Normativity and its vindication: The case of Logic[1]

Concha MARTÍNEZ VIDAL

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ABSTRACT. Physical laws are irresistible. Logical rules are not. That is why logic is said to be normative. Given a system of logic we have a Norma, a standard of correctness. The problem is that we need another Norma to establish when the standard of correctness is to be applied. Subsequently we start by clarifying the senses in which the term 'logic' and the term 'normativity' are being used. Then we explore two different epistemologies for logic to see the sort of defence of the normativity of logic they allow for; if any. The analysis concentrates on the case of classical logic. In particular the issue will be appraised from the perspective put forward by the epistemology based on the methodology of wide reflective equilibrium and the scientific one underlying the view of logic as model.

Keywords: normativity, logic, wide reflective equilibrium, view of logic as model.

1. Logic is normative

It is clear that logic has traditionally been considered as normative. But, let us not take anything for granted and try to understand what this means. First, we need to state what we understand by logic. The term 'logic' is ambiguous in that it can be understood as what Peirce called, following medieval logicians, *logica utens* —the rules that a given subject, or community of subjects, uses—or as what he called *logica docens*—the set of theories that logicians have developed. In what follows we will use the term 'logic' in the second sense given above.

The task of logical theories (*logica docens*) has been frequently depicted as that of developing theories for the evaluations of arguments. Following Burgess, the Kneales, and many others we claim that it should be obvious that "logic as a discipline could not develop until the practice of rational argumentation had flourished" (Resnik, 1985, p. 230). In the same line, Corcoran (Corcoran 1973) has claimed that before Aristotle developed his conception of proof a large amount of proofs had already been obtained. Therefore, since logical theories developed after rational argumentative practice, in what follows, our bank of *data* will consist in a certain type of rational argumentative practice: The arguments and proofs in classical mathematical practice. My emphasis on mathematical practice intends to leave aside the problems pointed out by Resnik (Resnik, 1985, p. 227) in relation to the issue of using logical theories to describe a "natural practice":

> "First, when we speak of *our* logical practice, whose practice do we have in mind? Ours *qua* professional logicians and philosophers? Ours *qua* rational and educated adult members of our culture? And what do we mean by *practice*? Are we concerned with inferential behaviour as exhibited in the arguments we write and endorse, the inferential choices we make, and our responses on

[1] I would like to thank participants in the *Seminar on Normativity* (Granada, February 12-13, 2004) for their useful comments.

> tests of reasoning? Or are we interested in our considered judgements of logical cogency? If we construe "practice" so broadly that it includes answering questions on the tests of reasoning used in some psychological studies, then it appears that no extant logical theory has a chance in the world of capturing our practice." (Resnik, 1985, p. 227.)

Crispin Wright has answered some of the questions posed by Resnik above; in particular, Wright defines a practice as

> "… any form of intentional, purposeful activity." (Wright, 1992, p. 15.)

And a *move* as

> "… any action performed within the practice, for its characteristic purposes…" (Ibid.)

In our case classical mathematical practice is a purposeful activity and logic studies it, therefore, logic studies the moves performed in classical mathematical proofs. And logic, for its part, is a practice too, one whose methodology is as well that of proof.

Next, we need to elucidate what is meant by a 'normative' feature of a given practice. Wright defines it in the following terms:

> "Various proposals are no doubt possible, but we should recognise straight away a distinction between descriptive and prescriptive claims about normativity. A characteristic of moves in a particular practice is a descriptive norm if, as a matter of fact, participants in the practice are positively guided in their selection of moves by whether a proposed move possesses that characteristic. […] By contrast, a characteristic of moves supplies a prescriptive norm just in case the reflection that a move has that characteristic provides a […] reason for making, or endorsing, or permitting it, even if such reasons tend, for the most part, to go unacknowledged by actual participants." (Wright, 1992, p. 15.)

According to the definition of 'move' above, it currently seems obvious (though it took a long time to come to it) that introduction and elimination rules for logical connectives reflect the *moves* underlying mathematicians practice.[2] Hence, those rules in themselves are not prescriptive like Resnik has pointed out (Resnik, 1985, p. 236). However, they are descriptive of classical mathematical practice, that is to say, they are rules such that participants in the practice are "positively guided in their selection of moves" by the fact that they are instances of the introduction and elimination inference rules for the logical constants. Other examples of descriptive rules could be: in case one has to choose among several rules, pick any rule that allows us to shorten a proof, one that takes us directly to the conclusion, and so forth. But, it is also possible to enunciate rules that are normative of classical mathematical practice. For instance, one prescriptive rule for classical mathematical practice is the following meta-rule: an inference rule will be permitted only if it is truth preserving.[3]

Thus, since the object and method of logic is mathematical proof, in as much as logic tries to formulate norms (whether descriptive or prescriptive), logic is about the norms underlying classical mathematical practice. But does this convey that logical practice itself is normative? Well, since the method of logic is also proof (the same

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[2] The remark about how long it took to formulate the introduction and elimination rules for logical constants has been taken from a comment by Neil Tennant on the internet forum "FOM", http://www.cs.nyu.edu/pipermail/fom/1998-January/001042.html.

