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Scientific Experts and Citizens' Trust: Where the Third Wave of Social Studies of Science Goes Wrong

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According to a familiar approach, in cases of technological decision-making – i.e., in those cases in which the subject-matter of political deliberation presents a scientific element as one of its essential features – a clear-cut distinction should be drawn between the technological and scientific moment, on the one hand, and the socio-political one, on the other. First of all, the former is taken to be conceptually prior to the latter. Secondly, the technological and scientific moment is understood as being up to the experts: their aim is said to be the quest for facts, the search for an explanation, the construction of reliable technological tools, and so on. After that, once reliable technological tools have been produced by the experts, the socio-political moment takes the stage for the purpose of setting policies to implement: society, through its representatives, asks the experts about the means to reach the ends and values established by the society itself. The clear-cut distinction between techno-scientific and socio-political moment therefore goes hand in hand with the distinction between facts and values. More precisely, it is the epistemological soundness of the fact-value dichotomy which grounds the idea that in any well-conducted policy the two moments should be kept separated.

Such conception has been strongly criticized by the proponents of Social Studies of Science (henceforth, STS). They argue that the alleged objectivity of scientific experts is a myth, and, consequently, that there is no sound epistemic reason to trust them. In reality, the facts on which their knowledge is ultimately based are constructed by society. The task of sociology of knowledge is precisely that of deconstructing those facts, for the purpose of showing the role played by social values and interests in the process of their constitution.

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The growing de-legitimation of scientific experts has reached such a point that some sociologists of science – born and raised within the STS paradigm of research – have realized that that tendency should be countered. This is the aim of the Third Wave of Social Studies of Science, launched by Harry Collins and Robert Evans. The Third Wave, which Collins and Evans champion, aims at defending all the values, truth included, which define science as a specific "life-form". As they remark in their recent book *Why Democracy Needs Science*: «We desperately need to preserve the moral imperative that guided science under Wave One», that is, under the traditional image of science as a distinctively epistemic enterprise (Collins and Evans 2017: 77). They argue that, even though it should be acknowledged that the claims of scientific experts to provide society with reliable knowledge have proven unwarranted, it is nonetheless possible to defend the importance of their role in society on moral grounds.

The goal of the present essay is to analyze and criticize Collins and Evans's view. We argue that, no matter how ingenuous it might be, their proposal is highly debatable. Their argument relies on the assumption – which we firmly reject – that the objectivity of scientific knowledge presupposes a foundationalist epistemology, as a consequence of which an epistemic account of expertise cannot be advanced. We disagree on this point, and we also believe that a defense of the notion of expertise on purely moral grounds is both descriptively and normatively unsuccessful, and should therefore be rejected.

We start off with a conceptual consideration. Collins and Evans think that the notion of expertise should be viewed as substantial. They argue that being an expert is a property that a person possesses independently of the fact that he or she is acknowledged as an expert. On the contrary, we adopt a relational account, centered on the notion of trust, which paves the way for the distinction between scientists and experts. We agree with Collins and Evans that being a scientist – being a scientist by *profession* – should be treated as a substantial notion. However, we maintain that the *status* of expert necessarily implies a relation to a group of persons who choose to trust that particular scientist as a reliable source of knowledge. A scientist becomes an expert when she is trusted by a group of laypeople.

Such relational account, inspired by pragmatism, enables us to vindicate the intrinsically epistemic character of the notion of expertise. Laypeople do not consult experts because they are willing to preserve the moral values embodied in the latter's form of life, but rather because they have some reason to trust them as reliable source of knowledge in light of

the particular problems at stake in technological deliberation. The notion of trust so conceived cannot be defended on purely moral grounds, but has to be accounted for in strictly epistemic terms. Our approach, centered on the Deweyan idea of problem-solving, aims to show that it is possible to defend an epistemological conception of expertise without relapsing into the traditional – and rightly criticized – dichotomy between techno-scientific and socio-political moments.

In the first section of this article, we lay out Collins and Evans's Three Waves of STS. In the second section, we discuss and analyze their moral defense of science and expertise. In the third section, we criticize Collins and Evans's view, and we challenge the tenability of their position. Finally, in the fourth section, we present our pragmatist account of expertise, and we argue that the notion of expertise cannot be understood apart from the notion of trust.

1. *Three Waves of Social Studies of Science*

In numerous articles and books, Collins and Evans have suggested dividing the history of STS in three great moments or waves. Among other things, those three phases represent three different ways in which the relationship between science – and expertise – and democracy can be framed.

