

BETWEEN FORM AND FUNCTION
SOCIAL ISSUES IN MATHEMATICAL CHANGE

Eduard Glas

Until quite recently, philosophers of mathematics understood their own role primarily as a logical one. Their object of study was not informal mathematics, but the rational reconstruction of what was assumed to be its formal equivalent. Since mathematics was widely believed to be a formal science *par excellence*, correspondence between the logical substitute and its original was more or less taken for granted. Philosophy was almost exclusively concerned with the language of the product of mathematical research. It aimed at developing the logical organon of what was perceived to be the time- and culture-independent rationality of the enterprise. The history of mathematics was invoked as a vast reservoir of selected examples of the various modes in which the rational method operated to produce its logically irrefutable results.

However, historical scrutiny has since brought out that major mathematical achievements in the past would have to be qualified as hopelessly irrational when judged against what philosophers conceived as the rational method. On the logical-philosophical view of mathematics, it was hard to see how the indubitable logical quality of mathematical results could be reconciled with the seemingly so poor logical quality of the reasonings of those who had produced them. Yet, when philosophical methodology is to possess any relevancy at all for the practice of mathematics, it must be able to account rationally for the factual ways in which the tremendous body of knowledge had been acquired. Logic could furnish excellent answers to many questions, but in this case they were the wrong questions.

As Lakatos¹ has impressively shown, the logical approach was based on a false premise. Concerned with establishing conclusively the logical foundedness of proved results, it excluded beforehand that mathematical knowledge could also develop through conceptual change. Hence historical development could only be conceived, in a thoroughly ahistoric way, as the steady

accumulation of absolutely established propositions. Lakatos's main insight was that mathematical claims to knowledge, even when logically irrefutable, are not indubitable, and therefore liable to improvement. He thus rehabilitated the role of rational debate in mathematics, the critical method of improving knowledge by the 'dialectic' of proving and disproving.

Logical irrefutability is in this new conception no absolute virtue in itself. Mathematics is irrefutable in the sense that no external experiences, however recalcitrant, can force us to give up mathematical claims to knowledge. Informal counter-examples against formally proved results can always be put aside inasmuch as they are no logical counter-examples. But the (non-logical) counter-examples that they supply can be turned into heuristic examples for the construction of richer and profounder theorems. They are therefore crucial to our ability of improving our knowledge.

Thus the preoccupation of philosophers with mere foundational questions has come to be challenged in much the same vein as Popper, Kuhn, Hanson, Toulmin and others have challenged the received view of natural science.² Lakatos has shown that mathematical knowledge, too, is not absolute and eternal, but changes in a dynamical process of development. His methodology³ specified rational criteria for judging the progressiveness of research-programmes, especially in comparison with rivals.

The methodology was intended to be normative of good mathematical practice. It derived this pretence from its alleged superior value as an explanatory theory for the history of mathematics. This allegation was given the following sense: more of the actual history could rationally be reconstructed in its terms than in the terms of any other methodology known so far. Thus Lakatos met the demand, stated above, that philosophical methodology should account rationally for the history of its field of application, and leave as little as possible to external contingencies.

The new methodology was a dramatic improvement but no radical break with the logical approach to the philosophy of mathematics. For it still took a logical substitute rather than 'real' history for its testing ground. It avoided the false premise of the older approach, but depended itself on the no less contestable premise that mathematics develops through the application of methodologies. Indeed, only on this assumption could reconstruction in methodological terms be claimed to be an 'explanation' of the history of mathematics (and science generally).

In the present paper I want to challenge this assumption and to examine the philosophical consequences of its rejection. I intend to survey the ways in which mathematical changes have actually taken place in some crucial phases in the history of mathematics. The relevant questions are: (1) what have been the grounds for mathematicians to settle on certain conceptual constructs and argumentative modes rather than others, and: (2) through what sort of processes have changes of conceptual and argumentative frameworks been effected. Elsewhere⁴ I have elaborated primarily the historical and methodological dimensions of these cases; on the basis of the results of these previous studies, I now want to examine the role of social and external processes in the development of mathematics.

