

Scientific collaboration: the very idea

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Prologue

In our societies science has become more and more a collaborative practice. From solving global challenges that require vast amounts of diversified data to managing research at levels of complexity (both theoretical and technological) unthinkable only a few decades ago, doing science collaboratively seems the way ahead. This entails not only collaboration among different scientific communities but also between scientific and extra scientific communities. In both cases science is faced by the challenge of making different, often distant, sometimes incommensurable languages and cultures communicate with each other in view of pursuing a common goal.

The challenge of course varies depending on whether either different categories of scientists are involved, or scientists confront themselves with categories of purported ‘epistemic agents’ outside the realm of science. While the former type of challenge has been well analysed and discussed (for example, by the likes of Peter Galison since the 1980s)¹, the latter has been less explored in philosophy of science – with the exception of those who turn their attention to those specific forms of scientific practice that go under the names of community-based science, citizens science, etc.²

In this essay I will look at some of the conditions and values that are required for a ‘virtuous’ collaboration to succeed in practice, in particular when such practice entails communication across types of knowledge that do not necessarily fall under the generic connotation of ‘scientific’. I will use, as my example of collaboration, the controversial relation between so called ‘local knowledge’ and scientific knowledge (which I will call ‘general’ to draw a suitable contrast with ‘local’) and look at ways by which the two can integrate for the better of research.

Terminological preliminaries

What is ‘local knowledge’? The expression can be unpacked in at least two ways. According to a first meaning, local knowledge is *knowledge of local facts*. ‘Local facts’ do not correspond to either what in the received view are called ‘initial conditions’ (as in Hempel’s D-N model), nor to ‘particulars’ in the sense of instantiations of a general law or theory (all iron bars melt at 1538°C; this particular bar is a piece of iron; this particular bar will melt at 1538°C). They rather correspond to any situational range (and arrangement) of contextual empirical factors and assumptions allowing laws and theory to be ‘fitted out’ on different concrete occasions that go beyond what laws and theory can predict by means of the way they are formulated (as general cases).³

¹ See Galison (1997).

² The list is much longer: participatory science, public science; crowdsourcing; participatory action research; public participation in science and research; public participation in scientific projects (Heigl et al., 2019), civic science (Kruger & Shannon, 2000); do it yourself science (Nascimento, Pereira & Ghezzi, 2014), street science (Corburn, 2005), crowd science (Franzoni & Sauermann, 2013), etc.

³ They are kinds of *ceteris paribus* conditions not as an indistinct category of clauses to be kept at bay when applications of laws and theories occur (and for them to apply appropriately), but a precise category of conditions that has to be worked out every time a law or a theory is called upon in an explanation or a prediction. For the ‘fitting out’ of theories see Cartwright (1999), 39.

According to a second meaning, local knowledge is *knowledge possessed by the locals* (local people, groups, communities, etc.). It has recently been defined as ‘place-indexed knowledge’ (knowledge of a place here and now), often overlapping with ‘place-bound’ (knowledge that entails a “sense of pertaining to communities that occupy a particular historical and cultural place”), and ‘place-based’ (knowledge “grounded in a culturally identifying world-system”).⁴ This knowledge is not scientific, it is not formalized and very often not formalizable in a science textbook, nor is it discovered by scientific method. Yet it is in its own way highly specialized, i.e. not easily accessible, or at least it is accessible only to those who share the traditions, experience and history of, say, living in a particular community. For these reasons, it is either outright neglected in specific cases, or more generally disputed in terms of credibility.⁵ Both attitudes could have a severe impact. They might lead to a disregard of the practical (not only logical) consequences of the acceptance or rejection of a theory, hypothesis, claim, which should instead be factored in when evaluating the evidential reasoning pro or con a theory, hypothesis, claim.⁶ As A.Wylie aptly pointed out, making local knowledge part of the decision-making processes that in different ways and at different levels concern the communities that possess such knowledge is an epistemic obligation (towards the way science is practiced and the results it can achieve), not only a moral one (eg. to the affected communities).⁷ This entails disputing the ‘credibility deficit’ that often tarnishes local knowledge and prevents its inclusion and use in scientific research. The result is helping eliminate epistemic injustice as a fallacy of reasoning, not only as a moral vice.