[3] Being truth-preserving is a compulsory rule.

method of mathematicians), and we have established that mathematical practice basically consists in obtaining proofs and that there is at least one prescriptive rule underlying mathematical practice, the same sort of rule should underlie logical practice too. In fact, if one analyses classical logical practice one can see that 'being truth-preserving' is in force also in the case of classical logic. Moreover, it can be said that underlying the practice of different logical theories there are diverse prescriptive rules. For instance, if the practice considered is that of classical logic, then it is clear that logical systems have to satisfy a minimum requisite for them to be interesting: they have to be sound. And there is a lot of discussion about whether completeness should also be mandatory for a logical system to be such.[4] But if the logical theory considered is that of intuitionistic logic or paraconsistent logic, the minimum requisites will be different. In the case of intuitionistic logic, harmony seems to be a key requirement any system must satisfy in order to be interesting. Therefore, there are different logical practices, and consequently there are diverse standards, different descriptive and prescriptive rules. The fact that diverse logical practices exist brings in many classical problems in the philosophy of logic, for instance, whether there ought to be one logic or many, the problem of which logic is the right logic, and so forth.

In order to make perspicuous what is at stake here, it seems important to differentiate among the following concerns:

- One matter is whether classical mathematical practice is normative. Corresponding to this we have argued that classical mathematical practice is normative in that there are descriptive and prescriptive norms underlying that practice.
- Another problem is whether logical practice is normative. In relation to this we have made the case that logical practice is normative in that there are both descriptive and prescriptive rules underlying it.
- Finally a third question is whether logical theories are to be understood as descriptive accounts of a normative prescriptive practice —that of classical mathematics— or as normative accounts of a normative prescriptive practice, again that of classical mathematics.

Resnik has clearly formulated the distinction between a descriptive theory of a prescriptive practice and a prescriptive theory about a prescriptive practice:

> "Whenever a community has a practice, the project of developing a theory of it suggests itself. When the practice is one of evaluation, a distinction must be made between descriptive and prescriptive theories thereof. The former aims to describe explicitly what the community's implicit standards have been: the theory is itself evaluated by how well it agrees with the facts of the community's practice. The latter presumes to prescribe what the community's standards ought to be: the community's practice is evaluated by how well it agrees with the norms of the theory." (Resnik, 1996, p. 12.)

[4] See Jané (in print) for a very interesting argument in favour of considering completeness as a necessary requisite.

The third issue above is thus whether logical theories are descriptive or prescriptive in relation to mathematical practice. In a sense, in order to establish whether a particular logical theory is in fact descriptive or prescriptive of a given practice what we have to do is to see whether it describes the practice (in our case, of classical mathematical proof) or whether on the contrary it aims at establishing how it should be. Thus, for instance, if the logical theory we are considering is classical logic, it is clear we can say that it is descriptive of classical mathematical practice; while, if the logical theory that is being analysed is that of intuitionistic logic, then it clearly does not depict the moves in classical mathematical practice. Yet, the development of intuitionistic logic aimed at prescribing that the current moves in force in classical mathematical practice should be replaced by those described in intuitionistic logic. In other words, though intuitionistic logic by itself is inadequate to account for the moves in force in classical mathematical practice, proponents of intuitionism claim, on well-known philosophical grounds, that intuitionistic logic should be used as the underlying logic of mathematics, and, subsequently, that mathematical practice should be modified. Yet, since those philosophical claims have failed to convince the majority of mathematicians to abandon classical mathematical practice, it can be said that they do not provide an adequate basis to support intuitionistic logic. This brings us back to our problem; namely, on what basis can a logical theory be vindicated, if any.

Note that if we consider category theory, instead of classical mathematics, it seems that intuitionistic logic can be seen as a descriptive theory of its underlying logic. That is so because the moves underlying the practice seem to match the moves depicted by intuitionistic logic.

In what follows we will try to formulate one possible way in which a given logical theory could defend itself as the underlying logic of classical mathematics. In doing so, we will claim that one appealing way to justify that a given logic should be used is not that it adequately models our inferential behaviour, or that it matches the judgements accepted by a given community, but that only that logic (or any other extensionally equivalent one in the relevant sense to be clarified) allows us to unveil a particular set of truths, a set of truths that is relevant for the explanation of an 'independent' endeavour.

2. Physics, history of philosophy of science, linguistics, ethics and logic

In this section our purpose will be to analyse how things work in other areas in order to elaborate a comparative analysis with logic. We will be considering the cases of physics (and other clearly scientific subjects such as medicine), history and philosophy of science, ethics and linguistics. In particular, we will be interested in seeing how the passing from the descriptive to the normative goes.