The First Wave, which corresponds to the dominant paradigm prior to the 60s, was characterized by the belief that it was possible to provide an epistemological justification of scientific inquiry. The First Wave believed it was possible to single out clear-cut criteria of demarcation separating science both from non-science and from other human activities such as politics or propaganda. Science was characterized as that enterprise exclusively concerned with the discovery of truth, and it was understood as free from moral and social values. Consequently, the theories advanced by scientific communities were taken to be genuine instances of knowledge. On this basis, Wave One was able to offer a simple and straightforward account of the relation between science and society. Since scientists are exclusively concerned with the discovery of truth, their activity is not burdened with social biases and moral prejudices. The fact that scientists are value-free entails, therefore, their reliability as experts. The judgments of scientists are exclusively responsive to how things are in themselves, as a consequence of which citizens are justified in trusting scientific experts. Even more radically, on these bases there is no sound reason why citizens

should not trust them. Trust from citizens is not something that scientists should earn; it is a by-product of the methodological assumptions of their disciplines.

Wave One's conception of science is no longer believed: it strikes us as naïve and over-simplistic. Starting from the seminal book of Thomas Kuhn, *The Structure of Scientific Revolutions*, both philosophers and sociologists of science have turned their attention to the non-epistemic factors that make scientific knowledge possible. The distinctive feature of Wave Two is the harsh criticism of science's demand for objectivity, and its motto is «distance leads to enchantment». When the curtain is raised, and the actual behavior of scientists is empirically investigated, it is easy to see, so the argument goes, that knowledge is less the result of a confirmation of theories by evidence than the outcome of rhetorical strategies of persuasion. Discovery of truth is therefore nothing but a misleading name for the social process of negotiation of what counts as valid within a specific community. As Collins and Evans remark: «under Wave Two, science is eroded as non-scientific values encourage new kinds of behavior. […] The view associated with Wave Two is that the truth of the matter cannot be found, that there are only interpretations and perspectives» (Collins and Evans 2017: 108 and 40).

In this scenario, there is no reason why citizens should trust scientific experts. Indeed, what goes under the name of scientific knowledge is made of the same stuff as political deliberation. Science is loaded with moral and social values; consequently, scientific experts are in no better position to tell citizens what ought to be done. Science and politics are negotiation and compromise through and through: «science is politics pursued by other means» (Latour 1983: 168).

The Third Wave of STS launched by Collins and Evans aims to counteract and defuse the most radical conclusions reached by Wave Two. In particular, it aims at defending the role of scientific experts in democracy (whence the title of their book, *Why Democracy Needs Science*) without relapsing into the naïve image of science formulated by Wave One. The most interesting aspect of Collins and Evans's proposal is that they believe that the distinctive values of science can be preserved within the Wave Two approach. They formulate this insight by saying that while Wave Two «showed that Wave One was intellectually bankrupt», Wave Three should be seen as a development and refinement of Way Two rather than as an attempt to reject its premises (Collins and Evans 2002: 240).

Wave Three agrees therefore with Wave Two on almost everything the

latter has said about the nature of scientific knowledge (Collins and Evans 2017: 11). They both hold that science is not value-free, that scientific knowledge should properly be seen as the result of a process of social negotiation, and that scientists do not have any privileged access to reality. The only point of divergence between the two concerns their normative position. While Wave Two leans towards more democratization, Wave Three purports to reintroduce a set of distinctions, on whose basis «to preserve the idea of expertise as specialist knowledge and to find a better way of analysing and managing the trade-offs between expert authority and democratic accountability» (Collins and Evans 2017: 11).

So, they remark, «Wave Three involves finding a special rationale for science and technology even while we accept the findings of Wave Two» (Collins and Evans 2002: 44; quoted in Collins and Evans 2017: 100). In order to do so, Collins and Evans distinguish between two different problems, that of legitimacy and that of extension. Wave Two was mainly concerned with the problem of legitimacy: its goal was to show that, once it is acknowledged that «the apparently neutral and objective advice provided by technical experts cannot have the unquestionable epistemological authority it claims», a more reliable procedure can be achieved if a «wider range of perspectives and experiences» is allowed to be represented into the decision-making process (Collins and Evans 2017: 13).

