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Preface

THE POLITICS OF THEORY IN THE HISTORY OF SCIENCE

Pietro Corsi

At first sight, there would make little sense for a professor from the University of Oxford to write a foreword to a collection of essays by Brazilian colleagues, devoted to the assessment of scientific activities — broadly understood — carried on in the State of Parana during the middle decades of the twentieth century. Nor would it be a sufficient justification to reference the personal ties I have formed over the last three years with the organizer of this book, Fabiano Ardigó, a graduate student in the History of Science Programme at Oxford. True, I have read — out of sheer interest — earlier versions of almost all of the chapters contained in this collection, and discussed them at length with Fabiano. Yet, during the years in which Fabiano completed his M.Phil. — the substance of which constitutes Chapter 2 of this book— we also discussed how to approach classic methodological issues such

as the dichotomy of centre-periphery, the question of transmission of knowledge, or the relationship between economic development and the growth of state-funded scientific and technological research (a topic particularly relevant to the history of contemporary Brazil). More generally, we engaged in a debate over the role and use of far-reaching historiographic models within the history of science and technology as it is practised today.

As far as the last point is concerned — the most generic, indeed, and usually the least discussed — I personally find it very difficult to convey to graduate students, let alone to convince colleagues, that the conscious or unconscious deployment of theoretical models is almost inevitable in all research, but that it does require that the historian increases the level of critical awareness and of reflexivity. There are of course colleagues, and entire sectors of the history of science and technology community, who are convinced that the theoretical work, the elaboration and critique of broad theoretical tools of analysis and frameworks of interpretation, is the most important feature of our work. This is quite legitimate, needless to say, and those who have the patience to peruse the indexes of the major international journals in our complex field of research over several decades will not fail to notice that the fascination for successive or competing theoretical and methodological issues has profoundly marked the development of the discipline. It is as if historians of science have for a long time (and many still do today) shared the implicit belief that “Science “ can be reduced only to (in turn) a set of theoretical statements or experimental protocols, of paradigms, of institutional articulations and practices, of social and gender relationships, of literary styles or even of body postures. The preferred model further guarantees that “Science” can be described through discursive practices (our texts) capable of catching its essence, so to speak, or at least of isolating and highlighting those features that can be seen at work anywhere and at any time and which imply that something called (then or now) “science” is being debated or done. Equally, single albeit crucial episodes in the development of contemporary science, such as the so-called Scientific Revolution of the Seventeenth Century, have been narrated from the vantage point of a plurality — at times bitterly competitive — of wide-ranging epistemological, intellectual, social, literary and religious models, each seen by its supporters as the only, single factor capable of accounting for the historic event or moment under consideration. A furious debate was even engaged in as to whether Protestantism or Catholicism could be seen as the key pre-condition for the Scientific Revolution: as if the word “Protestant” or “Catholic” (or “Science”, for that matter) indicated a unified body of beliefs, of social, political and institutional articulations and practices, of assumptions concerning God and His dealings with Nature.

A further feature of the predilection for broad theoretical pronouncements is not so much the descriptive role these are explicitly called upon to play, but the tendency to attach to them a strong prescriptive role. Almost without exception, major theoretical models have claimed and do claim to account for the phenomenon “Science”: almost all sub-disciplinary articulations of the history of science assume “Science” to have existed and to exist still today in one particular form, characterized by essential features that only the new theoretical elaboration being proposed is at long last capable of conceptualizing and of accounting for. Strong theoretical statements appear to function as criteria of exclusion, more than of inclusion. They patrol disciplinary boundaries, and inform the seemingly objective economy of quotations and bibliographical reference; they indicate criteria of belonging, rather than engagement with the subject. Almost all proponents of strong theoretical models, and their followers, like to draw a rather simplified view of the past of the discipline, allegedly dominated by positivistic or idealistic accounts elaborated by rather simple-minded amateurs — as if the history of science had ever existed as a unified discipline, as if, indeed, unified monolithic disciplines have ever existed outside anachronistic, post factum and often self-serving ideological, political or merely academic reconstructions. Strong theoretical statements help to create a sense of excitement and belonging, the conviction that new research horizons are at long last opening up: truly reliable ones, this time. They dispense with tedious and time-consuming engagements with the historiography of the past: they select the primary sources or the issues worth working on, and indicate the kind of examples that would nicely fit the requirements of the model. An almost archaic, highly hierarchical division of labour is implicitly instituted: the theoretician indicates the ways to follow, and by implication the ones to avoid; the Ph. D. student or the aspiring academic will fill in the appropriate supporting evidence, and will perhaps propose slight variations to the model.