There is a case to be made for scientific and local knowledge to integrate with each other. By means of such integration an enlargement of the research agenda might occur, to include types of evidence and interpretive resources that would not naturally get considered by research conventionally pursued. This is said not only in the sense of learning new facts but also learning more about the ways facts can be learnt and brought to scientific attention.⁸ Of course, this is not a plea for an unruly acceptance of any sorts of diverse perspectives. Quite to the contrary, it is a plea for establishing mechanisms to assess what counts as credible and relevant knowledge, without confining ourselves to conventional forms (along the lines of Longino’s process of ‘critical scrutiny’).⁹ How can we formulate conditions for integrated collaboration to be possible in practice? What idea of collaboration is at stake?

A preliminary couple of pointers regarding collaboration are in order. First, in a collaborative situation, the emphasis is on working together towards shared goals and holding shared responsibilities towards the goals. In that, collaboration is different from cooperation (where the emphasis is more on individual performance vis a vis a vis other, often competitive individual performances in situations where the goal is to achieve the best/most convenient possible output for all, even if not the

⁴ See Massimi (2025), 10-14.

⁵ I have analysed a case of neglect in the context of the Vajont dam disaster, e.g., Barrotta-Montuschi (2018). A well known case of disputed credibility has been analysed by Wynne (1996).

⁶ As the vast literature on inductive risk teaches us.

⁷ See for ex. Wylie (2015); and Wylie (2014).

⁸ As also remarked by Wylie (2014).

⁹ See Longino (1990), in particular ch.4.

best/most convenient for each individual).¹⁰ Secondly, in order for a collaborative group to work successfully together, the group must embrace a variety of single contributions, even and particularly when these entail diversity (not necessarily conflict) among them within the group itself. Acceptance of such diversity within a situation where goals are shared by different contributors amounts to an endorsement of inclusiveness. Inclusiveness does not automatically lead to collaboration. It is a guiding principle that, if implemented appropriately, leads a group to pursue collaboration as a means to achieve the shared goal.

What counts as ‘appropriate implementation’ in the context we are interested in (the collaboration between scientific and local communities)? There are three dimensions that ought to be considered – epistemological, methodological, ethical.

According to an *epistemological* dimension, differences in knowledge contributions should be included in such a way that reliability of specific contributions and relevance to the shared goals are secured (at least in ways that increase their probability). As I mentioned above, acknowledging that local knowledge might have epistemic weight and value does not per se vouch in favour of an unqualified acceptance of any local knowledge. We are not advocating a view whereby some knowledge, just by virtue of being local, is good knowledge. It has still to pass both the test of relevance and that of evidence – and at least in the second case the testing might require means and protocols that do not strictly speaking comply with scientific research as ordinarily practiced.

Here is where the *methodological* dimension becomes relevant. How can we test local knowledge when it takes the form, say, of experiential, know-how, lived experience, or tacit knowledge, or tradition/community-based beliefs, or anecdotal evidence? etc. Surely the standards and tools routinely used in scientific empirical practice are not of much help in assessing the content and credibility of these types of knowledge, and to some extent it would be wrong to submit them to these types of tests. We should instead be bold enough and admit the independence of these types from conventional forms and sources of knowledge testing. The latter, treated as merely technical tools of knowledge acquisition, and demonstrating their worth by design rather than context and purpose, often do not tell us the whole story (or the story that matters in specific circumstances). Strategies of ‘critical scrutiny’ à la Longino, as mentioned above, are to be put in place to perform a double-level task: first, direct our attention to background assumptions and constraints (epistemic, cultural, practical, ethical) and to different sets of values pertaining to standards and protocols of research; second, put different communities of knowledge bearers on equal footing when it comes to accept, at least in principle, their epistemic worth (the scrutiny apply to all sources of knowledge).