Two cases of conceptual variation

Case I

The opposition between the analytic and the synthetic approach to mathematics in the first half of the nineteenth century is well-known. In mathematical physics, Gratton-Guinness⁵ has traced a dichotomy between what he has called "analysis" and "calculus/descriptive geometry" oriented types of practice. Less well-known is that both distinctions were rooted in a cultural clash, in the period of the first French Republic, between the established analytical tradition, guided by Lagrange and Laplace, and a new, geometrically-oriented approach, swayed by the revolutionary upstart Monge.⁶

These mathematicians had been assigned by the government to normalize and rectify mathematics to a perfectly transparent and hence universally learnable 'language'. It was believed that mathematics could, and had to be purged from all remnants of human speculative imagination, regarded as the source of enduring obscurantism and ideological forgery.

We thus have here a case of leading mathematicians being invited to expound expressly their basic views of the nature, scope and language of mathematics, to be imposed normatively on the new generations of French mathematicians. When we embrace this splendid opportunity to compare their basic views mutually, highly interesting differences come to light which turn out to be incommensurable, as I will now demonstrate.

Lagrange and Laplace⁸ seem to have adhered rigidly to the terms of their assignment. They set out to free the discipline from all enduring informal, quasi-empirical and intuitive ele-

ments - which had been their intention anyway, as avowed supporters of the formalistic trend initiated by Euler, "*notre maître à tous*". They saw mathematics as a computational device for the edification of the mechanistic System of Nature, the legitimacy model of a free society based on Natural Law, in the service of Enlightenment. In spite of considerable differences in direction and emphasis, Lagrange's 'formalization of analysis', and Laplace's 'analytization of rational mechanics', were complementary projects within the same program of linguistic enlightenment.⁹ Mathematics was conceived as a formal language. All residual non-lingual elements, in particular all appeals to geometrical imagery and other intuitive models, were considered impurities, blemishes. Its ultimate aim was the deduction of all phenomena from a limited set of basic principles expressed in the language of mathematical analysis. Mathematics had to be construed as a linguistic device to perform formal operations upon uninterpreted symbols according to formal rules. The superior value of analysis, says Laplace, is that it transforms reasoning into mechanical procedures.¹⁰

Monge¹¹, too, emphasized the linguistic uses of mathematics, as required by the terms of his assignment, but expressly rejected the reduction of mathematical reasoning to a formalism.¹² He insisted on the indivisibility of form and content, and denied that any rules, mechanical or otherwise, could be given for the conduct of mathematical investigations.¹³ For him, analysis was not a language, closed in itself, but merely the 'script' for the notation of reasonings about quasi-empirical, especially geometric contents.

Monge's new geometry was in the first place an exact and systematic theoretization of still incoherent and disconnected collections of heuristic rules and techniques of practitioners. It was certainly not an application in the pejorative sense of a theoretically inferior derivative device. On the contrary: it furnished invaluable new tools for the exploration of new problem fields with considerable bearing on even pure analysis. The program was progressive in the (Hallett's)¹⁴ sense that it solved problems far outside its proper domain, problems for which it had not been specially designed, and which seemed rather to belong to Lagrange's and Laplace's spheres of competence.

However, the latter analysts strongly disapproved of the informal ways in which Monge had arrived at these results, and *a fortiori* of his enduring reliance on geometric imagination and argumentation in the demonstrations and proofs thereof.

The 'same' mathematical facts were perceived very differently.¹⁵ Any purely analytical argument was read by Monge as a

discourse about spatial transformations, whereas for Lagrange and Laplace it constituted a mere series of formal rearrangements of uninterpreted signs. Any geometrical demonstration, on the other hand, was regarded by the analysts as mere pictography for illiterates in mathematics, whereas for the geometer it represented what mathematics was ultimately about. Since their perceptions were altogether different, they lacked a shared basis of consensus for assessing the relative merits of the two approaches.

Monge's accomplishments could not be measured by the standards of Lagrange and Laplace. His methods bespoke the practitioner by their appeal to unformalizable and inalienable skill and craftsmanship, and his emphasis on the informal transmission of mathematical skill and proficiency through precept, example and practical exercise. It is a case of a confrontation between previously neatly demarcated practices, now competing for the recognition of their distinguished professional competences and technical skills. The two groups pointed to different kinds of professional credentials and ideological motives to substantiate (in the case of Lagrange and Laplace) or to upgrade (in the case of Monge and his school) the value of their own occupational activities and attainments, presented as the pre-eminently trustworthy body of knowledge and proficiency to render disinterested services to the glory of the Nation.

