

Chapter 2

Social Sciences, What for?

On the Manifold Directions of Social Research



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*In fact, nothing is more conservative than science. Science lays down railway tracks.
And for scientists it is important that their work should move along those tracks.*

—Ludwig Wittgenstein (ca. 1946 in Monk, 1991, p. 486)

The following chapter revolves around one simple yet intriguing question: what do social sciences give back to the societies supporting their work?¹ The most common—and certainly not wrong—answer is knowledge: social sciences, through different institutions, artifacts, and practices, provide the social world with knowledge about human and cultural affairs. This knowledge, moreover, ought to be novel, as it is meant to change what was previously assumed—either by revealing new findings or by transforming what is currently taken for granted. Typically, this knowledge should address human affairs that are puzzling or unknown, and therefore demand forms of inquiry other than common sense or first impressions—which, it is assumed, are easily available to non-social scientists. Thus, if we understand social sciences as a counterpart of natural sciences (cf. Dilthey, 1883/1989),² the

¹ It is likely that the acute reader have already noticed that this question naïvely assumes that giving back something is actually desirable for science in general and social sciences in particular—thus disregarding the possibility that scientific endeavors should be pursued with as little worldly constraints as possible while the former is in fact assumed, at this point, the reason behind this is properly elaborated along the chapter.

² While Dilthey's (1883/1989) distinction between human and physical sciences is usually invoked to affirm that both are systematic endeavors for pursuing knowledge (*Wissenschaften*), it must be noted that the crux of this distinction originally was to make clear that human ("soul" = *Geist*) sciences (*Geisteswissenschaften*) and natural sciences (*Naturwissenschaften*) cannot be possibly compared, due to how differently they seek knowledge—*Verstehen* and *Erklären*, respectively.

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former should aim to make the mysteries of the human world understandable, just as the latter have thoroughly explained the natural, physical world over the last three centuries. An apt example of the former line of reasoning is that, as social research has consistently shown, giving cash money to people living in impoverished conditions is not the golden solution to their struggles—as conventional wisdom suggests. The efficacy of such money transfers actually depends on whether they are conditional or unconditional (Manley, Gitter, & Slavchevska, 2013), applied in rural or urban populations (Fernald & Hidrobo, 2011), or whether they are given to male or female beneficiaries (Stampini, Martinez-Cordova, Insfran, & Harris, 2017). In doing so, this set of social science studies makes a contribution to address pressing social issues, like poverty, in a much more efficient way than our lay ideas would allow us to. Or at least so we are expected to think.

Acknowledging that the vision presented above is problematic in many aspects³ is not really difficult. Yet it is hard to argue against its main implication, namely, that social sciences are meant to create valuable knowledge for the societies supporting their work. But even if we agree on this point, the question about what social sciences give back is certainly far from being settled. For the notion of (scientific) knowledge hides behind its noble guise many troubling questions (see Collins & Evans, 2002), which are especially thorny for social sciences: who decides which topic should be investigated—scientific communities, funding agencies, or citizenry? Following which methodologies—quantitative, qualitative, or mixed ones? Moreover, what kind of knowledge is desirable? Does it have to have a concrete impact in the world? Or should it instead raise theoretical problems? Regarding their subject, should social sciences only aim to solve pressing issues? Or should they, on the contrary, aim to raise problems where we assume that there is none? And, last but not least, should the knowledge-making efforts have priority funding over physical sciences, or vice versa?

The existence of these many questions would not be a problem should contemporary social sciences—as corpus of knowledge and collective of researchers—have clearly defined answers to them. Yet the case seems to be the exact opposite, as even different research groups within a single social sciences' faculty or department are likely to have different answers to these questions. If we acknowledge the existence of this confusion, it could not be left just as an open question. Therefore, and as the extensive use of “should” in the previous paragraph makes clear, the discussion presented in this chapter takes the subject of this volume—social philosophy of social sciences—necessarily into moral grounds. More specifically, it draws the discussion into explicitly addressing what kind(s) of scientific knowledge should social sciences give back to the societies promoting their research. In other words, discussing what is the purpose of contemporary societies by supporting the existence and development of social scientific research. I am fully aware that even raising this close connection between scientific activity and social interests is contentious for