On the contrary, Wave Three is concerned with the problem of extension, which was left unanswered by Wave Two. Indeed, the latter has merely shown that more "subjects" than the experts are legitimated to participate in the decision-making process; it has not addressed the issue of the scope and limits of participation. To properly answer this question, a normative stance has to be adopted, which provides criteria for inclusion and exclusion, and, in doing so, also settles once for all the problem of legitimacy. As Collins and Evans remark:

> [T]he solution to the problem of legitimacy is also the solution to the problem of extension: all the "right" people will have a say in the technical debate, and those who have no relevant specialist expertise will contribute as citizens participating in existing democratic institutions without pretending to be, or being described as, experts (Collins and Evans 2017: 14).

However, in order not to betray the spirit of Wave Two, those criteria cannot be epistemic. A different route must be taken.

2. *Collins and Evans's Moral Defense of Expertise*

It has been said that the Third Wave of STS aims to preserve the idea of expertise as specialist knowledge, and that the argument in support of this view cannot be epistemic. Wave Two, Collins and Evans write, has shown that there is nothing special about science: as a consequence of that, they notice, it is now very difficult to defend science «on the grounds of its truth and utility» (Collins and Evans 2017: 19). However, it is possible to take a moral road, and defend science on the grounds of its contribution to the values of a community. The key point here is to acknowledge that, even though it is true that science cannot reach truth, the values that it embodies and exemplifies are nonetheless eternal.

It is not easy to find in Collins and Evans's work an explicit formulation of the line of thought that is supposed to warrant that thesis. In some passages, they seem to derive it directly from the fallibilistic view of science. In particular, they seem to maintain that according to fallibilism – which is commonly held as the standard position in philosophy of science – no foundation of our best scientific knowledge can be provided, which entails that there is no sound epistemic reason to trust science. Put in this way, the argument is untenable: the rejection of foundationalism and the consequent adoption of a fallibilistic perspective do not amount to discharging any possible form of objectivity. Epistemologically speaking, this is a *non sequitur*. We will therefore try to outline a plausible argument that, we believe, could be accepted by Collins and Evans as faithful to their intentions. Only after having clarified the argument, we will go on to criticize their position.

As said, Collins and Evans's starting point is the thesis of the fallibilistic nature of science. We have also remarked that that thesis is not strong enough to directly support the conclusion that they would like to draw from it. But let's put the matter in another, slightly different way. First of all, assume the validity of the pessimistic meta-induction. According to this view, since all the scientific knowledge that was taken as true in the past has been later shown to be false, we should have the humility to admit that our best scientific theories will very likely turn out to be false in the future. Indeed, there is no evidence that current scientific theories are substantially different from the ones believed in the past; so, they may well share the same fate. At the end of the day, Newtonian mechanics seemed correct for so long, and yet has now been shown to be false and has been replaced by Einstein's theory of relativity.

Pessimistic meta-induction is a highly questioned concept, and is far

from being uncontroversial. Nonetheless, it is not wholly implausible, and can be argued for with some success. Note that, in order for the argument to be consistent, it is necessary to take pessimistic meta-induction in its more radical form, as excluding truth-approximation. Indeed, if contemporary scientific theories turn out to be less false than their predecessors, it would be still possible to introduce an epistemic element in the context of evaluation: the epistemic value of avoiding error is almost as important as that of reaching the truth. For the sake of this argument, we will also assume this radical version of pessimistic meta-induction as plausible, even though we are very dubious about its soundness.

The second assumption of this argument elaborates on the first, and can be formulated as follows. We know that past scientific knowledge turned out to be false, and we also know that current scientific knowledge will turn out equally false; nonetheless, we still hold science dear, and we are ready to defend it from the vicious attacks of its opponents. So, for instance, we are willing to defend evolutionary theory against the claims of Creationism, even though it is very likely that both are false. It follows therefore that the reasons why we are led to defend and safeguard science are not epistemic, since we are committed to its preservation within our society independently from its truth.

This second assumption seems more plausible – at least *prima facie* – than the first one since its content has a strong and unquestionable factual component. It reports that a significant number of citizens in Western societies are ready to defend science against those who are willing to deny its importance for our form of life. This is a sociological – i.e., empirical – fact, and, consequently, we as philosophers take it for granted. Collins and Evans might be ready to say that if the plausibility of the first assumption is admitted, the truth of the second one is hardly questionable, since the latter can be seen as a corollary of the former. We do not agree with them, but we will postpone the examination of this issue until the argument has been settled.

So, it may be asked, if this account is correct, what is the rationale behind our choice in favor of science? The last step of the argument is the core of Collins and Evans's theoretical proposal – which they label "elective modernism". They maintain that since we are interested in preserving science even in the absence of sound epistemic reasons, the motives of our decision should be of a different kind, namely, of a moral kind. More precisely, the reason why we are interested in having science in our society is that the life-form of science embodies values which we want to preserve. According to Collins and Evans, contemporary Western societies are

undergoing a progressive erosion of their distinctive values, in great part as a consequence of free-market capitalism. In this scenario, they argue, science is one of the few remaining fortresses of morality.