‘Directing attention’ and ‘putting on equal footing’ are not only part of a methodological task. As noted before, excluding epistemic perspectives ex ante is both epistemically damaging and morally objectionable. And here is where the third dimension of inclusion proves its worth. From a *moral* perspective, inclusion of different types of epistemic agents is the right (equal, just, fair) thing to

¹⁰ It could be observed that good collaboration also entails cooperation, and that cooperation can occur also at group level.

do, as inclusion presupposes acceptance of the view that knowledge is not the prerogative of only one type of agent, but it is distributed over a range of possible candidate agents, each with their own specificities and sets of priorities and rights.

Inclusion and integration

Inclusion is different from assimilation. It does not aim at eliminating diversity but rather to embrace plurality. What do I mean by this? A real collaboration entails being able to capitalise on the differences of the contributions to knowledge. In the literature on interdisciplinarity/ transdisciplinarity¹¹ a word that is often used to describe the relation of disciplinary interaction is 'integration'.

There are different sorts of integration, more or less radical in their effects in terms of assimilation. Holbrook (2013), in questioning how communication is made possible across disciplines, distinguishes three types of integration. He focuses on disciplines within the academic domain. However, some of what he says points us in a suitable direction when it comes to the case we are interested in (local knowledge – extra academic domain).

One variety of integration, that Holbrook names *the Habermas-Klein thesis*,¹² aims at generating common understanding, or as Habermas puts it, aims to "bring about an agreement that terminates in the intersubjective mutuality of reciprocal comprehension, shared knowledge, mutual trust, and accord with one another." (Habermas 1998, 23; quoted in Holbrook 2013, 1869) Bringing about an agreement, in the habermasian framework, is reaching consensus. Reaching consensus is not an easy task to achieve. Miscommunication abounds in most communicative action, and there is no method for securing the final result (agreement or consensus) other than putting forward conditions that allow all involved parties to sit at the same negotiating table. Consensus is an ideal to aim at for integration to emerge, in this version. However, the accent is ultimately put on differences being settled and overcome, on the assumption that a common ground can be created for positions to meet. This makes integration akin to some form of an *assimilation* of positions that sacrifices the preservation of diversity.

A more challenging variety of integration is what Holbrook describes under *the Kuhn-MacIntyre thesis*. Here the complication comes from the fact that the various disciplines are deemed incommensurable, so communication is possible only if "one learns the language of another discipline from within as a second-first language." (Holbrook 2013, 1871) In this case, integration does not aim at consensus. The need for integration stems from realising that one discipline does not have all the resources required to address or solve a problem. For that reason, appealing to another field/perspective becomes advisable. The strength of this thesis lies in the idea of preserving the differences among perspectives, and the aim of integration is not assimilation or sameness, but some sort of justified interaction among different perspectives.

¹¹ For a concise definition of inter and trans disciplinarity and their differences, see Toomey et al (2015).

¹² Holbrook calls each variety of integration by the names of the authors that he deems relevant to the formulation of the features singled out by each variety. I will take each label at face value.

A third variety, named by Holbrook *the Bataille-Lyotard thesis*, also takes its cue from the differences among disciplinary perspectives but focuses on how much people are/should be willing to sacrifice in terms of their disciplinary identities, and how open should they be to the possibility of co-creating new shared languages in order to achieve successful communication. This comes close to what P. Galison once called the production of pidgin or 'creole' languages (blends of different languages) in the metaphorical space of a 'trading zone'.¹³ The production of such languages identifies the possibility of achieving some local agreement (securing ad hoc communication) despite maintaining global differences (respecting disciplinary differences). I will return to this point below and further elaborate.

So far, the issue of integration has been discussed in the context of disciplines, that is from within a broadly conceived academic background. What happens when integration extends to extra-academic fields? Part of the transdisciplinary literature takes this aspect on board.¹⁴ As noted in Koskinen-Maki (2016), this literature builds on systems theory and a Mode-2 concept of knowledge,¹⁵ as well as on post-colonial research (Nowotny et al. 2001; Hirsch Hadorn et al. 2008; Pohl 2008; Zierhofer and Burger 2007, referred to in Koskinen-Maki, 421). Mostly, however, this literature consists of empirical case studies and transdisciplinary projects. It provides little normative and conceptual analysis of issues such as integration. The aim is to get integration done, or see how it is done, rather than discussing what it conceptually requires to succeed and what it entails. Conversely, when such discussion does occur – as in some pluralistic approaches in philosophy of science – the optimistic picture of integration as an achievable goal in practice often painted by some transdisciplinary literature becomes more nuanced and problematic. Koskinen and Maki specifically analyse this contrast of assessments by comparing some relevant literature from both sides. In what follows I take the cue from their analysis, and elaborate and expand it in view of supporting my argument.