³Particularly the idea that social sciences are ultimately supposed to be nothing more than the counterpart of natural ones, as noted in the previous footnote

many readers—as it apparently reenacts the “anti-science” arguments developed during the so-called science wars (Segestråle, 2000), which presented scientific activity as a façade of objectivity that merely echoed dominant social discourses (e.g., Fuchs, 2000). “Pro-science” defenders, on the other hand, claimed that the problems of science precisely start when this activity is tied or restricted by worldly constraints like political or ideological agendas (e.g., Bauer, 2000); and thus science should develop free from any societal limitations. Instead of swinging to any of the extreme positions proposed back in that intellectual quarrel, in the present chapter I approach the science-society relation through the notion of *guidance* developed by Valsiner (2012).

The concept of guidance is, first and foremost, a reminder that crafting scientific knowledge is—and has always been—an activity embedded in very particular social institutions and historical contexts, as the history of psychology in the United States makes extensively clear (Valsiner, 2012; see also Danziger, 1979). Acknowledging the former is an essential step to understand that scientific criteria are not the only determinants for the paths through which scientific communities develop their research—as it is also a response to contingent social demands and events. Thus societies—through several institutions and policies—*guide* the creation of scientific knowledge towards certain areas and topics, *without forbidding*, nor encouraging, alternative developments (Valsiner, 2012). A contemporary example of how science is socially guided is the funding ban that, since 1996, the Centers for Disease Control and Prevention (CDC)—United States’ public agency devoted to address menaces to public health—has to exert over any research projects that aim to address gun-related violence as a public health issue, following the so-called Dickey Amendment (Omnibus Consolidated Appropriations Act of 1997, 1996). In practice, this Congress amendment establishes that no public funding can go to study the causes, prevalence, or consequences of violence exerted by means of a firearm within the United States, as it “may be used to advocate or promote gun control” (p. 244). While this restriction could be assumed as overtly banning that topic of research, it rather states—through an official act of Congress—that the creation of such knowledge represents no value for the country. Therefore, through the funding policy of its public agencies, the United States has guided scientific communities away from studying gun-related violence, thus discouraging yet not censoring research on it. Hence, the pressing need for getting better knowledge on this subject in the United States has had to be almost entirely⁴ pursued through private or international initiatives—as the acknowledgments of existing research on the subject show (e.g., Swanson, McGinty, Fazel, & Mays, 2015). Thus the interest of scientific communities and the public for gaining scientific knowledge on firearm violence has not been suppressed—but discretely silenced (Kellerman & Rivara, 2013).

⁴It is important to note that, following initiatives from the Obama administration, in 2013 the National Institutes of Health (NIH)—also a public agency—opened calls for research projects on the causes of firearm violence, effectively funding two projects addressing the consequences of possessing firearms (for a review, see Rubin, 2016). These projects, however, remain as the exception rather than the rule.

As shown above, the notion of guidance (Valsiner, 2012) is useful to understand that scientific activity is tightly connected to the needs and priorities of societies, but not forcefully subsumed to follow them. Thus scientific development is, so to say, socially canalized towards certain paths while nudged away from others. Such orientation, however, is not necessarily exerted from the outside towards scientific communities, as groups of scientists can also make—and usually do—efforts to push their disciplines towards particular directions in order to favor their social positioning rather than scientific merit (Danziger, 1979). More importantly for this chapter, and as the example presented above makes clear, such guidance is neither static nor univocal. On the one hand, the directions in which different disciplines are guided are in constant change along the time, just as societies' interests change too (e.g., the sudden interest in nuclear physics during the first half of the twentieth century). On the other hand, scientific activity can be guided towards many paths at the same time. In the example presented, United States' funding agencies have driven scientists away from studying the mortal consequences associated to owning firearms, while international agencies (e.g., London-based Wellcome Trust) have specifically promoted the creation of exactly that area of research. Therefore, it is indeed possible to identify a dominant guiding trend, yet it is not possible to conclude that it is the only one at play. Nowadays, social sciences seem to be guided (simultaneously) towards quite different paths by different actors: from being the critical reserve that observes societies from afar (e.g., Crandall, 2017) to the social engineers devoted to address the pressing issues of the twenty-first century (e.g., Western, 2016), to the scholars responsible of bringing forth issues that societies have failed to acknowledge (e.g., Foucault, 1961/2006), and to be devoted to discover the basic laws governing human and cultural affairs (e.g., Werner & Kaplan, 1963; Luhmann, 1984/1995). Thus, if social sciences seem to be lost in their purpose, it is probably because they are trying to be guided, at the same time, towards producing quite different *kinds* of knowledge on different, ever-changing *topics* of social interest. Given this scenario, it becomes essential to hold an open discussion about how social sciences should navigate through these multidirectional, dynamic efforts of guidance, as they could lead social research towards quite different purposes and subject matters. The present chapter aims to provide a proper framework for such dialogue by outlining six dominant directions to which social science has oriented its knowledge.