Science ceased therefore to be conceived of as the privileged source of knowledge, and become the «fountainhead of values» (Collins and Evans 2017: 19). But what are these values that are deemed as worthy of preservation? Collins and Evans are explicit that they do not have a new list of values to propose; they rely on Merton's classical analysis, and translate the latter in terms of the notion of "formative aspirations of science". "Formative aspiration of science" is a heuristic tool which refers to the set of normative constraints that should be satisfied in order for an action to count as a *scientific* action, and for an individual to count as a *scientist.* The notion of aspiration highlights the fact that an individual need not be successful in satisfying those constraints; indeed, this would be a too restrictive condition. It is enough that her actions are guided by the values of science: these are *observation*, *corroboration*, *falsification*, and the Mertonian norms of *communism, universalism, disinterestedness*, and *organized skepticism.*

Because no epistemic justification of those norms is believed to be possible, Collins and Evans do not attempt to demonstrate their validity. They are content to appeal to the moral conscience of the citizens of Western society, asking them whether they prefer to live in a society in which expertise is respected and defended, and information is shared, discussed, weighted and criticized; or rather in a society in which no distinction is drawn between experts and lay people, and information is kept in the hands of the few.

It is clear, and it is difficult to disagree with Collins and Evans on this point, that we prefer to live in the first type of society. Nonetheless, if elective modernism is true, our preference turns out not to be grounded on proofs and demonstrations: epistemic justification makes way for moral persuasion.

3. *A Criticism of Collins and Evans's Moral Strategy*

Up to now, we have limited ourselves to reconstructing Collins and Evans's argument. It is about time to assess its validity. In this section, we will list some objections to their proposal, for the purpose of showing why we take it to be seriously flawed. These objections will then pave the way for the formulation of our pragmatist account of expertise, which will be outlined in the next chapter.

The first objection is epistemological, and is concerned with Collins and Evans's criticism of foundationalism. Collins and Evans seem to hold that any possible form of objectivity – no matter how it can be conceived of – is essentially interwoven with the foundationalist project, to the effect that in order for knowledge to be true, it must be grounded on some indubitable set of principles or data. We do not have enough space here to delve into a detailed analysis of this issue, so we limit ourselves to a sort of sociological remark. Foundationalism is now hardly a mainstream position in the contemporary philosophical landscape, but, nonetheless, attempts to come up with a consistent theory of objectivity are a daily occurrence. This is due to the fact that foundationalism and objectivity of knowledge are distinct concepts: at best, foundationalism is one of the manifold ways in which the objectivity of scientific knowledge can be accounted for. Things are much more complicated than Collins and Evans think they are.

The second objection is directed against the conclusion of the argument, i.e., the idea that science can be defended on purely moral grounds. Two points are at stake here. First of all, Collins and Evans seem to commit a fallacy of abstraction. In general terms, the latter takes place when a certain complex phenomenon is investigated and analyzed from a specific point of view, and the results of the investigation are identified with the whole phenomenon. In the case under discussion, it is evident that science can be investigated as a form of life, and it is also evident that it is possible to single out some values as distinctive of scientific activity. There is nothing wrong in treating the values of science as formative aspirations of its practitioners; similarly, it is completely legitimate to defend those values on moral grounds. Nonetheless, that does not mean that the moral dimension can be severed from the epistemic one, and taken as autonomous.

Secondly, Collins and Evans are not content to sever the moral aspect of science from its epistemic dimension. They also place the two aspects in contrast with each other[1]. In doing so, they consciously refuse to employ epistemic resources to strengthen their argument. It goes without saying

[1] For the sake of fairness, it should be noted that Collins and Evans do not rule out the possibility of an epistemic defense of science. They are clearly not committed to it, but they also argue that their elective modernism makes room for this kind of approach. They suggest to read their proposal as adding a second arrow to the quiver of those who are interested in defending the role and function of science in contemporary democracies. It is nonetheless very difficult to see how this is actually possible, since Collins and Evans are explicit in rejecting the validity of the epistemological analysis of science. In our opinion, their concession sounds less like a genuine theoretical option than a rhetorical *captatio benevolentiae*.

that theirs is a bold choice; however, its consequences are puzzling. Here is what Collins and Evans write about their moral defense of the value of observation: when it is said that those who have observed something in a systematic way are «[a] *better* source of opinion that those who have not», they remark, the italicized better «cannot mean "more efficacious"» since «if it did we would have a foundational justification»; consequently, they conclude, «[better] does not mean better at anything, it just means better» (Collins and Evans 2017: 20).