Integration, in practice and in theory

Starting from the transdisciplinary literature side, integrating academic and extra-academic knowledge appears particularly relevant when what is at stake is solving a policy-relevant problem, or more generally some problem that involves some pressing real-life challenge for a range of interested actors. These problems are complex, in that they reflect the interests and purposes of different stakeholders. For this reason, they require all the available relevant knowledge coming

¹³ Literally: real situations, identified by anthropologists, in which different peoples are able to exchange goods, despite differences in their language and their culture. Metaphorically: scientists from different paradigms and different communities find a way to collaborate/coordinate with each other despite their differences. See Galison, (1997), 783.

¹⁴ For a definition of transdisciplinarity (TD) I follow here Holbrooke (2013): “the integration of one or more academic disciplines with *extra-academic perspectives* on a common (and usually a *real-world* as opposed to merely academic) problem.” In the same article Interdisciplinarity (ID) is defined as “the *integration* of two or more disciplines focused on a common (and it is sometimes insisted, a *complex*) problem.” (ibidem, 1867)

¹⁵ The notion of Mode 2 knowledge production was first introduced by Gibbons- Nowotny in Gibbons et al. (1994). It is knowledge produced in the context of application by means of transdisciplinary, interactive and socially distributed collaborations. It complements Mode 1 knowledge, which is located principally in scientific institutions and produced by means of specific scientific disciplines.

from disparate sources (Pohl et al. 2008). Integration becomes then that ‘core methodology’ that is responsible for bringing together diverse societal actors and bridging different perspectives (Pohl et al 2021; Bammer et al., 2020; Hoffmann, 2016; Jahn et al., 2006; McDonald et al., 2009), or otherwise called, ‘thought-styles’ (Pohl et al 2021, 20). One way of describing integration in TD research is the following:

Integration (...) does not mean pieces of knowledge added up to a simple sum of the parts or a coherent and comprehensive whole. Rather it is a metacognitive process by which participants may reshape their mental representations of a concept or domain when brought into close contact with different views. (Pohl et al. 2021, 22; referring to Kestra, 2017)

Of course, integration, viewed as such, brings with it a challenging aspect. How can participants reshape their mental representations? How can different, sometimes distant, knowledge systems and beliefs meet and appreciate each other? The TD literature pays attention to the contexts and conditions where these questions emerge. Social-interactional factors facilitate integration, such as emotional qualities (respect, admiration, recognition, trust), meaningful personal relations, group identity, complementary team roles, socializing outside meetings, and group working styles and routines (Boix Mansilla et al. (2016), 589, quoted in Pohl et al. (2021), 23). Generally, a ‘climate of conviviality’ is conducive of successful integration where people are willing to learn from each other (Pohl et al 2021, *ibidem*).

Clearly, the focus is on the pragmatic and practical dimensions of integration, and the aim is that of creating a shared, combined framework of sustainable understanding. But how about viewing the challenge of making distant beliefs and mental representations meet and integrate from an analytic/conceptual perspective? If we look at some pluralist approaches in philosophy of science, we might be inclined to think that, in order to integrate, we do not necessarily try to overcome, or ‘tame’ differences.

In depicting his own version of scientific pluralism,¹⁶ H.Chang specifically refers to integration as one of the forms taken by interactive pluralism (the other two being ‘co-optation’, and ‘competition’ – Chang 2012, 279-284). According to interactive pluralism different systems of knowledge, or different approaches and perspectives, should not just be tolerated in science, in the sense that they should not just be allowed into the realm of scientific practice to pursue their own aims in their own ways. More proactively, they should be made to ‘interact’, because science can only profit from such interaction. By means of illustration Chang recalls a comparison, once made by C. Pierce,¹⁷ between a cable and a chain: “a chain is only as strong as its weakest link”, whereas “(a) real cable (...) is actually stronger than its strongest strand, due to the productive interaction between the strands” (279).