Holding such a discussion is, in fact, long overdue, in order to avoid the rise of polarization among social scientists that—sometimes inadvertently—hold completely different positions (O'Connor & Weatherall, 2018). Far from any metaphysical reflections, discussing about the former is a pressing need for a group of disciplines that has been called into question for different reasons: from being a self-perpetuating and endogamic community of opaque intellectuals (Sokal & Bricmont, 2003) to being second-class disciplines that should move as soon as possible to integrate within natural sciences (Fitzgerald & Callard, 2015) and to being an interesting yet eccentric way of investing public resources that could be done as long as economic circumstances allow (Campaign for Social Science, 2015). Even if all these critiques could be debated and argued against, there is at least one issue

that should invite social scientists to question their current purpose, namely, the publishing game (Gabriel, 2017). This “game”—probably familiar to most readers of this volume—is well expressed by the motto “publish or perish,” which implies that every scientist, social ones included, has to publish as much as possible in order to develop a career in academia, i.e., getting and keeping a position, as well as receiving funding for research. While at a first glance this sounds as a noble goal, it is muddled by the current high-impact publishing scheme. This model, as described by Gabriel (2017), implies that public-funded research made by social scientists is given for free—or in exchange of *ad honorem* peer review—to private-owned outlets, which charge hefty fees for granting access to read the published work, fees that are usually paid by the same governments that funded the original research. By participating—willingly or not—in this publication model, social scientists have ended up writing increasing numbers of publications (Fanelli & Larivière, 2016), which are kept behind paywalls, and thus available to a very limited range of readers, mostly academics associated to a large-enough institution. Aside from the questionable economics of this model, the need to publish in high-impact, indexed journals (e.g., Web of Science index) pushes social researchers to adjust their scientific work to the topics, methodologies, and formats that those specific journals—i.e., their editors—accept. Not surprisingly, this publishing game has encouraged social scientists to create knowledge for the sake of publishing rather than giving back something of public value to the social world; thus the increased perception of “losing touch” with the world (“Are we losing touch?”, 2018) producing research that could be ultimately irrelevant (CORDIS, 2018a) and even of questionable integrity (Edwards & Roy, 2017). Therefore, having a common understanding of what is that public value is more essential than ever.

While the present chapter is certainly incapable of changing the publishing game described above, it looks to bring forth the discussion of what for are social sciences doing research nowadays. This is done hoping that, in the long run, discussing the latter also makes unavoidable to question the former. In order to open up and organize this discussion, the present chapter develops a conceptual framework that could help its development by outlining and relating existing positions on the direction that social sciences should follow. More specifically, six different positions on what kind of knowledge social sciences ought to create are first presented. As it will be discussed, these stances combine with each other in ways that makes it difficult to neatly distinguish them through the usual theoretical vs. applied lines (e.g., Johnson & Field, 1981; Nafstad, 1982; Roll-Hansen, 2009). Thus it is proposed to group these positions into a single framework composed of three opposite pairs: return on investment vs. value in itself, applied vs. basic social research, and citizen vs. academic relevance. This framework aims not only to identify the orientation of a given set of social science research but also aims to help in finding common ground among these apparently contradictory positions. Finally, I conclude this chapter by sketching ways in which social sciences might reconcile its differences without sacrificing its inherent diversity—emphasizing that doing more research and receiving more funding should be accompanied by being increasingly more reflective on how our own research shapes, impacts, or is irrelevant to the social worlds in which we live.

