, income per capita and democracy are positively correlated because countries move along long development paths where these two variables are jointly determined by other, deeper, structural factors. This objection is situated at the methodological level.

However, Robinson's pooled approach assumes that effects are stable across space and time, which is actually quite implausible for complex phenomena (Kittel and Winner 2005). In addition, the author apparently mixes up the discussion of causal factors for democratic transitions (for which he presents some evidence) with the question of democratic stability (for which no conclusive evidence is offered), and he also confounds the examination of the average impact of the development level with the aim of identifying a threshold effect. Indeed, Przeworski et al.'s argument is not that the likelihood of democratic transition or democratic stability increases monotonically as economic development increases, but rather that after a certain level of development the probability of democratic breakdowns decreases dramatically. In fact, another interpretation of historical data also exists, which is incidentally acknowledged by Robinson: a causal impact of income per capita on democracy could appear (only) after very long time periods.

At the end of the day, the proper role of economic development in transitions to democracy remains controversial and very sensitive to the specifications of the analytical models. Furthermore, another crucial question is still open, that is, the stability of democracy at lower levels of development. This is of course a puzzle that entails strong normative implications. At this point it is worth reminding ourselves that modernization theories actually offered a 'political instrument' and a 'cognitive framework' through which US foreign policy elites interpreted and justified their actions and their country's role in the world, especially during the Kennedy years (Latham 2000). After all, arguing that a certain level of economic development is necessary to sustain democracy could operate as an argument for prioritizing economic development over democracy in poor countries, offering a potential legitimizing strategy to non-democratic and partially democratic regimes. For instance, Huntington (1968) controversially argued that the maintenance of order

under autocratic regimes could be a precondition for the development of economic growth in transition countries and subsequent evolution towards democracy.

The other way round, the viability of strategies promoting democratic institutions in poor countries will be supported even more, if democracy is also a pre-condition of economic development in the medium-long term. But one should expect the relationship between the two variables to be more complex than what is supposed above. First, the threshold of annual per capita income identified by Przeworski et al. cannot be considered as strictly necessary, given that democracy can also survive at lower levels of development (Alesina and Rodrik 1994; Rodrik 1997), as the crucial case of India shows (from the World Development Indicators database, we know that India's purchasing power parity (PPP) per capita income was $5410 in 2013). Looking at the low ranking of this country, we can doubt that a high level of economic development is necessary at all. It is however plausible that in very poor countries democracy will be unstable: according to Przeworski et al. 39 out of 69 democracies fell in countries that were among the poorer. Their 'intuitive story' is that the struggle for becoming a dictator is more attractive in poor countries, due to the higher marginal utility of predating existing resources at lower levels of development. Therefore, several questions remain open, including whether there exist other conditions that are sufficient for sustaining democracy at relatively low levels of economic development.

### 6.3.2 The Effect of Democracy on Economic Growth

Interestingly, the inverse causal relationship is widely debated in comparative politics, too, even though it is somewhat counter-intuitive to expect at the same time an independent effect of the variable X on the variable Y and, respectively, an effect of the same Y on the same X. It is a sort of new variation on the chicken or the egg dilemma. However, the effect of democracy on economic growth has been extensively investigated (Helliwell 1992). Unsurprisingly, as for the inverse relation, results remain contradictory. According to an extensive review article of 84 published democracy-growth studies (Doucouliagos and Ulubaşoğlu 2008), 15 per cent of the regression estimates are negative and statistically significant, 21 per cent are negative and statistically insignificant,

37 per cent are positive and statistically insignificant, and 27 per cent are positive and statistically significant. This is not good news for the progressive accumulation of knowledge.