Let us start with physics. Physical laws are irresistible. Physical objects cannot but obey them.[5] Logical rules are not. As we claimed above, that is why logic is said to be normative while physics is not. The laws of physics do not correspond to any inten-

[5] Railton (2000, p. 4) points out that the same goes for conceptual necessity.

tional and purposeful activity. They are not norms. Yet, there are some prescriptive rules that are taught to children and whose support is, for instance, our knowledge of physics or medicine. By way of illustration, we teach our children not to jump from any window in a high building in order to preserve their safety. The basis for this prescriptive rule is quite clearly that we know that we, as physical objects, cannot but obey physical laws, and one of the laws of Newton says that any body will fall…[6] The normative force of rules like the one we are considering is huge. We also teach our children not to smoke in order to live a life as long and healthy as possible, but the normative force of this rule is not so big, because not smoking is neither sufficient nor necessary for living a long and healthy life. Yet, the normative force of both the rules above comes on one side from our medical knowledge about the effects smoking has on our health and on the other from our knowledge of physics. So we come before a human practice, that of living, and there are some very clear prescriptive norms in relation to it. The normative force of the rule comes from our scientific 'knowledge' and depends to a great extent on the confidence the underlying scientific theory provides. It also hinges on the goals we assume; thus arguments of the sort of the following are very popular among smokers: smoking (or eating or drinking, etc) is a pleasure and what do I want life for if I cannot enjoy my most dear pleasures!

Now, an interesting question to pose is that of how much revisionism we can tolerate about these norms. It is clear that in relation to the first norm —"don't throw yourself out of a high window"— no revisionism can be tolerated if the law of Newton is true and if we want to continue living. As to the second case, it could only be rejected if our best medical theories eventually concluded that our current beliefs about the evil effects of smoking were wrong, if our current medical theory proved false and an alternative medical theory was developed or if we changed our goals. And, this is it for science.

Paul Thagard (Thagard, 1982) has analysed the model for passing from the descriptive to the normative in the area of history and philosophy of science.[7] He abbreviates de method as "HPS" and describes it as follows:

> "1. We select cases of actual scientific practice. Selection is made on the basis of subsequent events in the history of science which have marked the cases as significant contributions to the growth of scientific knowledge.
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> 2. We develop case studies which describe scientific practice.
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> 3. We assume —or this can be argued for— that scientists have generally been successful in achieving the epistemic goals of science.
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> 4. Then the actual methods of the scientists in our case studies are at least an approximation to what the methods *ought* to be. We describe the methods found.

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[6] Of course we know that from practice, but in this case I will consider the scientific explanation underlying that practical knowledge.

[7] For a careful discussion of the issue of normativity in the area of philosophy of science, see Rodríguez Alcazar's paper in this volume.

> 5. We reflect philosophically on the methods found in the case studies, developing more complex normative modes, which can then be applied to other case studies.
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> This description is schematic and its linearity is highly misleading. It seems to suggest that we proceed by first doing history and then by deriving methodological principles. But of course our historiography is unavoidably influenced by expected methodological conclusions." (Thagard, 1982, pp. 27-8.)

Later in the same paper, he claims that this methodology is not adequate to account for what happens in the case of logic (he understands the term in the sense of *logica utens* aforementioned, not *logica docens* as we are doing in this paper). The reason is that in the case of the logic that underlies our inferential practices in general we cannot identify the analogue of those special cases that led to the significant growth of scientific knowledge. Therefore, the reason to vindicate a particular methodology for science is that it has proved successful to obtain results for an independent description theory. As he indicates:

> "The situation in logic is rather like the situation doing historical philosophy of *social* science. Whereas in natural science there is sufficient consensus in the scientific community that selection of cases for study is quite uncontentious, the schisms in the various social sciences preclude doing case studies whose validity would be universally accepted. For example, a study of explanation in economics would get very different results if the investigator concentrated on Marx's capital theory rather than neo-classical theories." (Thagard, 1982, p. 32.)

Hence, according to Thagard the problem with social sciences is that the evaluation of cases as successful or unsuccessful is theory-ladenned, that is to say, in Wright's words paraphrased in terms of social sciences:

> ... reports of explanation in economics do not count as acceptable, or unacceptable, independently of one's economic theory, that what is proper to report oneself as going on is a function of, inter alia, elements of economical theory which one carries into the situation.[8]

Now, let us consider the case of linguistics, as Burgess puts it:

> "The data for descriptive *theorizing* consist of evaluations of members of the community whose evaluative practices are under investigation (e.g. "That's not good English"). […] theorists who are themselves members of the community (e.g. native English speakers investigating the grammar of English) often use their own impressions of the felicity or infelicity of particular examples as their main source of data. Such impressions are intuitions in an every day sense, impressions of whose source and grounds one is unconscious. Intuitions in this sense are notably fallible and corrigible, especially in the case of a theorist out to establish a pet theory…" (Burgess, 1992, p. 13.)