Return on Investment Versus Intrinsic Value

Despite being typically reduced to one or two lines in the acknowledgments section, funding is an essential and usually painstaking aspect of doing scientific research. Essential as it defines, in very material terms, the scale of the research project that a social scientist might be able to conduct. This ranges from how many research assistants and PhD students could be hired as support, to what kind of materials and instruments are to be available, and especially how large could the scope of the project be in terms of time and participants. Yet the focus here is not the struggles that social scientists usually endure to secure funding but the very natural question that follows from any relation mediated by money: what has to be given by social scientists in return of that money? The question seems especially relevant in a context in which economic analyses reveal that science, in general, seems to be offering “diminishing returns,” i.e., making less groundbreaking results despite increasing many times its funding (Collison & Nielsen, 2018).

On this issue, it is possible to identify a perspective for which social sciences and their research projects are understood as government-funded projects, out of which some form of monetary return is expected (e.g., Willis, Semple, & de Waal, 2018). As the nature of social science projects makes clear, this return on investment is not expected in the same way as natural and physical sciences—for which technological innovations and patenting makes that return easily measurable. Instead, the monetary impact of social research could be assessed only through indirect measures, for instance, by measuring how a community development project contributes to lower crime rates, which in turn reduces police and jail costs (e.g., Mocan & Rees, 2005). Not surprisingly, the issue has been largely discussed by natural sciences, even as the subject of a *Nature's* editorial (Macilwain, 2010), under the subject “science economics.” Yet the same perspective has only been partially applied to social sciences and humanities, pioneered by the IMPACT-EV project (CORDIS, 2018b). This initiative, aptly named *Evaluation, monitoring and comparison of the impacts of EU funded SSH⁵ research in Europe*, was funded between 2013 and 2017 by the European Union Commission, aiming to develop a system of “evaluation concerning assessment of the scientific, policy and social impact of SSH research project outcomes” (2018b, p. 1). Besides its scientific impact, it is particularly interesting to note the assessment of both policy and social impacts. Regarding the former, the project declares that “we will focus on EU directives or recommendations, national, regional and local policies” (2018b, p. 1), while by the latter “we understand results of the policies and citizens’ actions based on research evidence in relation to the five EU 2020 targets” (2018b, p. 1). In simple terms, from this perspective, any social research project should be able to demonstrate that it is capable of making not only a contribution in terms of increasing scientific knowledge but also in terms of advancing social and policy goals.

⁵“SSH” is the common abbreviation of social sciences and humanities.

In opposition to the former, there is a view that depicts scientific knowledge at large—social sciences included—as something valuable per se (e.g., Burawoy, 2007). By having an intrinsic value, the production of scientific knowledge should be pursued without any worldly hindrance, compromise, or limitation—especially regarding economic aspects. The former naturally implies that funding a research project should not imply any form of retribution other than the knowledge produced through it—for instance, research conducted to assess the impact of human rights (e.g., Friedman, 2018). In other words, this perspective considers social research as “invaluable knowledge,” which should not be measured by any standard beyond its own expansion in novel directions—i.e., knowing more about something is sufficient justification for doing (social) science. Far from overstretched, this position represents particularly well the case of social sciences like cultural studies and linguistics, which, in fact, contribute “nothing more” than knowledge. If such knowledge happens to be deemed as less relevant at a particular institution or country due to financial constraints, as it happened at Copenhagen University in 2016 with a series of culture and language programs (see Hedetoft & Hede, 2016), then research and education should cease.

Summarizing up to this point, it is possible to identify one position arguing in favor of making social sciences accountable for their contribution to social improvement. This contribution, moreover, should be measured through some form of quantifiable index—ideally of economic nature—in order to demonstrate that doing research is money well spent. On the other hand, there is a position claiming that social sciences should not be asked to give back anything other than pure knowledge as the expansion of scientific, social knowledge holds an intrinsic value—that goes beyond any money-related concern.