To sum up, again, three main positions exist:

1. The first argument is that democracy has no observable implications for growth. The proponents of this view argue that variables other than political regimes matter for economic growth, and policies that favour growth can be promoted independently from the subsistence of any political regime. Strong governments are supposed to be decisive for resolving problems of collective action and build the adequate conditions for growth (Bardhan 1993). Free markets are regarded as determinants for growth under both democracies and autocracies (Bhagwati 1995). These studies, however, generally rely on restrictive definitions of democracy (e.g. free elections), which disregard other substantial features such as civil rights, free information and citizens' control over governmental decisions (Barber 2004; Beetham 1999; Dahl 1989) that may uncover a differential impact.
2. The second position underscores the fact that democracy has moderate negative or null direct effects on growth, but it has various indirect positive effects. First of all, following Huntington, (novel) democracies have relatively weak political institutions and, being submitted to redistributional pressures and rent-seeking demands, they cannot sustain growth-oriented policies (Huntington 1968). Accordingly, the overall effect of democracy on growth is expected to be moderately negative (Barro 1996; 1999). Barro suggests a nonlinear relationship, whereby an increase in democracy may enhance economic growth at low levels of democratic development, but the same will reduce growth when higher levels of political freedom have been attained. According to other studies, the overall effect of democracy on economic growth is found to be moderately negative, when taking into account the positive indirect effects (democracy fostering growth especially through the accumulation of human capital) and the negative indirect effects (democracy reducing the rate of

physical capital accumulation and raising the ratio of government consumption) (Tavares and Wacziarg 2001). A fine-tuning of the argument with extensive meta-analysis shows a zero direct impact of democracy on economic growth, but highlights a significant, robust and positive indirect effect through higher human capital, lower inflation, lower political instability, and higher levels of economic freedom (Doucouliagos and Ulubaşoğlu 2008). It should be noted, however, that the general limit of this kind of econometric studies is the exclusive focus on the short-term effects.

3. The third set of arguments assumes a positive relation between democracy and growth. The basic idea is that democracy produces incentives for trade and investment, by securing the rule of law, and ensuring a climate of liberty, property rights and the free-flow of information (Baum and Lake 2003; North 1990; Rodrik 1998), while dictatorships display a propensity to confiscate resources, especially when they expect to be short-lived (Olson 1993), and they tend to be more corrupt and inconsistent (Nelson 1987). However, empirical findings are not robust enough to support a direct link with immediate economic growth (Sirowy and Inkeles 1990). Some recent research studies propose a theoretical framework that emphasizes complex causal relationships between growth and democracy. For instance, Gerring and colleagues propose a systematic historical examination of the net effect of democracy on growth (Gerring et al. 2005). They maintain that if democracy matters for growth today, this positive effect does not stem from its current level or degree, but from the stock of experience with democracy. In this sense, democracy is treated not as an immediate triggering factor, but as a long-term determinant. The institutional effects are supposed to unfold over long periods of time, and to be cumulative. When measured as a stock variable, democracy appears to have a strong positive relationship to growth performance regardless of the specifications of the model. In addition, to correctly model these complex phenomena, it is reasonable to restrict comparison to similar countries, instead of comparing all existing cases (Minier 1998): the trajectory of Portugal should not be compared to the patterns of

democratization in Ecuador or Peru, but to originally high-income countries also exhibiting high growth rates, such as Greece and Spain. Using this approach, the author is able to demonstrate that changes towards increased democracy produce faster growth than for similar countries that did not experience democratization.

Jointly with the self-enforcing effect of economic growth, a crucial condition for democratic stability is expected to correspond to income equality and the related presence of distributional policies. A certain theoretical consensus exists on this point (Li and Reuveny 2002; Reuveny and Li 2003). Not higher per capita income, but economic equality should play a crucial role in increasing both the chances of democratic transitions and the stability of democratic regimes, while per capita incomes also rise in countries where incomes are becoming more equal (Boix and Stokes 2003). The possible explanation of Boix and Stokes' argument is as follows: economic elites living in countries where incomes are quite equally distributed are more likely to sustain democracy because the redistributive scheme chosen by the median voter would deprive them of less income than if income distribution were highly unequal. Following the experiences of many Latin American countries, it appears that huge material inequalities provided the social basis for authoritarian regimes, and, where income inequality is the greatest, people are more willing to accept dictatorship and are less satisfied with the way democracy works (Karl 2000).