As is evident from this quotation, Collins and Evans argue that there is no epistemic reason why we should prefer observation over mere guessing; the only sound reason is that we should prefer to live in a society were people do observations, are skeptical about their conclusions, are open to discussion, and are willing to falsify their beliefs at certain occasions. That preference is moral; it has to do with the way in which we would like to conduct our lives. It has nothing to do with the epistemic credentials of those acts.

But is it truly so? Is the picture of science that Collins and Evans draw plausible? Part of our perplexities are related to, or depend on, the epistemological confusion that we have criticized above, so we won't repeat them once again. But there is something more to it. Let's take the idea of the fallacy of abstraction seriously. It seems clear to us that we have good reasons to prefer observation over mere guessing, and we agree with Collins and Evans that some of these reasons are moral. After all, scientific observation is grounded on the virtue of carefulness, which is a trait of a reliable and responsible character. Since observation is evidence of a good character, we prize it, and we are ready to defend that activity on moral grounds. Such entanglement of the epistemic and the moral is not problematic for our argument: we accept it unhesitatingly. The point is: are these moral reasons as autonomous from the epistemic ones as Collins and Evans would need them to be in order to justify their conclusion?

We think not. Imagine a strongly counter-factual situation in which, because of radically different laws of nature, observation did not have any epistemic value. Suppose, for instance, that the past continuously changed in ways which were unpredictable to us. Consequently, what we have observed at t cannot count as evidence at t' because things are now different from how we saw them. In this case, would observation be defended as a moral value? It seems that this can hardly be the case. From a genealogical point of view, it is very difficult to believe that mankind would have developed a genuine interest in observation if the latter had been completely ineffective. At the end of the day, if inspecting the viscera of birds

had proved itself a reliable method to forecast the future, human beings would have continued to consult the haruspices for information. The fact that observation has been preserved in the course of evolution seems therefore unaccountable unless we have recourse to epistemic values. Nonetheless, Collins and Evans are committed to what we may call the thesis of the dispensability of the epistemic. Accordingly, that move is not open to them: they are compelled to use exclusively moral resources. Honestly, we do not see any possible way out of this predicament.

However, it is fair to remember that Collins and Evans do have at least one other argument in support of their conclusion, which should – or is at least intended to – corroborate the idea of the dispensability of the epistemic. Here is their argument.

Elective modernism is concerned with technical decision-making. Consequently, it is at this level that the validity of Collins and Evans's approach should be properly assessed. Now, when the focus of analysis is shifted from scientific research to technical decision-making things change dramatically. Indeed, in the case of technical decision-making, experts are asked to answer questions that are urgent and decisive for society, without having time to do further investigations and defer their answer. It is a fact that when they have to act under these conditions, experts are often wrong: the opening pages of Collins's book *Are We All Scientific Experts Now?* provide an impressing overview of the errors of experts, from mad cow disease to 2008 financial crisis.

Technical decision-making shows, therefore, that the traditional, epistemological image of science is a myth. From a strictly epistemic point of view, scientific experts are not as trustworthy as we may want them to be since there is strong empirical evidence that they make a lot of mistakes. In addition, the consequences of such mistakes are not confined to the laboratory, but affect the lives of thousand and thousand of people.

Despite all of this, elective modernism wants to defend the positive role of science in society. However, the epistemic track record of science is not strong enough to provide a consistent argument in its support. Consequently, we had better go the moral route.

This argument is ingenious. It introduces some new concepts that actually change the agenda of discussion. In particular, the shift from scientific research to technical decision-making is theoretically fertile, and also shows a promising direction to explore. All that said, however, we still believe that Collins and Evans do not succeed in satisfactorily arguing for the validity of their elective modernism. Their argument is shaky at best.

First of all, it relies on a selective induction from negative cases only. Nobody is willing to deny that scientific experts are often wrong – even though the reasons for their mistakes should be carefully investigated. However, it is simply not true that experts are always wrong: in a technical decision-making scenario the risk of error is undoubtedly enhanced, but it is exaggerated to conclude that expert advice is epistemically unreliable. In addition, much of scientific knowledge is not deterministic: the fact that a singular case may happen to be in contrast with a set of general laws held by the scientific community does not count as evidence of the epistemic unreliability of those laws. On this point Collins and Evans are simply too rash.