¹⁶ There is a wide debate about pluralism in philosophy of science, and different varieties of pluralism and of pluralistic philosophers of science. I will not enter this debate, but just recall what from this debate proves useful to highlight the features of integration that are necessary to my argument. For reviews of positions and critical assessments about pluralism see, among others, Kellert et al. (2006), and Ruphy (2016). Chang’s own version of pluralism is named by him “active normative epistemic pluralism”. See Chang (2012), ch.4.

¹⁷ The quote is from Peirce (1984 edition, 213).

Among the forms of productive interaction discussed in the literature on scientific pluralism Chang singles out integration. One version is S. Mitchell's.¹⁸ She preaches in favour of integration in describing biological systems, such as communities of insects. These are so complex that they could not be described by any monistic perspective. Integration of different explanatory models is required, on an ad hoc basis, to offer an appropriate account of these systems. Integration must be ad hoc, in order not to transform the co-existence of different models into yet another unificatory strategy (all models ultimately converging into one). Equally, the co-existence of models does not aim at straightforward isolationism. Each model can be an accurate and yet partial representation of a natural object, phenomena, or fact, so that an appropriate explanation entails at most a strategy of integrated compatibilism, and never full-fledged unification.

In her version of integrative pluralism Mitchell focuses on integration of the same types of tools (mainly explanatory models). In a different version of pluralism attention is paid to integration when different types of activities and tools are in the picture. P. Galison uses the term 'intercalated' to explain how integration of different scientific strands (theory, experiment, instrumentation) occurs in physics.¹⁹ Here the pluralistic aspect of integration is given particular emphasis. By means of a number of examples Galison points out how individual strands develop independently of one another, and even when they converge, none of them "lose their separate identities and practices." (Galison 1999, 137) Intercalation is a form of coordination without homogenization, Galison writes (*ibidem*, p.136), and most importantly, it occurs locally. Local integration is possible even when strands are very far apart – even incommensurably so. For example, during WWII,

[T]heorists, experimentalists, and engineers were forced to work with one another in the large wartime projects. They emerged with nearly five years' experience of each other's way of approaching problems and an enduring faith that postwar science had to exploit the collaborative efforts that they credited for the atomic bomb and radar. In large part the collaboration consisted of establishing a place where ideas, data, and equipment could be passed back and forth between groups. (Galison 1999, 149)

During these exchanges the different groups did not change their identities, or their own approach to work. They just learnt to work together in view of a locally shared aim. And, as Chang puts it, "(a)t each point in time, the integration between the strands has to be worked out anew, as each strand develops independently". (Chang 2012, 280)

Integration, in pluralist philosophy of science, has been discussed also in the context of values. Philosophers of science traditionally paid attention not only to so called epistemic values – values that affect the acceptance of scientific theories and practices – but also to social and cultural values – values that shape the environment where science is performed and pursued. There has been a long-standing debate as to whether the latter should be allowed into scientific practice (should science be value free?), and when they are allowed, in what form and to what extent.

¹⁸ See for ex. Mitchel (2002).

¹⁹ See for ex. Galison (1999); and Galison (1997), ch 9.

There are different views about the presence of non-epistemic values in scientific practice. Some radical views (e.g., the sociology of knowledge, STS studies, etc.) argue that all values are social, including the epistemic, the consequence being that the choice of a theory or the meaning of scientific terminology is said not to be determined only, or maybe even primarily, by methodological constraints internal to science but by social utility and relations. This is argued to the extent that there is no way to make any distinction between epistemic and non-epistemic values in science. (Bloor 1991; Knorr-Cetina 1983; Latour 1987).