Empirical results are however mixed, again. To begin with, a very strong inverse correlation has been observed between income inequality and regime stability for a sample of 33 democracies (Muller 1988). Democracy is likely to be associated with lower income inequality in the long run when observed from a diachronic perspective. This association holds independently of economic development, which is found to have no direct effect on democratic stability after controlling for income inequality. Moreover, Alesina and Rodrik show that democracies with uneven distributions of income will have lower growth than democracies with more equal distributions (1994). The other way round, other studies found no significant association between the two variables (Bollen and Jackman 1989), and quite surprisingly a systematic review of the existing evidence suggests that democracy is not indisputably

related to lower levels of income inequality, when measured at one point in time (Sirowy and Inkeles 1990). Bollen and Jackman argue that data quality is generally suboptimal, indicators are underspecified and methodology is not always appropriate. More specifically, they assert that previous research confounded stability with democracy, leading to measurements difficult or impossible to interpret, and that democracy should be measured in continuous terms, and not as a dichotomy, as it is commonly assumed.

To sum up, the debate on the (bidirectional) relation between economic development and democracy, a central topic for comparative politics, leads to very mixed findings. As we have shown, different procedures of concept formation and different methodological choices largely account for these inconsistencies. The variable 'democracy' is conceptualized, operationalized and measured in very different ways, ranging from a minimal definition of electoral democracy to encompassing formulations involving several additional criteria such as the quality of public participation. Different concepts relate to different theories and produce biases towards different empirical results. Similarly, the juxtaposition of studies using different methods might imply complications in interpreting their reciprocal implications and even more in combining their findings. More explicit reflexion is required on this crucial point. For instance, macro-quantitative studies detected at least an association between economic development and democracy, but they presented diverse and sometimes contradictory results. Single-country studies about democracy and economic development typically highlighted the role of case-specific factors, and comparisons involving a small to medium number of cases offered rather inconclusive results, because democracy is seen as the product of discrete and contingent events. The actual problem is not about conceptual and methodological variety, which is on the contrary potentially helpful for improving the analytical reach of comparative politics (see Chapter 5 in this volume). Instead, the main problem lies in the lack of communication and potential misunderstanding among scholars working with these different tools.

## 6.4 WHAT WE CAN LEARN: TOWARDS POSSIBLE SOLUTIONS

At baseline level, two critical points for improving the accumulation of knowledge in comparative politics can be identified.

### 6.4.1 Improve Concept Formation

The first is the importance of concept formation. As our review has shown, the strength of the association between economic conditions and democracy primarily depends on what is considered to be a case. When a concept such as 'democracy' is defined too loosely, then several 'false positive' or 'false negative' cases are included in the analysis, which could artificially confirm or disconfirm a causal theory (cf. Braun's Chapter 4 in this volume). Mahoney and Goertz (2004) quote in that regard the 'raven paradox' elaborated by the German philosopher Carl Gustav Hempel, according to which the hypothesis 'all ravens are black' is also counterfactually supported by evidence based on all 'non-black' things that are also 'non-ravens', e.g. by green apples. This is a logical fallacy, which demonstrates that irrelevant and incongruent pieces of evidence should be actually excluded from a comparative analysis. To avoid this type of bias in the study of democratization, the coding of the regimes under investigation has been fruitfully extended beyond the usual dichotomy of democracy/autocracy, by including the category of partial democracies, and by adopting a fine-grained scale, so as to differentiate between 'true' and 'false' positive and negative cases.