Secondly, if their argument were correct, some unfortunate moral consequences would follow. Suppose Collins and Evans are right, and assume that there is no epistemic ground for scientific expertise, but only some kind of moral persuasion. The search for truth – which is the «fundamental formative aspiration of science», according to Collins and Evans – would therefore turn out to be an illusion since, as Wave Two has shown, «the truth of the matter cannot be found», and «there are only interpretations and perspectives». Note that Collins and Evans are happy with this view: it is true that their aim is to somehow counteract the «corrosive effect of Wave Two», but they do not question the validity of its conclusion (Collins and Evans 2017: 40). There is no truth of the matter.

How is it possible? Collins and Evans ask us to distinguish truth conceived of as the value which should justify scientific inquiry (let's call it, Truth with capital T) from the notion of truth as is usually employed by scientists to characterize their own particular form of life (truth with lowercase t). While Truth must be gotten rid of, the concept of truth is essential for science as a form of life. Indeed, if scientists do not believe that they are actually succeeding in discovering the truth of reality, their work as scientists would be substantially impossible. As Collins and Evans explicitly remark:

> One cannot do good science without disbelieving social constructivism. Individual scientists have to believe they are seeking the truth and that there is a chance of finding it, even while social scientists insist it is the social group that ultimately determines what counts. Furthermore, scientists must ignore the social constructivists if the formative aspirations of science on which this entire thesis turns are to be robust (Collins and Evans 2017: 76).

Collins and Evans do not see any trouble with this sort of self-deception.

On the contrary, they argue that it is necessary in order to preserve the source of values that is science. This assumption is highly disputable, but let's accept it for the sake of discussion[2]. At the end of the day, one may even argue that such self-deception is for the greater good, since scientists are thus given the chance to live a valuable life in a privileged environment. So, let's concede that this deception is benign.

But consider another kind of deception: imagine a society in which experts – who are not a reliable epistemic source of information – are nevertheless still consulted by citizens seeking advice. It might be argued that the same line of argument is available in this case, and that citizens too are benignly deceived when they turn to experts for making an informed decision. However, there is an asymmetry between the two cases. Indeed, in this second case citizens do not participate in the form of life of science. Consequently, contrary to scientists, citizens do not enjoy any good from being deceived. Indeed, the only good that they could enjoy would be an epistemic one, that of being correctly informed, since this is the goal at which they aim. But, according to Collins and Evans, this is a myth. It follows, therefore, that in the case of citizens deception cannot be good. It is deception pure and simple.

4. *A Pragmatist Theory of Expertise*

The last remarks were intended to show that Collins and Evans's moral defense of science leads to morally unacceptable conclusions. If our argument is correct, therefore, elective modernism is unsatisfactory by its own standards. It is a kind of double-truth theory which ushers in in a strong form of elitism and mass-manipulation[3]. More relevantly for our purposes, it also entails the impossibility of any relation between experts and laypeople because of the illusoriness of the ground on which they would enter

[2] It should be noted that Collins and Evans's views on this issue are more complicated than this. They acknowledge that it is possible – though quite rare – for a scientist (natural as well as social) to be aware that Truth cannot be achieved, and nonetheless to continue to play the game of science, which revolves around the search for truth. Collins and Evans call "owls" those scientists who have this capacity of double vision. So, properly speaking, self-deception is not a necessary condition for being a scientist. However, while working as scientist, an "owl" cannot adopt the reflective attitude which reveals that Truth does not exist; it is only when she reflects on her activity that she can reach that conclusion.

[3] On this point see Barrotta (2018: 169 ff.).

into contact. It seems highly plausible, indeed, that if lay people were to know that – no matter how well acquainted with the subject-matter of their research scientific experts might be – these so-called experts have no epistemic warrant for their opinions, they would stop asking them for advice. Consequently, the problem of expertise would fade away. Either expertise is an epistemic notion or it is a deceit.

The pragmatist theory of expertise that we are going to outline in the following pages starts by acknowledging precisely this fact: expertise is an essentially epistemic notion. Citizens turn to experts on the exclusive belief that the latter's opinion is warranted, and that by acting on the experts' advice they have the greatest chance of reaching the desired goal. Clearly, citizens do not possess the epistemic resources to assess the validity of the responses given by scientific experts. This is an *a priori* condition: indeed, if citizens were able to acquire enough expertise to peer-evaluate experts' opinions, they would cease to be citizens and would become experts in turn. Once again, the problem of expertise would fade away.

