

History of Geomorphology

From Hutton to Hack

*Edited by
K. J. Tinkler*

*The Binghamton Symposia in Geomorphology:
International Series, no.19*

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Preface

TERRA INCOGNITA

it will be some time before historians and philosophers awaken from their dogmatic slumbers and see the history of geology as an area with broad implications for the study of science generally.

(M.T. Greene, 1985, p. 97.)

It is a great pleasure to present this book, the proceedings of the *Nineteenth Annual Binghamton Symposium*, to the geomorphological public to commemorate and honour both the publication of James Hutton's extended essay *Theory of the Earth* in the 1788 *Transactions of the Royal Society of Edinburgh* (their very first volume), and the twentieth century career of John Hack of the *United States Geological Survey*, whose particular contribution to geomorphology has been an explicit formulation of an equilibrium theory of landscape development (Hack 1960). Although Hack is approximately two centuries later than Hutton eighteenth century 'geologists,' especially in Britain, might not have been surprised to read a theory committed to viewing landscape as a state of equilibrium achieved within a system of competing forces, as I hope I demonstrate in my own essay (Chapter 3).

I am convinced that it constitutes a volume worthy of both individuals for it is the first set of essays whose determined focus is the history of geomorphology, although it is by no means the first book on the history of geomorphology. For good measure those few of us who have written books on the history of the discipline (and many of whom were at the Symposium) are frequently taken to task in this volume for having neglected substantial fields and significant areas of concern! We can all retort that nothing in our own essays can be found in our books, and that surely is very satisfactory!

At present geomorphology is showing signs of out-growing a lengthy adolescence and may soon adopt a formal international structure which will be

both independent of, and competitive with, similar structures in geography and geology to which its parent bodies belong. This move is likely to be seen as one of great significance by those involved in its organisation and by many beyond it, and it may well signal structural changes within the discipline whose actual outcome and meaning will not be clear for several decades.

My perspective on such a move is cool, because the adoption of an historical perspective with respect to the practice of 'geomorphology' in times past brings the recognition that times present are in no sense different. That which seems appropriate, fundable, official and achievable is just that: the essays of Stoddart (Chapter 8) and Hewitt (Chapter 9) both neatly illustrate, in their different ways, the distinction between 'official' and 'scientific' approaches to the same problem. But institutional expansion, Vitek's paper notwithstanding (Chapter 14), may not be why our geomorphology will be of interest to future generations. What that interest will be is a matter for speculation. Will it be contemporary interplanetary equivalents of the great explorations of the nineteenth centuries? Will it be our ability to collect vast quantities of real-time process data, and to manipulate it with mathematical models and GIS systems, on computers whose memory capacity is already outgrowing our imagination? Might it be that we have achieved disciplinary consciousness and have begun to write histories of ourselves? Or will it be our realisation that despite an almost total failure to communicate significant quantities of information between major language groups, we have nevertheless managed to muddle our way to a reasonably coherent picture of how the world's surface works? (see, for example, Starkel's essay, Chapter 12) We cannot know: if we did we could be prophets. We must keep an open mind, and we should read the opening essay by Gordon Herries Davies not just for its splendid rhetoric, but to realise that its logic is irrefutable.

I hope the reader will approach these essays with an equally open mind and try to share with the authors their sense of wonderment and empathetic understanding. Gerald Friedman (1988), in a recent editorial in *Earth Sciences History*, points out the tensions and difficulties that exist between geologists and historians when either sets out to write the history of geology, or any part of it: the chauvinistic views that the one is short on history, and the other is short on geology. The inevitable consequence is that either, or both, may miss something of significance that the other has to offer!

Yet this is particularly paradoxical since geologists have every reason to be sensitive to the difficulties of reconstructing a convincing and reasonable historical narrative - even if their means and materials are different. I should add as a corollary to this that Martin Rudwick's book *The Great Devonian Controversy* (Rudwick 1985) is of particular interest because here we have a geologist, long since turned historian of geology, who has used a narrative technique in a strict form for a specific historical purpose: to relate in strict chronological sequence the events which led to the uncovering of a major gap in the chronology of the geological column itself.

In one view, historians of science can be placed on a polarised scale with the externalists at one end, the internalists at the other. The former stress the external structural apparatus of society and nation that shape the actions of men, even men of science, whereas the internalists focus on the internal debates within particular sciences. In discussions of the nineteenth century, for example,

the externalist would stress the national need to push westward in North America, with the consequent outcome of substantial exploration and the development of state and federal surveys. These, they would argue, gave rise incidentally to valuable geology. The internalist writing about the same century might describe the plethora of theories for mountain building (Greene 1982), or the debates about the reality and extent of Ice Ages.