According to more moderate views, it is acknowledged that social inquiry is embedded in social contexts, and yet theory choice is dictated neither entirely by the social, nor by the data or the logic of science. Longino, for example, argued that the gap between theories and data (as described, for example, by the so called underdetermination thesis) is to be filled up by a range of assumptions that also includes values, of both sorts. So she claims:

The general lesson of underdetermination [is] that any empirical reasoning takes place against a background of assumptions that are neither self-evident nor logically true. Such assumptions, or auxiliary hypotheses, are the vehicles by which social values can enter into scientific judgment. (Longino 2004, 132)

This of course changes the ground of discussion regarding social values. They are not seen either as an obstacle to scientific research (as by the supporters of the value freedom thesis) or as a substitute for epistemic considerations (as by various representatives of STS studies), but as “a rich pool of resources – constraints and incentives – to help close the gap left by logic.” (Longino 2004, 133) If science is after truth, truth on its own is not enough to guide scientific research. Choosing the questions, setting the goals, discovering facts and entities, are mediated by a wide array of social values that make the pursuit of truth operational in research contexts. This makes the integration of social values necessary to the practice of scientific inquiry, without integration becoming a form of translation or radical embeddedness (of the epistemic into the social). Different categories of values ought to maintain their identity and independence in performing the role that is expected of each category.

Kitcher develops this view further, by suggesting that the integration of scientific values with social values is required to give direction to scientific inquiry. The social values Kitcher (2011) focuses on are typical of democratic/liberal societies: freedom and equality. When appropriately developed, he argues, these are the values that should be endorsed in order to save science both from vulgar democracy (based on people’s impulses or ignorance) and from elitism (either internal to science, i.e. originating from scientific subcommunities, or external, i.e. including not only scientists but also the ‘paymasters’, those who fund science). Translated into science these values become on one side, freedom of inquiring and debating openly and inclusively, and to the extent of sometimes even challenging well-established paradigms; on the other, an equitable division of labour when it comes to scientific knowledge, which includes the right to participate widely in scientific inquiry and to benefit from scientific results. In particular, scientists should actively engage with

citizens and make sure that science pursues results that are socially relevant. This is what an ideal *well-ordered science* (Kitcher's own expression) in a well-functioning democracy should strive for.

Tangled integration

A more recent take on the idea of integration can be brought forward in the context of what in Cartwright et al. (2022) we called the 'tangle' of science. In this book we argue that what makes for the reliability of scientific products is the close, cohesive interaction of different strands and tools of scientific activity and practices that come together in view of serving specific purposes. There is much more going on in science than producing scientific claims and theories and confirming their truth. There is a vast array of products created by science (models; measurement definitions, procedures, and instruments; concept development and validation; data collection, analysis, and curation; experimental and non-experimental studies; statistical techniques; methods of approximation; case studies; narratives; etc.; etc. – all listed in Cartwright et al 2022, 3), and not for all of them do questions of truth apply. And yet each should be accredited with reliability if the final outcome is to be reliable. Accrediting reliability is different from warranting truth. It requires a vast and complex body of work that allows a scientific product to do its job (including confirming truth, where truth is at stake). We call this accrediting body a 'tangle'. We define a tangle in the following terms:

...the rich interwoven net of scientific creations that constrain and support each other—the concurrent, mutually feeding back and forth, developing network of ideas, concepts, theories, experiments, measures, bridge principles, models, methods of inference, research traditions, data and narratives, etc. etc. that make up a scientific endeavour, with its long tentacles out into other similarly rich tangles that it rests on and that can in turn rest on ingredients from it. (Cartwright et al. 2022, 5)

And as most philosophers, we use a favourite metaphor. A tangle is like a Jacana bird nest floating on water, built to support its precious eggs. It is made of twigs, stems, and leaves, expertly interwoven in such a way that the resulting structure is solid and firm. A few twigs might go amiss, but enough must stay, and in the right position, so that the nest does not collapse. Tangles that work (which we call 'virtuous') are like reliably built Jacana bird nests: rich (with lots of different kinds of twigs to bits of grass and weed), entangled ("the pieces relate to each other and to the product in a variety of different ways, and these are the 'right' ways for the job" – Cartwright et al. 2022, 6), and long tailed (they reach other tangles, themselves engaged with other scientific products in other domains of works, interacting successfully with the empirical world).