Citizen Versus Academic Relevance

Connected to the former, but pointing into a different direction, is the elusive issue of the relevance of social science research. Elusive as defining what research topics and methodologies are considered relevant is something highly contingent to particular scientific communities (see Lave & Wenger, 1998) and sometimes even to particular scientists (see Polanyi, 1962). This variability is itself expressed on the different disciplines composing social sciences. For instance, how exactly can we decide if a social- or group-level analysis of discrimination is more relevant to hold? Since this issue by far exceeds the scope of this chapter—although it deserves further discussion—here I narrow the discussion to the source of relevance, i.e., *who* should be the arbiter for determining what is going to be considered as relevant social research, rather than discussing *what* should be deemed as relevant and irrelevant. Thus, it is possible to recognize the existence of two main sources of relevance: the local—and global—community of social science scholars that currently review and circulate this work or the communities that are the subjects of study and/or intervention by social scientists.

The first position identified—i.e., placing scientific communities as the ones that should decide what is relevant or irrelevant (social) research—represents the current state of affairs. In fact, declaring this position as the dominant one is something easily verifiable: nowadays, any piece of research that aims to be considered as proper social science has to be published through a peer-reviewed journal or book. Therefore, it is peers, i.e., fellow scientists acknowledged by the community, the ones who deem a work as relevant or irrelevant for that particular community. Here it could be argued that if a given journal does not accept a work for review—due to any reason—there are many more available that could receive it and eventually publish it. While this is correct, it rather reinforces the main point, namely, that scientific communities, through their many different established publishing outlets, are the gatekeepers for what is socially accepted as scientific research—since if no journal editor receives a given work, and peer reviewers accept it, this work cannot properly become a scientific piece. Similarly, most research funds have some form of—typically blind—peer review systems, which makes the argument also applicable to them. Thus, as long as fellow scientists act as *bona fide* reviewers, peer reviewing is a fair system for which it is difficult to think in a better replacement. More importantly, it does help to keep in charge researchers that have dedicated years to study a subject, which also avoid external interventions like political, religious, or other forms of censorship.

The former approach, despite being the dominant one, has nonetheless been criticized as endogamic and authoritarian precisely for the isolation from the social world that provides to scientific communities (e.g., Feyerabend, 1975). In fact, it is not hard to see that placing scientific communities as the only gatekeepers for deciding what is, and what is not relevant for social sciences is quite risky—due to the inherent perverse incentives involved in doing so. Among these risks, it stands out the possibility of communities abusing their privileged positions in order to promote certain topics of methodologies not due to their scientific merit but to put their own work in a good light (see Latour & Woolgar, 1979). Thus, it gives these works ample acceptance and diffusion while keeping its critics out of the spotlight. Whereas the former is borderline with scientific fraud and probably unlikely to happen on purpose, it notwithstanding points to the classic analysis of Kuhn (1962) about how reluctant scientists are to accept radical innovations on their field. Besides the former, there is also the risk of turning social research basically self-centered, i.e., making social scientists focus their work on issues that are plainly irrelevant for the citizens and societies that support their work. Against this, it is possible to think social sciences as the ones providing knowledge on the issues that societies demand to know, for instance, providing empirical and conceptual research that makes possible to understand emerging problems like cyberbullying (e.g., Mishna, McInroy, Daciuk, & Lacombe-Duncan, 2017), fake news spreading (e.g., Vosoughi, Roy, & Aral, 2018), or the impact of widespread use of social networks (e.g., Buglass, Binder, Betts, & Underwood, 2017). Despite the allure of these topics for citizens, they might as well be of no real interest for social scientists due to several reasons—like being virtual iterations of well-known face-to-face phenomena. While this mismatch between academic and citizen relevance does not have to always be the case,

it has been a recurring critique towards critical and gender studies (e.g., Deutsch, 2007), as well as research on basic psychological processes (e.g., Epstein, 2016). The main counterargument for this position, as stated above, is the risk of turning this citizen involvement into veiled forms of censorship to the work of social scientists. Yet the opposite case also seems dreary: social research that has lost any touch with the social world, except for extracting information from it, in order to answer questions that matter to social scientists only.

Summarizing this second pair of opposite positions, it is possible to see that the issues of relevance for social sciences could be—in practice—defined in two ways: as the latest trend in the preferred scientific journal or congress and as the latest hot topic hitting news headlines and social media. At the end of the chapter, I will present a middle way between these extremes.