Thus, in the case of linguistics it seems linguists develop grammars from a given practice and what they actually take into account are their own judgements, their own intuitions. As Burgess points out "we do not know where those intuitions come from". In case of conflict the legitimate authority is a conventionally adopted one; usually, an academy or institute for the considered language that is responsible for formulating

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[8] The original test by Wright is: "... reports of observation do not count as acceptable, or unacceptable, independently of one's empirical beliefs, that what is proper to report oneself as having observed is a function of, inter alia, elements of empirical theory which one carries into the observational situation" (Wright, 1992, p. 161).

the rules. In case of conflict among linguists in the academy, they should come to an agreement. This seems to imply that the rules the academy formulates are *totally* arbitrary; but, on the contrary, they frequently follow the intuitions of speakers among which the academics are. In those circumstances in which the correct grammar of a language that lacked an authority for sometime is being developed (Galician language is an example), academics try to extrapolate the rules implicit in the existing practice of the language in order to formulate new rules. Their task is usually far from easy because there is not a common practice against which to evaluate what is "intuitively right." On its part, the normative force of the authority results from the fact that a given society decides to delegate the task of determining which is the correct way of writing (speaking, spelling and so forth) a given language in an academy. Some authors — Burgess among them— seem to think that things are quite similar in the case of logic: If one considers classical mathematical practice as our data and the task of the logician as that of describing that practice, it is the judgements of the mathematicians what the logician is modelling. In case there is a conflict between two mathematicians they decide whether a given inference is to be accepted or not and though there is no formally constituted institution responsible for the inferences that are being accepted, the mathematician community itself can be seen to play that role. (More on this in the next section)

Things seem to get even worse when the prescriptive norms for the practice of living have to do with moral affairs. In this field things are thorny, though it is usually clear whether a given behaviour is correct according to a considered standard. The difficulties come from the fact that there are several standards of moral behaviour and it is far from clear which of all those existing standards should be universally accepted. It is also true that there are some principles that are shared by different standards.

In this area, contrary to what happens in the case of prescriptive norms such as "don't jump from the window" or "don't smoke", the problem is that at the moment there is no independent descriptive theory about any independent reality in terms of which the truth of a given moral statement can be established and it is far from clear whether such a reality exists. Morals /Ethics are precisely practical disciplines and one of their traditional problems has been how the truth of a moral/ ethical theory should be understood. Christine Korsgaard (Korsgaard, 1996) has maintained that four successive answers have been given to the question of on what basis should the normativity of a given rule be vindicated:

- Voluntarism contends that obligation derives from the command of someone who has a legitimate authority over the moral agent and so can make laws for her. You must do the right thing because God commands it, or because a political sovereign whom you have agreed to obey makes it law. Normativity springs from a legislative will. The solution here comes to putting in place of the independent scientific discipline that establishes what is true, physics or medicine in the scenarios above, a 'legitimate' authority. This authority both establishes what is right and determines our moral goals. The problem is why we are obliged to obey this authority or to conform to a social contract.

- Realism argues that moral claims are normative if they are true, and true if there are intrinsically normative entities or facts they correctly describe. Realists try to establish the normativity of ethics by arguing that values or obligations or reasons really exist, or by arguing against scepticism. This proposal clearly aims at recovering for moral claims the sort of justification available for normative assertions in other fields. One main argument to defend this point of view has been that the alternative is scepticism. Christine Korsgaard has put forward that a moral sceptic is not one who does not believe in the existence of moral concepts but one who thinks that the explanation of moral concepts will be one that does not support the claims that morality makes on us.

- Reflective endorsement (Mill, Bernard Williams):

  "Morality is grounded in human nature. Obligations and values are projections of our own moral sentiments and dispositions. To say that these sentiments and dispositions are justified is not to say that they track the truth, but rather to say that they are good.

  …

  But the normative question is one that arises in the heat of action. It is as agents that we must do what we are obligated to do, and it is as agents that we demand to know why. So it is not just our dispositions, but rather the particular motives and impulses that spring from them, that must seem to us to be normative." (Korsgaard, 1996, p. 91.)

- The appeal to autonomy: Kant shares with the realist the thought that we must show that certain things are right and others are wrong and for that purpose a test is needed. But, against the realist the mentioned test is not one of knowledge (or truth), but that of reflective endorsement. According to Korsgaard, Kant founds that we are bound by the categorical imperative (we must choose a law) but he does not establish that we have to follow the moral law (act only on maxims that all rational beings could agree to act on together in a workable co-operative system). She tries to establish that herself. A law should provide a final reason —be good for its own sake— and the source for those reasons must rely in the internal structure of the rule, not in the action it promotes.[9]
- Another methodology that has been proposed for the moral case is that of wide reflective equilibrium. Thagard (Thagard, 1982) makes the following reconstruction of its application to the moral case:

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  1. We have a set of particular moral judgements about what is right or wrong, selected for expected freedom from error.

  2. We postulate a number of general moral principles which explain and justify the particular judgements.

[9] For a perspicuous critique of this view, see Corbí's paper in this volume.