In all essential respects, the two schools entertained incompatible views, for instance on:

the nature of mathematics:

formal language (LL)/quasi-empirical science (M)

its method:

formal procedures (LL)/informal imagination (M)

its object:

formalization of reasoning (LL)/exploration of the world (M)

its value:

logical perfection (LL)/functional expansion (M) of knowledge

The ways in which the two schools conceived of the nature, method, object and value of mathematics belonged to different frameworks which embodied the accredited accomplishments, competences and skills of the group. No rational arguments could force one party to concede to the other on penalty of irrationality. And so each party judged mathematical methods, approaches and developments in terms of what these achievements implied with respect to the value of the precious attainments of

its own practice.

Case II

The primary aim of the Lagrange-Laplace program had been to achieve logical perfection and purity through the expulsion from mathematics of enduring speculative ingredients (*e.g.* infinitely small quantities) and quasi-empirical remains (*e.g.* geometrical intuitions). These ideas were taken over by German mathematicians¹⁶ who were concerned about the professional autonomy of their discipline in the social turmoil of the period, and were devoted to its emancipation from subservience to science and practice. They declared mathematics to be a strictly internal affair of the profession, a humanistic value in its own right, irrespective of any practical uses. They believed that the exclusion of extra-mathematical concerns was the ultimate insurance against political and other exogenous threats to their authority. Professionalization went hand in hand with 'division of competences': only through compartmentalization of the discipline into disparate and self-contained specialties could the very highest technical standards be observed. About the middle of the nineteenth century, a variety of conceptions about purification, formalization and rigorization had come together in the Weierstrassian program of arithmetization.¹⁷

On the other hand, the proliferation of alternative geometries, in the wake of the 'Mongian Revolution', gave rise to fascinating new perspectives on their logical status and interrelations, and to crucial new insights into basic problems of formal logic.¹⁸ Our modern understanding of the structural nature of mathematics owes particularly much to the work of Weierstrass's adversary Felix Klein (1849-1925).

Klein regarded himself as a disciple of Monge, whose background and general outlook he shared, and whose intellectual descendants (among them Pluecker) had been his teachers and models. Raised in the emerging technical-industrial scene of the Ruhr-area, and trained primarily as a mathematical-physical scientist, the unity of science, mathematics and technology was for Klein a matter of course. He later declared never to have understood how there could be any contradiction between them.¹⁹

"Weierstrass's position was one of absolute authority...taken as the unassailable norm. Doubt was not allowed to arise, verification barely possible. Striving after his ideal of *Lueckenlosigkeit* (completeness) he proceeded in such a way that he ...had to refer back only to himself".²⁰ Although fully acknowledging

Weierstrass's merits as far as the solidity of the logical frame of mathematics was concerned, Klein abhorred the frame of mind that went with it. His ideal was "to encompass, through a comprehensive unitary idea, the opposing views of different schools of thought".²¹ He consistently kept upholding the unity of all science and mathematics against excessive differentiation into formally closed specialties.

In the '*Erlanger Programm*', Klein²² showed how every geometry can be construed as the invariant-theory of its characteristic group of space-transformations. It is a single outstanding example of the structural unification of previously disparate geometrical endeavours on the basis of an originally algebraic concept²³. Although subsequently Klein's classification has turned out not to be all-embracing, it nonetheless supplied a highly progressive framework for the systematic investigation of new geometries. It was also progressive in the sense that it threw new light on, and solved problems far outside its originally intended (geometric) domain, for instance in algebra and the theory of functions.

So in all essential respects Klein took a position that was diametrically opposed to the current trend towards ever more rigorous withdrawal within purely self-contained and "*lueckenlos*" closed specialties. He declared himself to be unable to conduct a geometrical investigation purely logically, without continually keeping the intended models in mind. Intuition-free formalism being of little avail, he posed against it the refinement of intuition by axiomatic procedures.²⁴ The axioms provide a solid and exact frame to mathematical knowledge, which is first arrived at intuitively, especially through careful inspection and investigation of quasi-empirical or even physical models. Any decision to accept or reject a proposed set of axioms thus depends on how well it represents and corresponds with the informal models we had in mind in the first place. Moreover, the intended models continue to guide developments, even at the formal level.