It follows, therefore, not only that the notion of expertise is essentially epistemic, but also that it is intrinsically interwoven with the notion of trust. There is no expertise without trust: scientific experts are those who are judged trustworthy by citizens. This is the thesis that we want to articulate through our pragmatist theory of expertise.

Such a thesis is likely to look highly problematic. For instance, it may be countered by arguing that it leads to a dangerous submission of scientific expertise to the judgment of lay people. Clearly, if science were made dependent on what citizens happen to think is true, the autonomy of science would be fatally weakened. This is an unfortunate consequence which threatens the possibility of objective knowledge, and, consequently, undermines the very idea of expertise. Indeed, citizens ask experts for advice because their opinions are taken to be true in and of themselves: if scientific opinions were in need of any kind of external support – external to the body of the scientific community – they would become a matter of preferences and polls. But then this would be nothing but Second Wave approach in disguise.

Clearly, this is not what we have in mind. Quite the opposite, our proposal aims to preserve the autonomy of science, and, at the very same time, to acknowledge the relational nature of expertise. In order to see how this is possible, it is useful to first clarify the concepts that we will employ. In particular, we want to introduce a distinction between scientists and scientific experts. Though not widely accepted, this distinction is not

wholly new[4]. As we intend it, the distinction purports to highlight a difference in the way scientists operate.

We rely here on an insight shared by Collins and Evans. As said above, elective modernism is concerned with technical decision-making. Collins and Evans rightly remark that what is at stake in cases of technical decision-making is the solution of a particular problem in which social and scientific issues are inextricably entangled together. The subject-matter of technical decision-making is, therefore, a complex *imbroglio* which cannot be reduced to its scientific components. Think, for instance, of the construction of a nuclear power station in one specific locality. Clearly, many of the problems to be dealt with in planning the construction work have to do with scientific and technical issues – from the composition of the concrete, which must be not too porous, to the design of reactor containers and the assessment of the irradiation effects. However, other legitimate problems arise, such as the opportunity to build in that particular site, the economical and social consequences of that project, the political and military risks that inevitably have to be faced, the ethical concerns about the impact of that decision on future generations, and so on. All these social aspects are just as relevant for the definition of the problem as its scientific components.

What is this example intended to show? We believe that it helps to shed light on the fundamental difference between science as it is carried out in laboratory and science as it is conducted in the public space. In the former case, the subject-matter of scientific research is abstracted and idealized; in the latter case, on the contrary, the subject-matter of technical decision-making is a group of processes and events taken in their concreteness. The example also highlights the different complexity of the subject-matter: by stressing the fact that technical decision-making cannot be boiled down to its scientific and technical components, it is implied that science cannot provide the whole truth of the matter. Any threat of technocracy is thus excised.

The difference between science in laboratory and science in the public space is what we want to grasp through our distinction between scientists and scientific experts. A few considerations are worth making here. First of all, that distinction is *functional*: the very same person can be a scientist and a scientific expert, as a consequence of being engaged in different activities – respectively, scientific research and technical decision-making. This does not mean, however, that we are committed to a relational

[4] See, for instance, Grundmann (2017).

conception of what it means to be a scientist. Much of the recent debate on the nature of expertise has turned around this issue, whether expertise is a substantial or relational notion. One of the strengths of our approach is that it allows us to take the best of both worlds. Thanks to the distinction between scientists and scientific experts we are allowed to say that being a scientist is a substantial qualification: in order to become a scientist, one has to reach a certain number of educational and academic achievements, not least of which is getting an academic job. On the contrary, to be acknowledged as an expert is a relational notion, which we conceive of as based on trust.

Our proposal is in agreement with the ordinary use of the terms: while we say that being a scientist is a profession, being an expert is a status, and the attribution of such a status is context-sensitive. Indeed, to be recognized as an expert depends partly on the specific problem at stake, and partly on the background knowledge of those who turn to experts for advice. So, for instance, if I do not know anything about wine, asking a sommelier who can give me tons of information about the different methods of production of wine is much less effective than asking to a wine shop assistant who can provide some educated guidance. At the end of the day, time and intellectual effort matter when one has to make a decision.