> 3. We attempt to come up with a maximally coherent set of beliefs, consisting not only of the moral judgements and principles, but also taking into account our background theories, especially concerning psychological limitations.
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> 4. We arrive at a state of reflective equilibrium, and conclude that the acceptance of the moral principles in the final set of beliefs is justified.
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> As with HPS this description is misleadingly linear: the process of reflective equilibrium is best represented by the flow chart in figure two. Normative principles are outputs from the system only after repeated adjustments of moral judgements and principles in the light of background theories have been made." (Thagard, 1982, pp. 30-31.)

The problem with this methodology is that no justification is provided for the initial set of moral judgements.

So, to summarize what is the relation between the normative disciplines above and certain descriptive theories? Is there any? On what basis are those rules vindicated? In the case of the prescriptive sentences such as 'don't throw yourself through the window' it is clear that their normative force comes from our knowledge of the truth of physical laws. In the case of History of Philosophy of Science, the normative force of the methodology that comes from the study of successful cases, that is to say, from the fact that we know that the outcome obtained was a major breakthrough. But the resulting normative force does not seem to be one of obligation. Using that methodology is not compulsory in every case. Also there seems to be some sort of feedback between expected methodological conclusions and the methodology. In the case of linguistics an authority legitimated by society is responsible for dictating the rules of the grammar of the given language and the academy makes its decisions on the basis of the judgements of its members. In the case of social sciences like economics it is quite easy to determine the sort of explanation available from the perspective of a given economic theory (Marxist, Liberal,) but it is very difficult to determine which standard should be in force. In the case of ethics the problem is clearly an open one; to establish whether something agrees to a considered set of rules is one thing, to provide reasons to support one standard or other is quite another.

3. Logic and wide reflective equilibrium

The methodology of reflective equilibrium was first proposed by Nelson Goodman (Goodman, 1955, pp. 65-68) as an adequate epistemology for logic. Resnik has claimed (Resnik, 1985) that wide reflective equilibrium constitutes an adequate epistemology for *normative* uses of logic, while he takes it that the epistemology of descriptive uses of logic is in many respects similar to that in place in science:

> "Once we decide on why we want to model a particular deductive practice by means of a given logical theory, we start with sample cases and fit them in the model (that is, formalize them) and proceed from there to build a larger and larger collection of successful cases." (Ibid., p. 229.)

He takes it that the methodology of wide reflective equilibrium is the adequate epistemology for logic when it assumes a prescriptive role because it allows us to explain why we are prepared to reform our judgements of validity. He describes it as follows:

> "One starts with one's own intuitions concerning logical correctness (or logical necessity). These usually take the form of a set of test cases: arguments that one accepts or rejects, statements that one takes to be logically necessary, inconsistent, or equivalent to one another. These are —in Rawl's terms— one's considered judgments. Given their subject matter, I find it convenient to call them considered judgments of logic. One then tries to build a logical theory whose pronouncements accord with one's initial *considered judgments*. It is unlikely that initial attempts will produce an exact fit between the theory and the 'data'. Furthermore, committing oneself to a logical theory invariably entails acknowledging unforeseen and prima facie anomalous logical relationships. (The so-called paradoxes of truth-functional implication constitute a case in point.) Sometimes one can respond to such anomalies with a simple modification of one's formal system. Sometimes re-translating a prima facie anomalous argument will reconcile it with the theory. Sometimes, however, one will yield one's logical intuitions to powerful or elegant systematic considerations. In short, 'theory' will lead one to reject the 'data'. Moreover, in deciding what must give, not only should one consider the merits of the logical theory *per se*, such as its simplicity, fruitfulness or elegance, and the firmness of one's logical intuitions, but one should also consider how the theory and one's intuitions cohere with one's other beliefs and commitments, including philosophical ones. When the theory rejects no example that one is determined to preserve and countenances none one is determined to reject, then the theory and its terminal set of considered judgments are in, to use Rawl's terminology, wide reflective equilibrium. The equilibrium is wide, because the theory is consonant not only with one's terminal set of considered judgments of logic, the mark of narrow reflective equilibrium, but also with one's broader system of beliefs." (Resnik, 1997, p. 159; 2000, p. 188.)[10]

If, for the sake of the argument, one accepts without further ado Resnik's proposal, then that a logic L1 is in force can be warranted on the basis that the aforementioned logic allows us to attain reflective equilibrium. The problem is that there can be different systems in wide reflective equilibrium.

> "Since constructing a logical theory involves balancing various values against each other and making choice after choice, there is no reason to expect it to lead different users to the same outcome. Even if we could make sense of an ideal limit of logical enquiry —when all the 'data' are in— we cannot avoid the possibility of different logicians (or different communities of logicians) correctly applying the method to the same initial data and arriving at different outcomes." (Resnik, 1997, p. 160.)

Thus, any person or community who had achieved wide reflective equilibrium in terms of another system —let us say, L2— would reject that L1 were in force. According to Resnik, these two people (or communities) could not even understand each other:

> "Since in determining reflective equilibrium one uses the logic contained in one's own evolving logical theory, one might think that a theory may be in reflective equilibrium from its own internal point of view and not so from the point of view of another theory. I hesitate to draw this conclusion since I wonder whether one could make sense of a rival theory while remaining true to one's own…. Reflective equilibrium may be a notion that is immanent to a logic rather than transcendent." (Ibid.)