This point is illustrated most clearly by the way in which Klein was able to elaborate and improve upon Riemann's theory of functions of a complex argument.²⁵ Riemann's accomplished formal exposition started off with the definition of the concept of analytic complex function as solution to a pair of partial differential equations under certain initial and boundary conditions. In this form, the theory was hardly understandable to the contemporaries, let alone liable to successful use and expansion. However, as Klein showed, the initiating formula, required for computational command of the field, was no starting point at all

but, on the contrary, the final *outcome* of many toilsome and laborious informal investigations! The clarification and development of the theory required that the physical models, which supposedly had led Riemann but were suppressed in the formal account, had to be 'rediscovered'. Klein succeeded in finding models for the theory (planar currents on curved surfaces) and on their basis was able to explain, elaborate and expand it.

The example is rather typical: the informal origins of the theory were in the formal exposition 'disguised' as definition, as 'linguistic convention' which failed to refer to any external reality whatsoever. Yet, informal models are not only the necessary preludes to mathematical knowledge, to be forgotten once the results have been codified in definitions, propositions and proofs. As Klein demonstrated, they continue to be indispensable for our abilities of achieving progress even at the formal level.

Incommensurability in mathematics

The process of mathematical development as exhibited in our case-histories, has two distinct faces. One necessary, though not sufficient, component, consisted in attempts to capture with formal rigor and completeness the entire problem range of well-defined and neatly demarcated, established fields of study. It aimed at logical perfection and professional excellence, necessary to ensure the 'soundness' of mathematics as a self-contained, autonomous and unassailable discipline, immune against all social, political and other external contamination. This goal was perceived to be attainable only in specialistic fields, in which rigorous standards of achievement were acknowledged and applied. To be a competent mathematician meant, therefore, that one was committed to a 'sound' specialty, and did not meddle with the interdisciplinary questions in the grey areas between specialties, where the appropriate rigor was simply not to be had, and purity (absence of contamination) was not attainable. This rigorous concentration on formal procedures, though necessary to protect mathematics from incoherency and inconsistency, was inherently insufficient for mathematical progress. Any formal exposition of (the content of) mathematics must rely on the availability of one common and universally agreed-upon language or vocabulary of terms and concepts to serve as the subjects and predicates of the propositions. Formalists had to perceive conceptual variation as a direct assault of pure and respectable professional standards.

Substantial progress in mathematics demands a complemen-

tary, heuristic approach, focussing, as did Monge and Klein, on the explanatory and unifying fertility of conceptual systems rather than on formal derivability. Typically, endeavours of the latter kind lack the conventional form of series of definitions, propositions and proofs, so characteristic of the formalist approach. They proceeded quasi-empirically, and were concerned with meanings and explanations at other conceptual levels, whose merits could not be captured in merely formal procedures. Aiming at theoretical reinterpretations and innovations of mathematical language, they envisioned entirely new perspectives on the true bearing of hitherto disconnected and fragmentary practices. They sought to reveal their common features so as to subsume them under a unitary conceptual and argumentative system, which explained both their potentialities and limitations systematically in its new terms, and showed the way to expansion over fresh fields of inquiry. The rationale for 'generalization' in this (counter-specialistic) sense was that it greatly enhanced the understanding of previously unconnected results as consequences of the same limited set of more fundamental, comprehensive and profound principles.

Monge and Klein constantly exceeded the bounds of established disciplines, and moved freely between geometrical and analytical, and between mathematical and physical approaches. Monge, for example, insisted that even the most complicated operations in analysis can geometrically be represented, and that any geometrical transformation can be accounted for in the idiom of mathematical analysis²⁶. He used analytic and geometric methods indiscriminately: he for instance translated the properties of tangent lines and planes to families of surfaces into the language of partial differential equations, in order to study the latter by analyzing the 'movements' through which these surfaces could be generated²⁷. This shifting between models was equally characteristic of Klein, for instance in his work on new geometries, complex functions, and algebra (he for instance studied the solvability of the general fifth-degree equation through investigations into the group of rotations of the regular icosahedron²⁸). Besides supplying forceful new exemplars to enhance the practice's problem-solving efficacy, these 'transgressions' of the sanctioned boundaries between specialties and even disciplines provoked important shifts in the ways in which mathematicians understood reflexively their own activities.