Another crucial conceptual issue derived from our review is related to the requirement of contextualizing comparative procedures when needed. Ideally, concepts should be precise enough to characterize unequivocally the phenomena of interest and at the same time be able to 'travel' across time and entities in order to be applicable to all possible relevant cases. In practice it is however difficult, sometimes even impossible, to strike a good balance between the so-called conceptual intension – the number of properties covered by the concept – and extension – the number of the objects to which the concept correctly applies (cf. Chapter 2 in this volume) (Sartori 1970; Sartori 1991). When this balance is

impossible (because theory is not yet sufficiently developed or the phenomenon under study presents an inextricable degree of complexity), then it is reasonable to limit our ambition to restricted, contextual studies that ensure valid, although circumscribed, cross-case comparisons. In order to reinforce the cumulativeness of findings it is indeed preferable to add a small piece to a larger puzzle than producing a sequence of fuzzy pictures. This means that it is extremely important to operate a close connection between a concept and the indicator used to operationalize that concept, even if this considerably reduces the scope of the empirical analysis. Too often, political scientists satisfy themselves with imperfect proxies in the absence of more appropriate empirical data. The risk is however to generate confusing, syncretic results, which are strongly inconsistent across different studies, and whose external validity is doubtful. The priority should be to construct a good measure for the phenomenon under consideration, rather than maximizing the number of cases and variables (Maggetti and Gilardi 2015). Then, findings can be considered valid under a more or less restrictive number of scope conditions.

Qualifying statements apply to all theories, even in the much-admired prototypical studies in the 'hard sciences' (Harris 1997). For instance, Newton's principle regarding the acceleration of gravity, according to which a free-falling object has an acceleration of 9.8 m/s/s downward regardless of its mass, requires at least two scope conditions: being on earth's gravitational field and the absence of friction. Scope conditions hold an even more important role in hypotheses formulation and testing in the social sciences because they are much more pervasive and their usefulness should be acknowledged more widely. They are not observational statements. They should not be equated to independent control or intervening variables. They do not permit to indicate the micro-foundations of the causal mechanisms at work in a hypothesized relation between independent and dependent variables. Instead, they 'specify circumstances under which the relationships expressed in hypotheses are expected to hold true' (Cohen 1989).

Strictly speaking, scope conditions neither explain nor determine the explanandum, but they restrict the applicability of a causal relation and define the conditions for observing an expected outcome. They are useful to explore complex causal relations and distil more precise hypotheses for the phenomena under study. When

narrowly defined, they also provide evidence about the relevant variables to explain negative and positive findings, and they offer an instrument to delimitate the context in which hypotheses apply (Foschi 1997). An example is McAdam et al.'s study of contentious politics, wherein they situated the scope conditions for the applicability of current theories of social movements to mobilizations in Western countries (McAdam, Tarrow and Tilly 1996).

### 6.4.2 Align Ontology and Methodology

The second point relates to the need for identifying the causal structure of each theoretical argument in order to compare and contrast empirical findings from disparate studies. In general terms, this point relates to the need for aligning ontology and methodology in comparative research as it was famously advocated by Peter Hall in an influential text (Hall 2003). In particular, a linear additive relation among variables of the type 'the more economic development, the more (or less) likely is the transition to democracy' cannot be assumed to be universally valid. Causal propositions can indeed involve equifinality and conjunctural causation (Ragin 2008).

Equifinality occurs when different causal patterns lead to the same outcome, each one being sufficient but not necessary to predict the latter. This is for instance the case when a given combination of variables is found to explain the patterns of democratization in a number of countries, for instance Latin American countries, while a different combination of variables might lead to democratization in another set of countries, e.g. in North Africa. Some of the variables that matter for this second, smaller set of countries, such as 'international pressures', can be 'insignificant' when included in a large N macro-comparative study, yet they have a crucial causal impact for these specific cases, which are of substantial (although not numerical) importance.

Furthermore, conjunctural causation refers to a situation where a given effect is produced by a combination of factors, which are individually inert but jointly sufficient to predict the phenomenon of interest. Continuing with our example, it is possible that social protest or the breakdown of state institutions alone have no unconditional impact on the probability of democratization. However, perhaps, their concomitant presence makes democratization more

likely. As with a chemical reaction, these conditions are reactive only when they interact. Similarly to equifinality, recognizing the possibility of conjunctural causation allows researchers to highlight and study causal connections that could be otherwise overlooked and remain unnoticed. Moreover, considering the possibility of equifinality and conjunctural causation is important to enhance the cumulativeness of findings, because it can enlighten the relations among case studies and large N analyses, and make the cross-fertilization across case studies more systematic. For instance, positive but weak statistical correlations in macro-comparative studies could indicate that more than one combinations of conditions lead to the same outcome, for which one combination applies to a specific set of countries (or other units of analysis) while another, different combination applies to another set of countries (Mahoney and Goertz 2006).