How could we decide between different systems in wide reflective equilibrium? Resnik speculates whether the situation would be one of incommensurability or one in which we would count on different standards and are unable to vindicate one instead of the other.

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[10] For a longer account of the wide reflective equilibrium method see Resnik, 1985.

It should be obvious at this point that Resnik's proposal allows us to maintain that a given logic should be used because it is the logic that allows us to attain wide reflective equilibrium. But it should also be clear that such justification would be immanent to the system. That is to say, determining which beliefs are simultaneously acceptable directly depends on the assumed logic; hence there will be as many systems in wide reflective equilibrium as logics allow us to attain 'equilibrium'. Thus, the problem is whether it makes sense to speak of "*the* method of reflective equilibrium, which seems to indicate that there is a single notion that applies to all logics" (Shapiro, 2000, p. 350).

Resnik takes it that this epistemology for logic is compatible with a number of views about logic. I take those would all have to be anti-realist conceptions of logic (conventionalist, expressivist and so forth) since, from considering wide reflective equilibrium as an adequate epistemology for logic, Resnik concludes that in case there are mind independent logical facts, we cannot learn them since there might be more than one logic that allowed us to attain wide reflective equilibrium. Shapiro objects that Resnik is too quick to rule out logical realism. He takes it that from the fact that there could be rival logical theories for logic, it does not follow that there are no logical facts. If that were so, no realism could be defended for any area or discourse, not even for science and Resnik is not ready to accept that conclusion. It could also be said that he does not consider the possibility that his philosophy of logic is inadequate because it does not make sense of logical pluralism.

Resnik takes it that there is a fundamental difference between logical methodology and scientific methodology, that is probably why he rules out logical realism:

> "… I do not think we can make sense of an ideal point where logicians are bound to agree at least concerning the logical data. Scientists in differing circumstances are likely to begin with different observational data, but it is at least arguable that their opinions concerning the observational data can be made to converge by exposing them to sufficiently similar experiences. Logicians, like scientists, are likely to start theory construction with different initial data. Some will see the proof that the Peano axioms are categorical as a logical deduction, others will argue that it involves set theoretic reasoning. … But, unlike the scientific case, we cannot contrive for logicians to concur concerning the 'data' unless we do some fancy brainwashing. For it is not just a matter of seeing that they have similar experiences; rather it is a matter of making them come to the same evaluations.
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> …
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> I have been discussing reflective equilibrium in the hopes of showing that the epistemology of logical necessity and possibility —in so far as there is one— is intuition based, and unlike an observation based epistemology, nothing outside us promotes the convergence of conflicting intuitions concerning logical necessity and possibility. In this *logical intuitions are like moral ones*, and disagreements about matters of logic can be as intractable as those over morals." (Resnik, 2000, p. 189.)

In what follows I will try to argue that the possibility remains open that we can scheme logicians to be of the same mind concerning the data, without any brainwashing involved.

4. A scientific epistemology for logic: The view of logic as model, Shapiro's toy example and the wide cosmological role criterion

The scientific epistemology we intend to put forward as a plausible epistemology for logic takes it that logicians build models of mathematical practice analogously to the way in which scientists (biologists, physics, and so forth) build models of their fields. This idea has been explored by Stewart Shapiro (Shapiro, 2002), John Corcoran (Corcoran, 1973), and myself (Martínez, manuscript). The difficulties for this view have to do with the point in which the analogy with science disappears; namely, while scientists do not dream of modifying the data when testing a model, our problem is whether the same goes for mathematical practice since our data are mathematicians' judgements of correctness involved in mathematical proofs (classical mathematical practice). The issue is that this view of logic does not seem to permit us any reason to vindicate that a given mathematician should use a given logic. Shapiro has made this point by means of the following "toy" example:

> "... suppose that a mathematician violates the conditions on free and bound variables, but still insists that her practice contains no mistakes. She claims that she has made no false conclusions, and we have to admit that we cannot find any in her practice. The logician points out that by ignoring the rules for parameters or free variables which the mathematician seems to be doing, she might reason from true premises to false conclusion. The mathematician retorts that the pedantic logician has mis-described the practice, and that she is not following the flawed rule he has attributed to her, nor is she incorrectly following what the logician says is the correct rule. The logician asks her to say what rule she is following. She declines the invitation, claiming to have no patience for logic, but she insists that she knows what she is doing —suggesting we butt out. The logician might feel like Berkeley complaining about the infinitesimal calculus. And the mathematician might feel like the analysts who apparently felt safe in ignoring Berkeley's critique, for a time, anyway." (Shapiro, 2002, pp. 161-162.)