The two approaches were 'incommensurable' in the sense that none of the parties could with the force of logic be compelled to give up the values embodied in its own way of doing mathematics. Although all would agree that mathematics progresses

through the enhancement of problem-solving efficacy, through the expansion of its scope and the unification of its language, as well as through the increase of formal rigor, these extra-paradigmatic values are apparently still too equivocal to guarantee convergence of judgement. In the cases considered, the perceptions of the mathematicians involved were too severely constrained by their different evaluative frameworks (which gave the highest credit to the attainments and competences to which each party could lay claim), to reach a common basis of judgement. It is the *practices* that conflicted, and that made mathematicians to confer rational preference on different *kinds* of theories, explanations, problem-solutions and other achievements. Typically, the truth of propositions was not at stake; there simply was no agreement about what was worthwhile to be proved and what counted as a sound argument of the proper kind. At least in our mathematical cases, incommensurability was not connected with radical meaning variance, 'intranslatability' of propositions across paradigm-boundaries. On the contrary, each party was perfectly capable to explicate the results of the other in its own terms and concepts. Hilbert, for instance, reinterpreted Klein's structural insights into set-theoretic language in order to show that geometry was reducible to analysis and algebra. More generally, formalistic mathematicians felt that Cantor's proof of the isomorphism between the set of real numbers and the geometric continuum justified them to consider geometry only a special case or 'application' of pure mathematics. It goes without saying that Klein regarded it precisely as the ultimate justification of his enduring reliance on the use of geometric models and arguments.

So the failure to reach consensus was due neither to formal impediments nor to radical intranslatability of meanings. Logic did not force the parties to maintain and highlight the differences rather than to settle on a common basis of judgement. However, neither could logic force them to shift their positions and thereby seriously to impair the values embodied in their own kind of practice. No rational dialogue about proofs and refutations can make these crucial phases in the development of mathematics internally accountable for. Although mathematical achievements, and even value-judgements about them, may be susceptible to rational debate, that does not warrant rational convergence of judgement about enduring issues of discord. How, then, can a settlement of the conceptual variation be reached at all?

The settlement of variation

No more than 'Logic' or 'Observation', is 'Society' to be regarded as an independently given basis for explanations of mathematical development. Mathematics is part of society, a social activity embedded in, and indissolubly intertwined with it.

For example, it may be said that without the military imperatives in the perilous early years of the French Republic (1792-94), Monge's approach could not have overcome the competition of the established practice.²⁹ On the other hand, Monge himself had first shown convincingly that the skills and competences incorporated in his mathematical practice were of decisive value for the Republic (in fact, its very survival in the crucial years 1792-93 was to a large extent due to Monge's good offices in the technology and organization of the revolutionary armament industry³⁰). It was therefore itself a political factor that tipped the balance of power in favour of the political situation that offered the best opportunities to his specific way of doing mathematics. For one thing, it put him in a position to create after his own image the 'Ecole polytechnique', on which the civilian and military prospects of the new society were to depend heavily. It offered him the opportunity to recruit students who were likely to remain committed to his framework, because it embodied the very skills and proficiencies on which they depended as resources in the highly competitive field of contemporary mathematics. Indeed, the new approach offered splendid opportunities for gifted students to pioneer into new fields of study and to make a name for themselves (as for instance Poncelet did with projective geometry). Nothing remotely similar could be offered by Lagrange and Laplace. Apart from fundamental research, the new geometry was practically important for the new, industrially organized technical practices based on division of labour, as a common 'language' and tool' for cooperation between the various participants.³¹ In the wake of the Napoleonic conquests, pupils of Monge's obtained influential positions at technological institutes, modelled after the Polytechnique, which were founded in a number of European capitals. Here again new pupils, prospective adherents to the Mongian program, were recruited, and so on and so forth.

As these examples make clear, it is rather futile to make a clear-cut distinction between internal and external, and between cognitive and social factors. The sense of any program is precisely that it links the cognitive level of objective knowledge to the social level of the community of practitioners who bear this knowledge and try to advance it by committing themselves to

shared programmatic principles. Besides expressing the cognitive contents of the practice, the program incorporates the evaluative framework of shared meta-views, concepts, argumentative modes and standards of achievement. It is essential both for the coordination of the activities of the practice and for its transmission.

The development of a program cannot be explained on cognitive, nor on social grounds alone. As described above, the rise of the Mongian program was due, apart from its inherent cognitive merits, to a favorable combination of political, military, professional and educational opportunities. However, these opportunities were not merely external contingencies, but were in part also products of the activities of the practice itself. They were due to social structures and processes that the practice itself had helped to shape because it took part in them. Its impact derived from the formation of a strong and fastly extending network, a 'coalition' of practitioners committed to the framework because it embodied their common professional interests and intellectual resources for competition. But besides social interests, they had also perfectly rational reasons (among them theoretical progressiveness and practical problem-solving efficacy) to support the program. Others, however, had equally rational reasons (logical perfection) and social interests (professional excellence, authority and autonomy) to adhere to the rival program, and hence logic alone could not force an unambiguous decision between them.