Applied Versus Basic Social Research

Placing this classic tension within social sciences as the last one is no coincidence. On the contrary, I do this to show that the usual debate between applied and basic⁶ research, which usually concentrates most of the academic discussion, is in fact only a fraction of the conversation about the knowledge created by contemporary social sciences. It is certainly not a minor part, but it is not the whole picture either. In this context, this tension invokes the long-standing discussion of whether social sciences should aim to ameliorate the ever-increasing number of social problems (from drug addiction to domestic violence) or rather focus on discovering the underlying principles of human and cultural phenomena.

The first view on this holds that social sciences should only conduct applied research, i.e., devoted to create innovative solutions for real-world problems, just like engineering or material sciences do. While this might sound as a cartoonish comparison, it has some historical grounding. In this vein, a notorious example of how social research could be oriented towards the betterment of society is the case of Cora Bussey Hillis, a housewife and mother who, inspired by the child study movement led by psychologist G. Stanley Hall at the end of nineteenth century, advocated for establishing a research station at the University of Iowa devoted to research child welfare (Valsiner, 2017). In brief, “[h]er argument was simple but compelling: If research could improve corn and hogs, why could it not improve the rearing of children?” (Cairns, 1983 in Valsiner, 2017, p. 84). While not a social researcher herself, Bussey Hillis captured the gist of why social sciences should be oriented to conduct applied research above all else. In brief, social sciences ought to provide knowledge that easily translates into viable, efficient solutions to pressing problems. Accordingly, any form of general knowledge derived from applied

⁶ Here I chose the term “basic” instead of “theoretical” since, as it will be explained shortly, I consider the latter to be an aspect of the former.

research would be desirable, but neither expected nor crucial—as the development of intelligence tests for selecting US Army personnel clearly shows (e.g., Tuddenham, 1948). Here it is important to distinguish this orientation from the previously described “return-on-investment” position. As noted, the latter is indeed concerned with how social research contributes to the concrete betterment of society, yet this can only be pondered in relation to the money cost involved. Therefore, even if social research is capable of being applied to address real-world issues, this would have to be done in a cost-efficient way that makes it competitive against other forms of social investment.

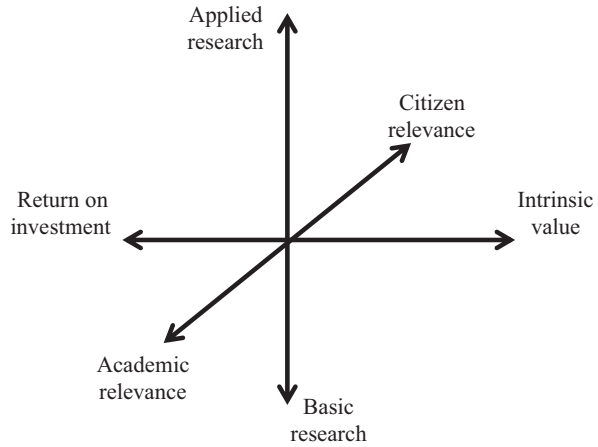
At the same time, it is possible to find the opposite perspective, namely, that social sciences should focus in different forms of basic research. The main rationale for this approach is that social sciences should not necessarily aim to impact the world in the short term but to provide novel ways of looking at and understanding human affairs. In this sense, the argument goes, any applied research that frames an issue inaccurately would ultimately lead to little or short-term impact rather than substantial change. Interestingly, when applied to social research, the term “basic” could be understood in at least two ways. The first of them is centered in raising awareness about current circumstances that, despite not being perceived as such, are in fact relevant social issues (e.g., Foucault, 1961/2006). This kind of basic research, typically conducted through conceptual analysis, looks to unravel situations that are easily assumed as problematic by the society, in order to show what is not being perceived. A case in point is youth delinquency, which at first sight could be seen as a problem of misbehaving teenagers, but upon further analysis it might be seen as even a rational decision when confronted with extremely unequal environments (e.g., Mocan & Rees, 2005). In a similar direction, the other variant of basic research looks for general principles that apply to understand multiple social phenomena (e.g., Luhmann, 1984/1995). Here, of course, it is possible to find multi-volume theoretical treatises that have shaped the understanding of social disciplines (e.g., Geertz, 1973). But, at the same time, it is possible to find works that explore basic human and cultural processes through a combination of theoretical and empirical work (e.g., Werner & Kaplan, 1963) or exclusively through an empirical approach (e.g., Bock, 1966).