The problem for the mathematician is that the logician is applying the wrong logical model to explain her practice. While the logician claims that according to the model she is using, that inference is incorrect, in other words, it is not truth preserving. The view of logic as model and the notion of mathematical practice it involves do not convey that the mathematician can do as she likes. What Shapiro contends is that even if that is so, in practice, the general frame provided by the view of logic as model does not help to settle the issue of who can correct whom. I would say, the view of logic as model not only does not provide the logician with authority to correct the mathematician in the scenario above, but also establishes that it is mathematicians who have to decide. The logician can only correct mathematician's judgements when the logical model on which she bases her evaluation has been established as an adequate model of the considered mathematical practice. In other words, the point is that the difference of opinion can be solved if the one who is disputing the correctness of the rule to the mathematician in the scenario above is a second mathematician; then the burden of proof is on the roof of the first mathematician and she *has* to make explicit the rule she is using in order to convince her colleagues that her intended proof is actually a proof. The following is clearly a rule underlying mathematical practice: a proof will not be considered as such until the relevant community of mathematicians comes to

accept it. This conveys that experts come to an agreement. The fact that they do seems to imply that some sort of objectivity is captured. Thus, what is shown to be true by a given proof is that a mathematical statement follows from the assumptions in the theory. Sometimes what a proof puts forward is that a given truth obtains when a certain link is established with some other well-known mathematical theory, or if a new axiom is accepted. Mathematical proofs show that certain relations of conceptual necessity are in force. That is what mathematicians agree to. And the somehow independent character of such conceptual necessities is what gives grounds to our conception of mathematical knowledge as objective —in some sense of the term. Now, those conceptual necessities are internal to theories. An intuitionistic mathematician agrees as to what follows from the current axioms in set theory and a classical mathematician accepts that the axiom of choice is unacceptable from an intuitionistic point of view. Wright (Wright, 1986) has hinted at this when he established that in order to see that a certain statement follows from a given set of other statements given a certain logic L, $_{L}\ \Gamma \rightarrow \varphi$, it is not necessary to compromise with the logic. It is possible to agree that φ is deducible (or follows) from Γ according to a given logical system L, without subscribing to the view that L is the logic that underlies a given mathematical practice; where in our case Γ would be the premises in a mathematical proof. From this point of view, it could be claimed that the situation posed by Shapiro in his toy example has to be solved by taking the dispute in front of the tribunal of the mathematical community since, however it works, it seems to be the case that mathematicians come to an agreement as to whether something obtains or does not obtain.

A somehow different problem comes up when, for instance, a classical logician or mathematician wants to vindicate classical logic and classical mathematical practice saying, against the intuitionist, that the latter should not be revised. The problem now is *external* to the theories at stake, and verdict will rely on the logical theory one entertains.[11] Note that this problem also arises in case the assumed epistemology is that of wide reflective equilibrium since it can be the case that different logics allow us to attain the desired state of equilibrium.

At this point one issue that may be relevant to address is whether the evaluation of these theories actually complies with the requisite posited by Wright (Wright, 1992) on theory-ladenness:[12] To sustain that one is in front of a problematic case of theory-ladenness one has at least to make a case that reports of correct mathematical practice do not count as acceptable, or unacceptable, independently of one's logical beliefs (where logical beliefs are the ones underlying a given mathematical practice); that what is proper to report oneself as having observed as logically sound is a function of, inter alia, elements of logical theory which one carries into the observational situation. Note that the logical theory one carries into the situation includes semantic assumptions

[11] Moreover, this situation poses an added problem: that we have to use a logic in order to decide whether classical mathematical practice should be modified.

[12] Wright distinguishes four cases of theory-ladenness and points that only the one depicted above is problematic. (See Wright, 1992, pp. 160-161.)

that determine, for instance, whether the underlying notion of truth is a transcendent or epistemically constrained.

Different logical models are frequently compared in order to determine: which provides the best account of the way mathematicians reason; which deductive mechanism is easier to use for a human/artificial logician; whether they are extensionally adequate (in the sense that they allow us to characterise the set of valid arguments); whether the language used has enough expressive capacity to formalise mathematical structures, and so forth. As a result, one logical system or another may be used depending on its adequacy to attain a certain goal. For example, intuitionists reject classical logic on philosophical grounds, not because it does not succeed in explaining the assumptions and methodology a mathematician employs. They take it that classical logic cannot be an adequate model of mathematical practice because it does not provide a suitable epistemology. Classical logicians argue that intuitionist logic is unacceptable because it mutilates classical mathematics, but constructivists in general and intuitionists in particular contend that given Bishop's claim (Bishop, 1967) —that a constructive reconstruction of analysis sufficient for science has obtained— intuitionistic logic succeeds in accounting for the interesting part of classical mathematics. Classical mathematicians reject the latter because, even if what constructivists contend were the case, that part of classical analysis that can be explained in intuitionistic terms is not adequate for certain foundational purposes. Besides, Shapiro (Shapiro, 2000b) claims that intuitionistic logic does not answer to the epistemological problems either, because the capacities the mathematician has to have in order to develop intuitionistic mathematics do not correspond to our real capacities as human beings.