Incommensurable practices may still share many essential standards of mathematical achievement. The 'individual' cognitive merits of 'individual' achievements can rationally be appraised and compared, and even agreed upon. However, rational consensus about 'individual' achievements as such is no warrant for agreement about the overall value of the whole program or practice. For any 'individual' judgement is still made relative to the 'collective' framework which, like the Kuhnian 'paradigm', embodies the practice's perception of the mathematical universe, together with the normative forms of problem and patterns of reasoning. To participate in a practice means to be committed to a coherent body of basic assumptions, standards, values, etc. all in one, not just a loose set of disconnected claims. Hence, different practices may acclaim the same 'individual' merits, but assign them to qualitatively different degrees on their inherent value-scales, and give different interpretations of how they bear on the cognitive goals of the discipline.

Between Laplace, Lagrange and Monge, for instance, was general agreement as far as individual achievements were con-

cerned. Monge was regarded as a very eminent geometer indeed, but geometry itself was considered to be of minor value for what Lagrange and Laplace perceived as the pure body of mathematical doctrine. Monge did not deny the tremendous merits of the analysts in point of formal rigor and logical perfection, but regarded their endeavours as formal exercises rather than as veritable contributions to what he conceived as substantial mathematical progress.

There are many universal rational standards to which all or most mathematicians subscribe, but they become effective only if embodied in the social setting of a program, as an evaluative referential framework for the coordination of individual mathematical activities. As we have seen, such universal mathematical standards of achievement as progressiveness, widening of scope, improvement of problem solving efficacy, and even 'truth' are still too fluid and ambiguous in themselves to enforce rational convergence of opinion.

When for all human purposes the consensus of the mathematical community is the ultimate criterion of rationality in matters mathematical, there can be no cognitive constraints on the judgements of mathematicians apart from those imposed by the mathematical community itself. In particular, there can be no independent rational criteria to enforce agreement when the consensus has (temporarily) broken down. In that case, the mathematical community has a problem in exercising its cognitive authority and autonomy on the basis of rationality. The coherence, strength, size and power of the social networks, and opportunities to interact favorably with the sociopolitical environment, may come to play a more prominent role.

The point is very clear in the cases we have considered. The Mongian program, for example, owed its success to the military imperatives in the early republican era, which gave it a dominant position in the Ecole polytechnique, the very best position to form rapidly a strong and extensive network. Laplace formed his own (informal) circle of adherents under his patronage, the Society of Arcueil, which has been described as prototypical of a social structure founded expressly on commitment to the same (Laplacian) research program.³² From its inception, the group was favoured by Bonaparte himself. After his *coup d'état*, the political climate changed rapidly. Especially under the Empire, the scientific prestige of these more 'formal' mathematicians of 'higher standing' came to overrule the factors that had brought Monge's mathematics to eminence. The Emperor Napoleon relied completely on the almost absolute mathematical authority that Laplace had continued to exercise over 'notable' men of science.

On the latter's instigation, the Ecole polytechnique was thoroughly reorganized and militarized (1806)³³ in order to bridle the egalitarian and republican spirit that Monge had breathed into it.³⁴ The new program was set up in the Laplacian fashion around analysis and rational mechanics (including celestial mechanics), and geometry was confined to a position of considerably lesser standing.³⁵

It belongs perhaps to the irony of history that the decline of Laplacian science, and of the Paris Ecole polytechnique with it, was later regularly attributed (by Klein, amongst others) to its pre-occupation with applied analysis at the cost of pure mathematics.³⁶ The rise of pure mathematics in Germany was initiated by educational reforms, which reflected both the abstracter ideals of the new philosophy of science (*'allgemeine Wissenschaftslehre'*), and the political-utilitarian interests of state officials.³⁷ Its success undoubtedly derived from professionalization and specialization, reflecting the industrial division of labour. As a matter of fact, the German mathematicians felt that this strong association of mathematics with the utilitarian goals of industry and politics was a constant threat to their authority and professional autonomy. They consequently upheld the values of non-utilitarian, value-free research into highly formal and abstract problems. They understood their own activities primarily in terms of the German cult of *'Gruendlichkeit'* (thoroughness and rigor) and idealism, and presented themselves as the bearers of the typically German penchant for abstraction and formal perfection. Independence from productive forces was absolutely essential to their self-image, and hence they perceived an insurmountable gulf between their 'pure' endeavours and the empirical-practical activities of science and technology.