This is a clear illustration of how integration works. Although we do not use the term, what we have in mind is precisely this idea of independent products, belonging to different domains, preserving their distinct identity, yet connecting with each other in view of a shared aim. One of the examples we discuss in the book is the discovery of penicillin. In his (2007) book Robert Bud writes:

Penicillin was a biological product whose manufacture would require the integration of a variety of scientific and engineering disciplines . . . Expertise in bacteria had to be linked to experience with moulds, chemistry, and the engineering of sterile systems for the support of living creatures. (Bud 2007, 23).

In the book we offer, among other examples, a revisited historical description of this discovery in the light of the tangle analytic tool. Penicillin was initially (with Fleming) just the object of bacteriological studies. After a decade from the initial work, it became the crossroad for a wide variety of methods, materials, experiments and data from other disciplines. In particular, if a substance discovered as the product of a mould is to function as a pharmaceutical remedy, then the context of its original discovery must engage with scientific products embedded in tangles that go well beyond the field of bacteriology. Moreover, in this specific case, given the strategic importance acknowledged to the substance in the contingent treatment of large numbers (infected soldiers during WW2), the methods of production were to meet the exigencies of public health and health policies, adding crucial social and political dimensions to the story. New techniques were developed (e.g. deep tank fermentation) to secure production of large quantities of the substance, and ad hoc collaborations among countries, e.g. US and UK, were sealed to supply stocks for the Allied Army Forces. The overall tangle structure where this type of integrated work developed appears long-tailed and rich: it required a large team of experts in various branches of science (pathology, biochemistry, medicine) and a variegated pool of stakeholders (scientific institutes, governments, drug companies). We can even claim that, depending on the disciplinary angle, penicillin is not a single scientific product, but many:

In Fleming's bacteriological work, it was mould broth. In Chain's biochemical work, it became a freeze-dried powder. Later, in therapeutic trials, it took the form of either a powder to apply locally or an alkaline solution to give systemically. Today penicillin is understood as a family of antibiotics whose members differ in their effective routes of administration. (Cartwright et al. 2022, 192)

To this we can add, from a public health angle penicillin is the 'magic bullet' that by killing a microorganism while leaving the host unaltered would show its potential to cure and possibly save millions of people.

This proves how the plurality of approaches and applications in the history of penicillin identifies this chemical substance as what Leigh Starr and James Grisemer have dubbed a 'boundary object.'²⁰ It results from the intersection of different tangles of investigation which, each in their own individual ways, need each other to make the study of the substance develop and progress. 'Penicillin' is a common point of reference that yet entails layers of differences.

²⁰ 'Boundary objects (...) have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation. The creation and management of boundary objects is key in developing and maintaining coherence across intersecting social worlds.' Star-Grisemer, (1989), 393.

In Cartwright et al. (2022) we do not expressly raise the issue of how the different tangles communicate with each other, nor how collaboration among different knowledge bearers takes shape, other than assuming that, once a common goal is recognised, efforts are pursued to make the means meet the end in the context at stake. I suspect that if prompted on this issue we would align with a Gallisonian type of pluralism: in the presence of a variegated range of scientific and extra scientific products, called upon to serve a specifically identified common goal, we ought to opt for local integration while respecting global differences.

Integrative collaboration

Let us return to our initial issue: why is clarifying the meaning of integration important when we try to identify what role collaboration ought to play in scientific research?

From the parallel discussion of integration in the context of inter/trans-disciplinary communication, and in that of pluralism in philosophy of science, we infer the following three points:

1. Single perspectives might not (and often do not) possess all the resources needed to address or solve a problem – scientific or other.
2. Integrating perspectives, in a non-weak sense, entails respect for differences.
3. Differences can run deep (incommensurability challenge) and should not be bypassed (assimilated, translated, unified, etc.) in view of embracing generalised consensus.