In more precise terms, being a scientist is a *necessary* condition for being a scientific expert, but it is not a *sufficient* condition. A scientist turns into a scientific expert when she is asked to participate in technical decision-making. However, as pragmatists never tire of pointing out, the application of a body of knowledge is not epistemically neutral: it raises new problems, and asks for different solutions. The problems that a scientific expert has to face are different from the ones that she faces when she works as a scientist. This is partly due to the fact that any concrete case presents some specific features which must be taken into account, and which cannot be derived from the body of knowledge already available (Barrotta & Montuschi 2018). In addition, there may well be reservoirs of information that are not formulated in scientific language, but nonetheless prove to be reliable and valuable[5] (Wynne 1996). Finally, as has been

[5] It is worth noting that the acknowledgment of the existence of reservoirs of information that are possessed by lay people does not enter into conflict with our assumption that being a scientist is a *necessary* condition for being a *scientific* expert. We do not want to take a position on the issue of lay expertise since there is not enough space to provide a detailed discussion. It is sufficient to remark that we can easily make room for that concept in our account by distinguishing between different varieties of expertise.

repeatedly stressed, in the case of technical decision-making the subject-matter is made more complex by the entanglement of scientific and political issues.

We are in a better position now to clarify the conceptual import of the notion of trust. We have stressed the fact that the "grammar" of expertise is grounded on trust, and that trust is an essentially epistemic concept. No expertise without trust, therefore. Trust, however, should not be conceptualized in an unidirectional way, as going from citizens to experts. If it were, our proposal would be substantially identical with the First Wave idea of "public understanding of science". On the contrary, trust is a bidirectional relationship: it is only because they succeed in being perceived as trustworthy by citizens that scientific experts are so acknowledged. Contrary to being a scientist, which is a profession, being a scientific expert is a social status that has to be earned and maintained. Trust can be withdrawn any time.

At the very same time, however, our pragmatist account of expertise provides some strong normative criteria to evaluate the *legitimacy* of citizen dissent against the advices of scientific experts. Dissent is not legitimate when it is directed against propositions a) that are accepted by the scientific community, and b) whose content is unaffected by any social consequences in which the objects which the propositions refer to may take part.

Take, for instance, the protests against vaccines. Are they legitimate according to our approach? The point at stake is to understand what these protests take as their target. If they are directed against settled scientific facts – such as the fact that vaccines do not cause autism – then they are not legitimate since they would interfere with the proper domain of science. Trusting scientific experts means to acknowledge and respect their competence in their field of expertise: public dissent has limits, which are defined by our best method of ascertaining the truth of a proposition. On the contrary, if the reasons of the protest have to do with the opportunity to publicly finance a campaign of vaccination, then the dissent is legitimate since the subject-matter of the problem is a social issue which cannot be boiled down to its scientific components. Here, trust puts some normative constraints in the opposite direction: scientific experts, in order to earn and preserve their status, are compelled to acknowledge the right of the citizens to participate – as epistemic contributors – in technical decision-making processes.

5. *Conclusion*

The goal of this article was to criticize Collins and Evans's moral defense of the role of science in democracy, and to point out that, contrary to what they believe, the notion of scientific expertise is epistemic through and through. We have shown that devoid of its epistemic dimension, the appeal to scientific expertise turns into a form of deception of the citizens. Then, we have argued that trust should be conceived of as the backbone of scientific expertise. Our pragmatist account of expertise revolves precisely around the idea that being a scientific expert is a social status that is to be earned and preserved: scientific experts are those who are perceived as trustworthy by the citizens. Finally, we have stressed that trust is a bidirectional relationship. More precisely, trust is a normative concept which puts constraints on the kinds of behavior that citizens and scientific experts are legitimate to perform. It follows that technical decision-making is a highly dynamic and conflictual sphere, in which the struggle for reciprocal recognition goes hand in hand with the effort to find the most reliable solution to the problem at stake.

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Abstract

Collins and Evans's Third Way of Social Studies of Science is an ambitious attempt to counteract the de-legitimation of scientific experts that is going on in contemporary Western societies and which, on a theoretical level, represents an unfortunate consequence of the corrosive approach championed by many proponents of Social Studies of Science. Collins and Evans argue that the importance of science in technical decision-making should be defended on purely moral grounds, without having recourse to epistemic notions. The goal of this article is to criticize Collins and Evans's moral defense of the role of science in democracy, and to point out that, contrary to what they believe, the notion of scientific expertise is epistemic through and through. Our pragmatist account of expertise revolves around the idea that being a scientific expert is a social status that is to be earned and preserved: scientific experts are those who are perceived as trustworthy by the citizens. We argue, therefore, that trust is a bidirectional relationship. Trust is a normative concept that puts constraints on the kinds of behavior that both citizens and scientific experts are legitimate to perform.

Keywords: pragmatism; expertise; third wave of social studies of science; scientists; philosophy of competence.

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