Especially after the unification of the State under Prussia, and the demonstration of its military superiority in the Franco-Prussian war, there had arisen a strongly felt need to reassert the historical identity of German culture, and to understand its economic, political and military role as an expression of its scientific *'Geist'* (spirit).³⁸ The universities were increasingly pressed to adjust their research and training to these needs. The new situation therefore offered good outlets for Klein's alternative view of mathematics as essentially a historical process of learning from practical and functional experience.³⁹ In particular, he understood specialization as an inevitable consequence of the professional division of labour. From the outset, he had intended his program to compensate for the highly undesirable loss of universality, offering a unified and coherent framework for cooperation between pure and applied, mathemati-

cal and physical-technological specialists, and for coordination of these activities.⁴⁰

From the 1880's onward, Klein was increasingly involved, ultimately as a full-fledged 'statesman of science', in matters of educational reform and science-policy.⁴¹ Reversely, the very considerable changes that, mostly on his instigation, were effected in the entire German educational system, still enhanced the relevancy and value of his views. Understandably, academic mathematicians, who had to face sharp losses of social esteem and cultural-political influence, interpreted Klein's activities as a political program, and responded accordingly. In so doing they made it perfectly clear that the struggle was ultimately about values and norms that extended far beyond the sphere of inner-mathematical discourse. In this connection, it is revealing to read what the appointment advisory committee for Weierstrass's succession wrote to the Minister: "...candidates must be apt to continue instructing the students in serious and selfless immersion in the problems of mathematics. On these grounds we had to leave personalities as professor Klein at Göttingen (born 1849) out of consideration, about whose scientific merits the judgements of scholars are very divided, and whose entire activity in publication and teaching is in contradiction with the above-mentioned tradition of our University."⁴²

Conclusion

The materials presented support a pluralistic (rather than relativistic) view of mathematics. The idea that all mathematical development leads up to one universal and unequivocal cognitive goal (Truth), has to be rejected. Complexes of variable cognitive purposes are involved, some of them mutually incompatible. Attaining these purposes may require different methods, some of them mutually conflicting. When cognitive goals and methods for attaining them are at variance, so are the standards of mathematical achievement. This does not imply necessarily a lack of consensus about methodological criteria in so far as they are specifiable *in abstracto*. The problem is not that practising mathematicians regularly violate explicit methodological rules, it is that such rules are used implicitly in the local and temporal practice under very different interpretations. The actual methods of working mathematicians receive their meaning from substantial, contextually dependent problems, perceived in a framework of goals, norms and values. The actual methods and modes of reasoning are therefore mostly incomparably more subtly

shaded and richer of ideas than could ever be specified *in abstracto* by philosophical methodology. Consensus at the abstract level of methodology does not warrant agreement about value judgements as to how particular mathematical achievements are factually to be appraised. Mathematics is not just 'applied methodology of mathematics'.

It is certainly of great philosophical interest to specify methodologies that can rationally account for the most part of the factual history of mathematics. But from this must not be concluded that, conversely, rational reconstructions in methodological terms explain the historical development of the discipline. Lakatos's reconstruction, for example, is about the utmost that has been achieved in the way of historically sensitive philosophical methodology. It reconstructs history at the level of Popper's world-III, in terms of research programs following a higher 'Logic' than that of the world-II reasons of the historical figures themselves. This may be allright for methodology, but historical understanding requires that the local and temporal circumstances, with all their contextual dependencies, be given full regard. At the level of the actual mathematical practice, *i.e.* the way of 'doing' mathematics, abstract methodological reflections may be elucidating, but putting them 'into practice' implies taking a number of interpretative steps that are not logically warranted and therefore may give rise to incommensurability of practices.

Besides theoretical pluralism, our cases support also social and cultural pluralism. Society, too, is to be regarded as a pluriform conglomerate of very diverse social entities with sometimes contradictory intellectual, cultural, economic, professional, *etc.* interests. *Qua* organized activity, mathematics is thoroughly entrenched in the professional, intellectual, educational, economic, *etc.* sections of society. It derives its relative autonomy and authority from the stability of this social settlement. Revolutions in society (political, industrial or otherwise) can therefore easily disturb also the established order in mathematics. Practices with conflicting interests will form new alliances and affiliations in order to substantiate or upgrade social recognition of the values of their particular ways of proceeding. They will tighten the bonds of shared partial interests or partially parallel objectives with occasional allies, and so connect their fate with the political ups and downs of the 'parties' to which they are affiliated.