If we analyse these arguments and counterarguments it can be seen that: i) the dispute between intuitionists and classical logicians may be seen like a difference of opinion between a descriptive account of classical mathematics (a prescriptive practice) and a prescriptive account of a prescriptive practice; ii) the best argument classical logicians have waved against intuitionists is that their account mutilates classical mathematics, in other words, that it forces us to significantly reduce our body of mathematical knowledge and that knowledge has proved very useful to other disciplines such as physics; iii) constructivists, such as Bishop, reconstructed part of classical mathematics trying to solve mathematical problems in a way that allowed us to compute the result; furthermore, it is claimed that that part of classical mathematics they succeeded in reconstructing is enough to account for that part of mathematics which is relevant to science.

Now, let us analyse the different moves in the argument above. Clearly intuitionists and classical logicians have tried to solve the dispute putting forward the import that maintaining one position or the other has for mathematical practice. But of course, determining whether a given mathematical practice is right depends, to a certain extent, on the philosophical thesis we assume. This is so because our intuitions or judgements about the correctness of a given argument will differ depending on which are our philosophical views. Hence, there seems to be a problem with theory-ladenness here: when we have to decide whether classical logic or intuitionistic logic is

the underlying logic of classical mathematics, our assessment of which logic is the right one heavily depends on the intuitions we assume in the evaluations. This is where Quine-Putnam's indispensability argument comes in. Its incorporation to the discussion above allows us to say we have a justification for classical mathematical practice that is independent of judgements of mathematicians. The problem is that the justification available does not comprise the whole of classical mathematical practice but only that part that is indispensable for science. Let us agree —for sake of argument— that Bishop's developments allow for an intuitionistic reconstruction of part of classical analysis and that that fragment of classical analysis obtained by intuitionistic means is enough for those aspects of classical mathematics the working mathematician not in logic related fields needs. If both parties agreed to the fact that both intuitionistic and classical logic can be seen as accounting for 99 per cent of classical mathematics is all there is to this story, we would have a case of two extensionally equivalent though conceptually different accounts of that part of mathematics that is relevant to the working mathematician not in logic related fields (Feferman, S., 2000, p. 402).[13] But, that is not the position of mathematicians working in logic-related fields and philosophy of mathematics. They reject Feferman's claim because only classical non-constructive mathematics is enough for foundational issues.[14] And the same goes for classical logic.

Let us consider the issue in terms of the Wrightean criterion of 'wide cosmological role'. The wide cosmological role test is one criterion proposed by Wright in order to determine whether a given discourse is objective:

> "A subject matter has a wide cosmological role just in case mention of the states of affairs of which it consists can feature in at least some kinds of explanation of contingencies which are not of that sort, explanations whose possibility is not guaranteed merely by the minimal truth aptitude of the associated discourse" (Wright, 1992, pp. 197-8).

Our issue would at this point be the objectivity of mathematical practice. Given that, as it was said above, that part of (classical) mathematical practice that features in scientific explanations is the one both classical logic and intuitionistic (or constructive) logic can account for (assuming Bishop's result), it is precisely that part of mathematics that satisfies the wide cosmological role requisite. Therefore, as things stand at the moment, that part of classical logic which is not constructive cannot be claimed to satisfy it. Nonetheless, it is not clear that "non-constructive mathematics does not satisfy the wide cosmological role requisite since it is not necessary for physics" it is not temporally indexed. The issue is whether we *do* know that no more mathematics than that 99 per cent that can easily be formalised in ZFC will ever be applied, for instance to the development of more powerful models of physics. Imagine that it were.[15] In such

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[13] That 99 per cent of classical mathematics that is of interest to the working mathematician can easily be formalised in ZFC and that is all mathematics that is currently being applied in science.

[14] That is not so because that that 99 per cent is enough is established from outside mathematics and mathematicians do not judge the adequacy of their theories by, for example, the fact that they are used in physics.

[15] Some seem to be thinking that it is going to be.

a case, classical logic would feature in some kind of explanation of contingencies that are not of classical mathematics, whose possibility is not guaranteed merely by the minimal truth aptitude of constructive mathematical discourse. In other words, classical non-constructive mathematics would be said to have a wide cosmological role. And again, the traditional contention against intuitionistic logic would be in place. For the time being, applying the wide cosmological role criteria also leaves us unable to decide.

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Concha MARTINEZ VIDAL is associate professor at the Department of Logic and Moral Philosophy of University of Santiago de Compostela. Her main research areas are philosophy of logic and philosophy of mathematics. She has published "Is Second-Order Logic Logic?" (*The Logica Yearbook 1999*, 2000), "The view of Logic as model: Comments on Stewart Shapiro's paper" (*AGORA*, 2001) and "Naturalismo y Lógica" (*Palabras y pensamientos: una mirada analítica*, 2003).

ADDRESS: Dpto. de Lógica y Filosofía Moral. Universidad de Santiago de Compostela. 15782 Santiago de Compostela (Spain). E-mail: lflpcmav@usc.es.