I am not arguing that new methodological insights are useless, or that they only answer generational, career or national needs, though it is well known that grant-giving bodies love to be told that the two or three years’ research they are going to fund, framed by radically innovative theoretical insights, will change the face of scholarship and establish the primacy of a given school or research group at global level. But the practical research and career benefits of alignment to strong theoretical models should not obscure the fact that over the last four decades a rapid succession of theoretical options, often owing to the interest the history of science has aroused within a growing number of disciplinary domains — from sociology to literary studies, from geography, economic history and anthropology, to religious, gender and colonial and environmental studies — have indeed contributed new insights into a phenomenon, broadly and uncritically called “science”, traditionally

(but not exclusively) studied for its rarely doubted contribution to the progress of humanity. In more explicit albeit reductive terms, the historical, epistemological, cultural, social, economic and political complexity of the phenomenon we are accustomed to call “science” or “technology” allows, invites and indeed requires a plurality of approaches. Yet, instead of an opportunity, this plurality of approaches is often seen by the devotees of one or the other model as a problem to be faced, which is usually resolved by a dignified refusal to acknowledge each other’s existence.

There is no space here, nor is it appropriate to insist in detail on the fact that the history of science has always been characterized by a plurality of approaches, and that the scholars who devoted their lives to singing the praises of the one or the other great scientific benefactor of humanity have rarely been in the majority. It remains true, nevertheless, that it is only in the very recent past that strong competitive models have been proposed, that new journals and new societies have been established, which often display a seemingly dismissive ignorance of all competitors. The new theoretical, social, and generational configurations of the multiplicity of trends devoted to the study of science and technology (however defined) in history do perhaps explain why theoretical models are rarely seen as heuristic, but tend instead to be presented as endowed with exclusive prescriptive connotations. Theoretical models are very rarely seen as tools or as methodological guidelines to be tested through research: increasingly they take on the much less flexible role of guardians of disciplinary boundaries. Adhesion to or rejection of a given model defines academic and personal relationships, determines the editorial policies of journals, and in many countries influences in fundamental ways the distribution of research funds, or the award of Ph.D. and post-doctoral scholarships.

Not that there were happy days when this did not happen. A Marxist historian of science did not have an easy life in finding a job in departments dominated by Popperians or classic positivist historians of science, and vice versa. If anything, during the long decades of the Cold War, even within the sparsely populated landscape of the history of science and technology, conflicts tended to be bitter, and straightforward militant politics was never far removed from the surface even of those who proclaimed that “science” had nothing to do with “politics”, and never had. Today, the connections to politics of debates within the history of science are less immediate and dramatic: they are in any case much less talked about. The bitterness of some debates, or the *damnatio silentii* inflicted on rivals, often has to do with academic politics rather than with world politics, even though there are academics so genuinely devoted to their own model as to believe that the two coincide. So, our graduate class discussions at Oxford had a lot to do with the feeling that historians of science have to take very seriously indeed the proliferation of theoretical models and

insights, but also that we must never lose sight of the essential difference between the heuristic value of a model, and its prescriptive, almost “degenerative” potential. This does not mean indulging in ecumenical blandness: quite the contrary. Precisely because the history of science broadly understood has very often been guided by a succession of quasi-monolithic theoretical models, and is now subdivided into a series of enterprises guided by models each seen as possessing at long last the key to all doors, it is important to have at least some ideas on why this situation has occurred, and to avoid the easy and often-heard explanation that the supporters of a rival methodology are simply wrong and have therefore to be ignored. Historians of science should simply accept that the highly complex object they try to understand — theoretically and historically — has never existed as such, that is, as a single set of practices. Moreover, a plurality of competitive heuristic models is probably more likely to help our work than a single imperialistic theoretical approach, one that can most surely make the best of a given feature, but at the price of reducing all complexity to the standards of its own prescriptive requirements. To take a classic instance, when during the 1970s sociologists of knowledge launched their powerful attack against Anglo-American positivism and the then hegemonic Popperian epistemology, they were rightly stressing features of the scientific enterprise traditionally ignored by historians of science inspired by classic philosophy of science. Again, when militant feminist theory invested the history of science, the result was undoubtedly a refreshing widening of the kind of questions historians need to keep in mind when researching the episodes in the science of the past they are interested in. More recent examples would take too long to list even in a cursory way. I will therefore only dwell, and very briefly, upon a methodological trend which has recently attracted much attention, in order to highlight the danger of falling into acritical prescriptive reductionism.