Next, as we have seen above with the example of the impact of democratic institutions that unfolds and becomes visible only over time (Gerring et al. 2005), the notion of temporality may need to be treated in a more differentiated manner than how it is sometimes assumed. This would help in favouring complementary research programmes focusing on time-related factors. Time is an important dimension of variation, along with synchronic variations, which requires the comparison of two or more points in time. In a research project we may want to focus on different aspects of the time dimension as we pursue different research goals. In each case, the appropriate methodology should be applied (Maggetti, Gilardi and Radaelli 2013). To begin with, it is possible to examine the duration of social phenomena with historical or with simulative methods. This approach can be used, for instance, to study how cooperative attitudes among individuals can emerge without central authority. Then the analysis of tendency makes it possible to characterize the overall trend of macro-phenomena or point out the presence of unexpected deviations and anomalies within a trend. Levels and stocks represent entities in which variables can be aggregated to account for different operationalizations of quantities when examined over time. Time-related variables may also refer to the explanatory power of sequencing, that is, the ordering of events. In this case, typical analytical techniques are time-series analysis, optimal matching, temporal qualitative comparative analysis, process tracing and game theory. In this sense, the temporal ordering

of events or processes is expected to have a crucial impact on outcomes. Furthermore, the notion of timing refers to the causes and effects of transitions from one social state to another, and can be used to study the consequences of specific events over time, for which the standard methodology is event analysis.

To sum up, 'time' relates to very different features. Accounting explicitly for different operationalization of temporality is helpful to ensure consistent and coherent understanding of the role played by this dimension of variation. As an example, Bartolini (1993) asked what is the temporal unit to be applied in the study of the French parliament. The argument goes, it depends on the research question. If one is interested in the role of the parliament in the political system, a good choice would be to select different regimes as the temporal units. The legislative period is more adequate when focusing on the quality or on the quantity of legislation. Shorter periods can also be considered if one expects that they can be relevant for specific policies. It is however not always easy to measure each property on each temporal unit.

### 6.4.3 Towards Possible Solutions

After advocating for more careful concept formation and attention to the structure of causal arguments, we can move to some more specific recommendations. We do not aim at finding a final answer to the problem of the scarce cumulativeness of knowledge in the social sciences. More modestly, we suggest some pragmatic strategies for making knowledge progress in comparative politics more concrete.

(1) To begin with, in our literature review on the relation between economic development and democracy we identified different 'ways of doing' that have distinctive advantages and limitations. Thus, the first strategy is to explicitly recognize the existence of trade-offs in research design. To conceive different design choices as trade-offs – compromises on something to achieve something else instead of 'culturally' opposed schools of thought – helps in favouring fertile exchanges across research traditions. A crucial trade-off is between the validity (or reliability) and the relevance (or realism) of the research question. Schematically, one can focus on very small, tractable questions at the expense of big problems that are harder to study (or the reverse). The first type of

study will provide reliable inferences (that is, conclusions grounded on empirical evidence) on very specific causal relationships, while the second maximizes the theoretical relevance and the complexity of results. The former type of research design can produce valid estimates of causal relationships, but many different analyses are necessary to give the full picture of a complex phenomenon. By contrast, the latter type of research design puts the emphasis on a holistic view of causal processes, but at the cost of validity. No choice is intrinsically superior if it is transparent and duly motivated; however it is important to note that generalization can be achieved through the accumulation of well-designed but necessarily narrow studies, while unreliable results, no matter how wide-ranging, are of little use from the point of view of knowledge progress, too (Maggetti, Gilardi and Radaelli 2013).