In sum, the main positions regarding the applied or basic character of social research are clear: either research is conducted to directly solve pressing social issues, or it takes a full step back from the social world, in order to reflect about it without directly meddling being involved in its events and discussions.

A Common Framework

Having outlined the six contemporary positions on what should be the kind of knowledge that social sciences should give back to the societies supporting their work, it is now necessary to organize them in a single framework in order to avoid leaving them as a plain list. As described above, the aim of this framework is providing a simple

Fig. 2.1 Framework of the six main criteria for social scientific knowledge



tool that helps at recognizing how different subdisciplines and research areas within social sciences are currently pointing towards very different directions regarding their purposes (see Fig. 2.1). This diversity, which is inherent to the manifold guidance (Valsiner, 2012) that social sciences experience, is not a problem in itself—and it could very well be a characteristic that reflects the long-standing co-existence of different research traditions (see Cornejo, 2005). Yet it does become problematic when this is neglected by social scientists, thus making them assume that their own particular view on the purpose of social research is the only possible or reasonable view—as it seems to be the case at the beginning of the twenty-first century. In this scenario, it is only natural to find social scientists that consider the work of other social scientists as irrelevant, purposeless, or even worthless. Helping to clarify this kind of misunderstanding is one of the purposes of this framework. Yet it is not the only one, since acknowledging that social sciences could gravitate to different directions is something necessary, but not sufficient. Thus, this framework could also be used to go beyond diagnoses, specifically to look for *common ground* among these different approaches. This is acknowledging that the positions identified are extremes of a continuum, and, as such, they could also be complementary rather than mutually exclusive. The last part of this chapter will further discuss this specific point.

When looking at the visual organization of the three opposite pairs described in the previous section, it seems reasonable to go back to the very distinction that this chapter looks to avoid—namely, an “instrumental” perspective (composed of applied research, citizen relevance, and return on investment) versus an “idealistic” perspective (aligning basic research, academic relevance, and intrinsic value). Yet I argue in favor of dividing them into three axes rather than merging them into two neatly separated opposites. The main reason against this traditional separation (e.g., Johnson & Field, 1981; Nafstad, 1982; Roll-Hansen, 2009) is the fact that it does not match the diversity of social science research. Thus, it is possible to find several examples of contemporary social research that simply does not fit in the

instrumental-idealistic dichotomy. For instance, action-research projects—as in communitarian psychology (e.g., Carollo, 2012) or social work (e.g., Healy, 2001)—are, by its nature, applied research that operates under citizen relevance. They, however, are prone to reject any association with any form of a return-on-investment mentality, as its value relies on developing local communities. On the contrary, human-computer interface studies, for instance, usually address basic processes (like perception or attention) with the goal of improving the development of different forms of computer software (e.g., Raptis, Iversen, Mølbak, & Skov, 2018). Despite its basic focus, these studies are usually conducted for making software more engaging and intuitive to the user, in order to increase the profits of companies that develop software, thus going against the intrinsic value logic that they should have under an idealistic view. Similarly, social and economic studies regarding the impact of information in decision-making (e.g., Martínez & Dinkelman, 2014) are developed with a strictly applied mentality, as they aim to inform the creation or improvement of public policies. Yet their relevance typically comes from academic rather than citizen environments—since determining at what point in time is information more beneficial is rarely an ordinary concern—which once again breaks the mold of fitting into either instrumental or idealistic alignments.

The former examples, as it is clear, break the classic applied vs. idealistic distinction—and have certainly been picked for showing this. While this does not imply that any form of social research could fit within the classic model, it does intend to make clear that it is an insufficient approach; insufficient for understanding how many different elements combine at the moment of defining the kind of knowledge that social science create. But especially insufficient for finding common ground among different directions, rather than polarizing their differences—as the current basic/applied divide has done. In the following section, I outline ideas for what this common ground could be for social sciences.