This is the renewed interest in objects and material culture in the history of science. There are various shades of opinion on this, and the two issues I put together — objects and material culture — would not be considered under the same heading by many colleagues. In my field of research, the history of the life sciences and in particular the history of evolutionary theories (a subject which is attracting less and less interest in mainstream history of science), attention to collections of specimens, indeed to the almost preliminary question of what constitutes a specimen, is indeed crucial. Colleagues who are concentrating on the study of collections, their moral economy and financial requirements, on the reconstitution of networks of collectors, merchants and users, are providing a deeper understanding of the colonial and global dimension of taxonomical debates in the modern and contemporary world, and are adding extremely important dimensions to our understanding of the history of the life sciences. But they are, at the same time,

risking losing sight of the social, cultural and political dimensions of natural history practices within the societies that supported them. The sociology of collectors is only possible when there is a society or societies in which a sufficient number of individuals are prepared to pay for collections and for what comes with and around them: from specially designed furniture to specialized books and journals, from preservation practices and chemicals to innovative drawing and printing techniques. And why at a given time would members of that particular society be prepared to do so much for natural history specimens, and not palaeontological ones, or drawings and paintings, books, coins, Chinese porcelain? In fact, some collectors kept amassing all of them together well into the nineteenth century, but during the last decades of the eighteenth century, at the level of private collections and of institutional support, specialization grew exponentially, deeply favouring botanical, zoological and mineralogical specimens, especially colonial and exotic ones, thereby conditioning the relationship between demand and offer, with important effects on the market for specimens. Competition within the market for specimens took different forms, one of which — the one many pursued at great cost of time and money — was the production of theoretical works made possible by the superiority of the collection owned by the author. Thus, the distance between the theoretical work and the sale of specimens was often minimal. Theoretical works were an integral part of the business, and they cannot be ignored or relegated to the useless world of ideas. More generally, I contend that ideological debates, as well as theological and philosophical ones, are an integral part of the societies within which they occur, and they serve to position, orient, represent or condemn individual or group strategies. A purely theoretical work claiming universal reach may tell you more about the private interests and social practices of its author than his account books.

If one moves to the wider, and (paradoxically) still little-studied domain of public debates about natural history during the eighteenth and nineteenth centuries, and part of the twentieth, and to the debates on evolutionary theories, one is struck by the fact that specimens and collections obviously played a key role, but that only a fraction of individuals who took part in that complex story ever possessed specimens, let alone entire collections. This is of course still true today, in an age of expert opinion and well-defined disciplinary boundaries guaranteed by the existence of teaching and research institutions. Indeed, how many distinguished participants in the Darwin celebrations of the year 2009 have ever studied a natural history collection (or read Darwin's work, one may viciously ask)? How many debates on evolution, in the nineteenth as well as in the twentieth century, were carried on by people who did not feel they had to know all the fine detail of the matter to speak

their mind? I am taking this example also in view of its paradoxical nature: if it would be impossible if not foolish to deny the role of specimens in natural history in general and in the history of evolution in particular, a history dealing only with material objects will scarcely be reliable and will risk being anachronistic, since it will assume an attribution of exclusive authority to the owners of collections or to the well-established naturalists working within prestigious specialized institutions. This was hardly the case up to the middle decades of the nineteenth century, when even prominent naturalists had difficulty in establishing their authority. As far as the question of evolution is concerned, influential commentators, interpreters and opponents of evolutionary doctrines, often mere readers of natural history works, challenged the authority of specialists in matters they considered to be too important to be left in their hands. Thus, for instance, to study natural history practices and their impact during the decades bridging the eighteenth and the nineteenth centuries by ignoring a priori amateurs and journalists, novelists and religious writers, politicians and educators, amounts in the last analysis to reintroducing positivistic assumptions about the “proper” procedures determining knowledge production, and about which historical actors must be looked at in order to understand the science of the past. What makes matters even more complicated is that the core of participants in natural history debates was not sharply divided into two camps, the experts and the non-experts, since the boundaries between the two groups were constantly shifting, and the number of “experts” in our sense of the word quite negligible.

A large portion of the scholarly production in the history of science has systematically neglected the so-called secondary figures, or scientific traditions that have ceased to occupy the central institutional, social, or cultural space they enjoyed in the past. One may perhaps indulge in a mild exaggeration, and say that almost every new theoretical model has been applied to the same core of individuals, institutions or historical events, whether real or mythical: Galileo, Newton or Darwin; the Royal Society or the Académie des sciences; the Scientific Revolution or the Chemical Revolution. It is as if the Hall of Fame of modern science proved capable of surviving the disappearance of the highly biased and hotly denounced criteria that had created it, that is, the very same criteria each new theoretical model implicitly or explicitly set out to oppose.