Everything else being in flux, mathematical 'law and order' cannot remain unaffected. When the stable social basis of its authority has fallen away, the 'objective' truth and unique

'rationality' of the established doctrine cannot fail to show its nature as a social construction. A multiplicity of latently or patently conflicting practices, at variance with the 'objective' doctrine, and in violation of its 'rational' standards, will enter the lists. Revolutionary mathematical progress, measured against whatever methodological standards one may want to apply, seems to derive its impact from the critical confrontation of the established doctrine with conceptual and argumentative alternatives. Being mutually incompatible and sometimes even incommensurable, these alternatives cannot possibly all agree with one and the same methodological theory of the development of mathematics, however liberally conceived. No explanation of mathematical development in terms of one single coherent and consistent methodology is possible. Hence no set of rational criteria, imposed normatively by some methodology at the exclusion of other (non-rational? irrational?) considerations, can fulfil its promises. Methodologies are surely important for mathematics: they may help to question taken-for-granted views, to discover hidden assumptions and logical errors, to suggest ways to improve on established doctrines, and to stimulate the critical use of creative imagination in the generation of alternatives. But no monolithic theory of rationality, pretending to cover all times and situations, will fail to inhibit rather than to stimulate the progress of mathematics.

Neither is this process accountable for in terms of (non-rational) social 'causation'. Like all other human endeavours, the development of mathematics is a form of socially organized activity, characterized by certain styles, attitudes and norms, directed and coordinated through variable perceptions of the goals and values of the enterprise. Progress depends to a large extent on the creative proliferation of alternatives, which challenge the consensus in the mathematical community. On account of the incommensurability of the norms, values, attitudes, *etc.* embodied in conflicting practices, external social relations have to be mobilized to settle disagreements that cannot be decided by logical arguments alone. As my case-histories were intended to exemplify *in extenso*, rational reasons are never without social interest, and social processes not without rational reason. The question whether 'reason' or 'society' is ultimately decisive for mathematical development, rests on a false dilemma.

NOTES

1. Lakatos [1976]
2. Survey in Tymoczko [1985]
3. Lakatos [1970], [1978]
4. Glas [1986], [in press]
5. Gratton-Guinness [1981]
6. Glas [1986]
7. Dhombres [1980]
8. Cf. Dhombres [1980], Ovaert & Sansuc [1976], Grabiner [1981]
9. Glas [1986]
10. Laplace [1796], pp. 464-466
11. Cf. Taton [1951], Loria [1921]
12. Glas [1986], pp. 256-261
13. Monge [1811-12], p. 15
14. Hallett [1979]
15. Glas [1986], [in press]
16. Cf. Jahnke [1987]
17. Cf. Also Kitcher's case-study of the development of analysis in Kitcher [1984], pp. 229-271
18. Nagel [1939]
19. Klein & Riecke [1900], p. 225
20. Klein [1926-27], Bd.I, p. 284
21. Klein [1921-23], Bd.I, p. 52
22. Klein [1974]; cf. Hawkins [1984] and Tobies [1981]
23. Cf. Wussing [1969]
24. Klein [1924-28], Bd.I, pp. 82-92; [1911]; p. 42
25. Klein [1921-23], Bd. III, pp. 499-573
26. Monge [1811-12], pp. 16, 75
27. Monge [1809]
28. Klein [1884]
29. Gratton-Guinness [1981], pp. 349-70
30. Cf. Taton [1951], pp. 33-37
31. Cf. Paul [1980]
32. Crosland [1967]
33. Olivier [1850], pp. 64-68. Cf. Taton [1951], p. 42
34. Olivier [1850]
35. Booker [1961]
36. Cf. Fox [1974]
37. Cf. König [1970], pp. 27-40
38. Opening remarks at the first general session of the *Gesellschaft deutscher Naturforscher und Aerzte* (1871), cf. McCormach [1974], p. 160
39. Cf. Glas [in press]

40. Cf. Klein & Riecke [1900], pp. 19ff and especially the *Nachwort*
41. Cf. for these activities: Manegold [1970]
42. Biermann [1973], p. 207

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