(2) The second strategy is to develop further 'middle-range' theoretical work on the connection between levels of analysis. This allows researchers to find a common ground across their research niches, improve their exchanges and eventually build upon each other's results, even when research projects are based on different theoretical foundations. This is at odds with overarching, hegemonic theories such as rational choice (see Braun Chapter 4 in this volume). A nice example of this type of effort is Zahariadis' piece on the theoretical frameworks of the EU's policy process (Zahariadis 2013), who in fact does not include the rational choice-inspired principal–agent model in his argumentation. He argues in favour of intellectual pluralism in the place of zero-sum intellectual debates, showing that, instead of considering different frameworks as right or wrong answers to a given problem, they can be seen as a menu of different choices that can communicate to each other. Building bridges across frameworks also allows researchers to build better frameworks, which are not only competing but can also be 'horizontally' complementary.

To begin with, investigating different theoretical frameworks pushes the analyst to clarify her own assumptions and expectations. What is more, an approach incorporating several frameworks fosters the development of competing hypotheses and therefore reveals the strengths and weaknesses of various theoretical perspectives. In his illustration, Zahariadis classifies the existing frameworks for EU policy-making according to the way they deal with issue complexity (which refers to the amount and nature of informational linkages

and may be operationalized as the degree of information overloads) and with institutional complexity (which refers to a multitude of rules governing close interactions among a high number of structurally differentiated units across different organizational levels). This way, it is possible to specify which theoretical framework is of better use for the study of each specific configuration of variables (the following examples are all mentioned in Zahariadis' piece). For instance, the multi-level governance and punctuated equilibrium approaches may be viewed as competing on issues involving higher institutional complexity (see also Chapter 3 by Kübler in this volume). Constructivism and policy learning are competing explanations in cases of lower institutional complexity. Policy learning and multiple streams are complementary as they diverge on assumptions about institutions and issues. The next step is to improve the deficiencies of each framework by relaxing its assumptions and using different analytical techniques to clarify its logic and test its hypotheses. Complementary frameworks may be used to account for different aspects or stages of the same issue and explain or predict outcomes more accurately. Competitive frameworks can be then ordered from the most to the least suitable, they can be merged together or ultimately dismissed.

(3) The third strategy implies the use of systematic reviews and meta-analysis, which are still underdeveloped in comparative politics, although they are widely used in related disciplines such as social psychology, health studies and economics. Meta-analyses are essential for taking stock of previous research, producing cumulative knowledge and developing a progressive research programme (Borenstein et al. 2009; Cooper, Hedges and Valentine 2009; Cooper, Lindsay and Patall 2008; Lipsey and Wilson 2001). Classic meta-analyses such as those used in epidemiological studies aim to combine the results of each study in a single measure of effect size or another statistical measure. However, another more feasible form of meta-analysis is more common in the social sciences, whose logic is closer to systematic reviews. This time the goal is to extract patterns from a sample of studies, to understand the logic behind methodological choices, to assess their coherence regarding the conceptualization and operationalization of indicators, and to compare and contrast empirical findings. This approach is in line with the goals of meta-analytical techniques when broadly defined, e.g.:

'methods focused on contrasting and combining results from different studies, in the hope of identifying patterns among study results, sources of disagreement among those results, or other interesting relationships that may come to light in the context of multiple studies' (Greenland and O'Rourke 2008). For instance, Maggetti and Gilardi (2015) used a fuzzy-set-based meta-analysis to examine 114 studies on policy diffusion, demonstrating persisting inconsistencies in the measurement of the mechanisms driving policy diffusion processes, such as learning, emulation and competition. Different indicators are used to measure the same mechanism, and the same indicators are used to measure different mechanisms. To improve this state of affairs, the authors put forward a conceptual structure serving as a guide for the coherent application of diffusion arguments in order to ensure measurement validity. It is indeed important to pay attention to the conceptual consistency of indicators. This requires the construction of original, innovative research designs instead of the constant replication of widely-used templates even in cases where they are not very suitable.