Until not so long ago, the same neglect that condemned to oblivion the so-called secondary figures applied to scientific practices within colonial or post colonial settings, or within countries and geographical areas that did not take an active part in the development of the scientific empires of the recent past or of today. The theoretical reflection on centres and peripheries, and the growing number of studies devoted to this issue, as well as the even more recent attention paid to colonial

and global history, have done much to change this long-standing neglect. Yet, it is often the case that scientific practices and their social or cultural articulations are looked at, and selected for sustained study, with reference to what was going on in the “centres” or in the most “advanced” countries. In other words, the seemingly purely descriptive terms “centre” and “periphery” are in fact hiding an impressive series of assumptions, and are powerfully orienting research priorities and choices. Moreover, to name just a few additional difficulties, “centres” are very difficult to define in historical terms, since they are rarely monolithic authoritative proponents of unified views on a variety of areas of research, whereas individuals active in “peripheries” often prove to have been rather shrewd in playing with a variety of “centres” to maximize their local advantage. Peripheries deserve to be studied on their own terms, without assuming a universality of research priorities and values that has rarely existed, if ever, in the so-called centres, against which to judge local scientific practices.

This is the reason why, in my view, the apparently simple issue the present volume seeks to address, concerning the individuals, the institutions, the networks characterizing scientific practices in Curitiba during the middle decades of the twentieth century, has produced so many interesting perspectives. As Fabiano Ardigó argues in his introduction, the perspective he calls “regional history” allows his colleagues and himself to look at local institutions, individual career patterns, political and religious competition and factional strife, without asking themselves whether and to what extent Curitiba was a periphery — and if so, of what — and what it lacked that would have gained it access to the exclusive club of subjects worthy of the attention of the historian of science. As the editor of this collection rightly points out, countries like Brazil pose the further problem of their immense size: the route of regional histories appears therefore as a tool capable of mapping a social, scientific and institutional territory so far almost completely neglected. From their own perspective, the authors in this volume are also questioning current assumptions concerning the development of contemporary science in Brazil, which is seen as linked to the growth of the economy and the foundation of the system of the Federal Universities. This powerful model, it is fair to say, has inspired important studies of scientific development in major towns and States of Brazil, Sao Paulo and Rio de Janeiro in particular. As with all models, however, it should not be taken as the blueprint for all possible stories.

The authors of the essays that follow have certainly avoided the trappings of uncritically applied general models, and the results are in my view fully repaying their collective effort. Curitiba — a small town that reached 120.000 inhabitants in the period under examination, very difficult to reach, in view of the costs of the avia-

tion industry of the time, and of the 250 impossible miles to reach Sao Paulo, the economic and scientific capital of Brazil — hosted an impressive variety of scientific activities, especially within the natural and agricultural disciplines. Individuals active in the town included the prominent entomologist Jesus Moure, who probably had more links with the Royal Society of London and the Rockefeller Foundation than with natural history institutions in his own country; and Reinhard Maack, the German traveller, mining expert, and geologist who was among the earliest supporters of Wegener's continental drift theory, providing as he did detailed comparative analysis of the palaeontology of areas in Parana and corresponding ones in South Africa. This is not to say that Curitiba is interesting because a few individuals active there deserve to be included in the history of science with a capital "S". Indeed, what emerges with fascinating clarity is the first sketch of the portrait of a town hosting a variety of competing lay or religious educational and research institutions, and of research laboratories providing services to agricultural interests, especially logging, coffee and cattle farms. The history of the Parana Museum, of its personnel and publications, its survival strategies and eventual failure due to the complex politics of the State and of the town, is also particularly telling.

It is not my task to enter into the strategies pursued by the various authors, or into the editorial logic that brought this volume together. For this, the introduction by Fabiano Ardigó that follows will amply suffice. It is clear that the tool of "regional history" will prove its full merits when further "regional" studies are carried out. Was Curitiba an exception, or did other regional capitals host the variety of institutions and activities described in this volume? To what extent did regional developments influence each other, and to what extent did personnel circulate through various regions? There are of course many further research questions to be put to the test, and the essays here collected are already answering some of them.

Finally, it is to be hoped that an English-language edition of this volume will one day be made available. Though it is also to be hoped that historians of science will soon realize that there exist more languages than are usual in the English-speaking world.