All three points hints at an important further aspect:

4. When communication and exchange of information across differences become the aimed-at focus, the means to achieve proper integration among perspectives does not entail even well-designed attempts at translating one perspective into another, or homogenizing them. Back to Galison, proper integration is pursued by co-creating new ‘creole’ languages where different perspectives come to converge in an ad hoc, specifically identified, space of shared action and locally identified implementation.

I switched to the term ‘perspectives’ rather than just sticking to ‘disciplines’ on purpose. When we move from the field of interaction among fairly well identified disciplines to the wider, less defined domain of forms of knowledge production, integration does not recede as an issue, and arguably it is still subject to the three points outlined above.

In the case of integration of the kind I focus on here – that between scientific/general and extra scientific/local knowledge – the four points listed above apply as follows:

1. Scientific knowledge does not possess all the resources needed to address or solve problems which arise in specific contexts (and neither does local knowledge). The issue of applicability of the general to the particular requires attention, and so do the differences in nature and role attributable to scientific and local knowledge.

2. Integrating scientific knowledge and local knowledge entails respecting the differences between the two. Neither neglect nor assimilation are the right procedures to adopt.
3. The differences between scientific knowledge and local knowledge run deep (they often speak through incommensurable languages) and their individual contributions should be acknowledged by avoiding any attempts at reducing one (local) to the other (scientific).

Regarding the fourth point listed above, devising a space where scientific and local contributions can fruitfully interact might require the adoption of some 'trading zone' where domain specificities are mutually recognized and respected, and communication between domains is preserved. In the particular case of scientific vs. local, a corollary is to be added. In order not to commit epistemic injustice, integration becomes on one side a specific responsibility (some kind of duty) for the bearers/supporters of general scientific knowledge, and on the other a right of the bearers of local knowledge. In other words, integration should be framed so that it can serve the purpose of avoiding injustice (both epistemically and morally).

What at least in part prevents integration is the more or less explicit endorsement of a particular picture of science, based on a clear separation between the scientific community (the experts) and the public (the non-experts), and on the implicit endorsement of some form of epistemic elitism. As I tried to argue in this essay, this picture is faulty, and epistemically it leads to errors of reasoning and value. If we want to implement an appropriate form of collaboration (appropriate in the sense that it leads to avoid the errors pointed out) this, I argue, should be pursued by electing integration as its leading value.

Conclusions

In the light of the analysis here conducted, what does pursuing integrative collaboration add to or change in the way science is practiced, and why should an emphasis on diversity and plurality be kept at the core of this way of practicing science? Let's start from the second part of the question.

I said at the start that acceptance of diversity within a collaborative group amounts to an endorsement of inclusion as its leading principle. How can we justify this claim?

Leonelli (2023) defines epistemic diversity as "the condition or fact of being different or varied in ways that affect the development, understanding and/or enactment of knowledge." (Ibidem, 29) Difference or variation has an epistemic impact on how knowledge is acquired and validated. Difference is predicated at a number of levels, thereby producing different effects: there is difference at the level of research sites, at the level of epistemic agents, at the level of the sources of evidence – just to name some. Besides, difference entails an important qualitative connotation. For example, when evidence takes into account different sources, the result is not just a juxtaposition of the different sources in terms of highlighting their proximity or contrast. The ensuing evidence becomes itself a more complex, richer notion, pointing at features that are not often plainly reduc-

ible to the original sources. They rather emerge almost anew from a productive, cross-checked interaction among sources.²¹

Inclusion changes the way science is practiced, for the better. How? To the question ‘what does a practice of integrative collaboration change for science?’ I can offer, in conclusion, a preliminary general answer. Integration, in the way it has been described in this essay, is foremost a democratic value. It speaks for equality, as well as freedom – in the sense advocated by Kitcher. A proper acknowledgement of integration makes science fitter for itself (as a knowledge producing activity) and for a type of society where such values are well supported and followed.

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²¹ In the already cited Wylie 2014 we find evidence of how multiple lines of collaborative practice in archaeological research, including archaeologists and descendant communities, push the epistemic boundaries of classic pictures of knowledge production, and make us rethink for example the fundamental question of ‘who owns the past’.

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