## 6.5 CONCLUSION

The cumulativeness of research is typically scarce in the social sciences – and particularly weak in comparative politics – leading to a process of knowledge progress that is highly scattered, uncertain and essentially inefficient. The example of the literature on the relation between economic development and democracy, a longstanding classical topic, has shown that even in this case findings are very mixed, contradictory and only imperfectly built upon a progressive research programme. These problems largely stem from what we called a difficulty in 'connecting the dots', which results from the institutional organization of the discipline that favours a niche-oriented strategy but also from the erratic nature of political phenomena to be compared. We discussed some existing solutions, in a non-exhaustive way. On the one hand, concept formation should be given special attention. Concepts should be precise enough to characterize unequivocally the phenomena of interest and at the same time be able to 'travel' across time and entities in order to be applicable to all possible relevant cases. When this ideal balance is impossible, then it is

reasonable to limit our ambition to restricted, contextual studies that ensure valid cross-case comparisons although circumscribed within some previously specified scope conditions. Next, recognizing the causal structure of scientific arguments is equally important. Considering the possibility of equifinality and conjunctural causation allows researchers to highlight and study causal connections that could be otherwise overlooked and remain unnoticed. It also helps to enhance the cumulativeness of findings, because it can enlighten the relations among case studies and large N analyses, and make the cross-fertilization across different studies more systematic. Then, to conceive different design choices as trade-offs – compromises on something to achieve something else instead of 'culturally' opposed schools of thought – also helps in favouring fertile exchanges across research traditions. It is also recommended to develop further 'middle-range' theoretical work on the connection between levels of analysis. This allows researchers to find a common ground across their research niches, improve their exchanges and eventually build upon each other's results, even when research projects are based on different theoretical foundations. Finally, systematic reviews and meta-analyses could be used more extensively in order to extract patterns from a sample of studies, to understand the logic behind methodological choices, to assess their coherence regarding the conceptualization and operationalization of indicators, and to compare and contrast empirical findings. This way, meta-analyses provide important tools for taking stock of previous research, producing cumulative knowledge and developing a progressive research programme.

To conclude, we do not aim at achieving a linear scientific progress or a perfect process of accumulation of universally valid findings in comparative politics. It would be however desirable to bring comparative politics closer to a progressive discipline, where a certain consensus exists about the fundamental underpinnings of the discipline; scientific discussions about specific problems rather than broad philosophical issues; several problems are regarded as solved; subfields are systematically concatenated; a relative consensus exists on what constitutes controversy; the advances of the discipline are represented in inter-related theories on which terms new problems are formulated; and existing knowledge is at least partially codified. The application of the aforementioned strategies for 'connecting the dots' could represent a partial, non-exhaustive

and pragmatic way forward. To do so in such a plural field as comparative politics, the endorsement of a reasonable degree of analytical eclecticism is recommended (Katzenstein and Sil 2005; Sil 2000; Sil 2004). This analytical perspective is not a priori committed to a specific theoretical perspective; it is ontologically relatively flexible; and problem-oriented. Analytical eclecticism is a fruitful middle path between the ambition for a synthesis moving beyond existing approaches (i.e., the convergence upon a new, unified framework of assumptions, concepts, methods and interpretive logics) and, on the other side, the mere juxtaposition of competing paradigms. Rather, it consists in the – quite inductive – integration of different research traditions into a single explanatory framework; at the same time, they are continuing to evolve on relatively separate tracks in order to portray the complexity and contingency of the social world and human agency, which risk being disregarded when adopting a single, coherent, elegant, parsimonious theory (Hirschman 1997). To do that, the distinct epistemological postulates underlying each research tradition should be adjusted, relaxed or even suspended. Following Katzenstein, the analytical advantages of eclecticism are numerous: it enhances analytical leverage through the study of power, interest and norms; it highlights different connections that parsimonious explanations dismiss; it protects researchers from taking as natural paradigmatic assumptions about the world; it offers a safeguard from the unavoidable failings of any one paradigm; and it helps making sense of empirical anomalies (Katzenstein and Okawara 2001).

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