Concluding Remarks: Finding Common Ground

In this chapter I have explored the role of contemporary social sciences; more specifically, I have discussed what do these sciences give back to the societies that make their research possible. While the short answer to this debate is “knowledge,” this points to a much larger and overdue conversation: what *kind* of knowledge are social sciences expected to craft. Since this dialogue necessarily touches upon the—sometimes hotly debated—science-society relation, I proposed to approach this issue through the concept of guidance (Valsiner, 2012). In brief, this concept emphasizes how the work of scientific communities is constantly tried to be guided by society towards certain directions, while pushed away from others. This guidance is not necessarily an external influence that coerces social scientists—as this could also be done from within scientific communities (Danziger, 1979). This concept, moreover, makes clear that rather than a single direction of guidance, societies usually try to canalize social research towards many different—and even opposite—directions.

Therefore, when thinking through the notion of guidance, it possible to understand that social sciences could have more than one expected outcome—thus making possible to analyze the many kinds of knowledge that social sciences are currently giving back. It is in this context in which the six positions previously described were organized into a single, three-axis framework (see Fig. 2.1). Through the analysis of the three opposite pairs of positions, it became clear that the classic distinction between applied and idealistic research does not meet the diversity of contemporary social science research. Moreover, by presenting a single pair of opposites, it has not contributed to create a common ground in which this diversity could co-exist collaboratively rather than competitively. In the following, I sketch ideas on this.

In order to find how the quite different positions presented could co-exist—and collaborate—it is necessary to look closely at some of the distinctions made, in order to determine whether they are really opposite or possibly complementary. Regarding the first axis presented, return on investment vs. intrinsic value, it is clear that any initiative framing the contribution of social science only in terms of improving numeric indicators, particularly economic revenue, is a certain way to alienate a major group of social scientists from their own work. On the other hand, pretending that social sciences “owe nothing besides knowledge” to the societies that make their work possible is not only questionable but also hardly tenable in practice. For any form of social science, knowledge relates with the social world outside academia either we want it or not; in other words, the days of patronage are largely over. Therefore, the question at stake is how to establish a meaningful relation for social science and society. For this it seems essential to compromise on both ends of the spectrum. On the one hand, there is no need for social scientists to suddenly become experts in calculating the social profitability of research projects, yet it seems necessary to acknowledge the need of defining parameters to assess whether the knowledge created is making the expected impact or not. On the other hand, it is important to keep in mind that, as researchers, crafting scientific knowledge is the gist of our activity. This, however, should not be an excuse for pretending to be locked in an ivory tower—from which knowledge runs downstream to lay people, but never in the opposite direction (cf. Schütz 1932/1967). The former is, interestingly, tightly connected to the second tension discussed. The issue of relevance undoubtedly calls for an open dialogue between scientists and citizens rather than swinging into one of the positions presented, as neither the current academic isolation nor playing the role of social consultant properly fits with social science that produces research not only *about* people but also “with people in it,” in the words of Ingold (2000). Here the ACCOMPLISSH initiative,⁷ backed by the EU program Horizon 2020, offers—at least on paper—a viable framework for keeping social sciences and humanities grounded into social worlds from they emerge. Finally, the third dichotomy, despite being the most traditionally assumed as such, is not necessarily a contradiction. This is because basic and applied social research could be seen just as different stages in the research process rather than separate—or even

⁷<http://www.accomplish.eu/#accomplish>.

opposed—categories. While academic and administrative organizations tend to promote such separation (e.g., innovation centers), this could be done otherwise. In this sense, working within research networks in which different teams lead different angles of a project—basic, applied, and intervention—could put together apparently antagonistic stances. The former, however, requires that social scientists think beyond their own comfort zone, being open to establish bona fide dialogues with colleagues who have different orientations and interests.

As stated at the beginning of this chapter, its aim is to open a discussion on what kind of knowledge should social science give back to the societies that make their research possible. And thus it would be pointless to close this work by providing an answer that I consider as definitive to this debate. This is why, as I have tried to make clear, my aim has been twofold: making visible the necessary distinctions for holding such a discussion and showing that among the sometimes-contradictory diversity of social science knowledge, it is possible to find ways to reconcile these manifold paths into common ways of development.

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