It may be of interest that the funding which brought the speakers (herein the authors) together for the *Nineteenth Binghamton Symposium* was supplied by the *Social Science and Humanities Research Council of Canada*, and had to be obtained in competition from a panel of historians, not geologists. That a grant larger than the average was obtained attests to the fact that these issues were addressed satisfactorily in the application.

I should like to be able to claim that this volume has a coherence and drive entirely of my own design - but that would be to distort the truth considerably. I did attempt to get a broad coverage of topics and times, of spaces and places. In my initial choice for speakers I approached individuals whose published record was plain for all to see. But I was also approached by others who had a particular paper to offer. The outcome is a happy amalgam of these two components. Each paper was refereed by a historian of geology, and the list of referees at the start of the book speaks for itself. Because I was able to attract Mott T. Greene as the principal discussant for all the papers, and because he was able to see a copy of every paper before the conference, I had additional and valuable input from that source. Finally, all this would be to no avail if the authors had not been willing to make considerable modifications in the light of the comments received. They *were* willing, and they often went to considerable lengths, as many will testify.

All of this is part and parcel of the usual process of peer review, it may be said, but in the context of the strong ambivalence that exists about how the history of science should be written, and the intrinsic differences between historical and scientific writing, remarkable concessions were asked and granted. For this I thank both authors and referees and offer the opinion that a good deal of the middle ground was covered quite amicably once the referees asked the right questions, to which usually the authors could provide penetrating, and to them, obvious replies.

The volume has coherence and drive, but it comes from a serious scholarly effort on the part of everybody, and not because of any planned content that I foresaw. The common themes that emerge do so because they are latent in the material. They are not obfuscated by hero worship or a pre-judged position, and the freshness which characterises the collection stems, in large part, from the fact that these matters have not previously been aired. I shall not review the papers in detail, they are all too complex for that, and Mott Greene provides a perceptive afterword from his position as discussant. However, I will cite a two examples.

From at least four papers (Hewitt, Sack, Stoddart and Alexander) we see how pre-conceived views are brought to a problem by the scientists themselves. It may be an Anglo-American prejudice about how geology should be done, an imperial, frontier, approach to the delineation of territory and problems, or a technological bias in which every available technique is brought to bear on a problem, and must perforce provide a 'better' (equals more detailed?) result.

Nevertheless the landscape shines through - it forces new perspectives on those who try to pry loose its secrets.

As a second example consider the eighteenth century. It is almost a commonplace in the history of geology that the subject emerged in the period 1770-1820 - both in name and as an institutional discipline (Porter 1977, Laudan 1987). It is reassuring to find, then, in my own contribution (on British writers), from that of François Ellenberger (on the French), and from work in Italy described by David Alexander, that there is ample evidence, in sources not previously examined, of capable field workers developing a convincing regional and temporal perspective on landscape evolution from the evidence of their eyes. They used well established rules of reasoning, and the power of their own imaginations. From the same sources we see that the popular misconception of that century as one in which writings on 'geology' were entirely composed of cataclysmic or biblical theories is in dire need of revision. Thus we can provide from geomorphology an independent thrust propelling the inception of geology, and one which shows that a golden age in geology did not arise from a few gifted minds, or by suddenly replacing misguided theories with rational insight.

Before I conclude I should like to say few words about our handling of the paper by François Ellenberger. We decided when doing the translation that all the original citations should be given in the original eighteenth century French, as well as in translation. And we extended this to individual words in order that nothing should be lost, for although the words are often identical after translation the reader may not otherwise know this. Many of the citations are from manuscripts not readily available except in Paris, and in view of the originality of the material we thought it best to make them as widely available as possible, at the expense of being pedantic. The orthography has been checked very carefully by Professor Ellenberger against the originals, and the reader is warned that accents were treated more freely in the eighteenth century than is now the case, and obviously there are differences in style, as well as spelling, from modern French. We should point out too, that in common with eighteenth century English writing on 'geological' matters, there is frequently a total lack of a technical vocabulary particular to the subject. Thus the translation, in preserving the character of the original, will also preserve its circumlocutions. In addition, it will be apparent that Professor Ellenberger is frequently paraphrasing closely some of his sources, without actually citing them *verbatim*. Where this is the case we have stayed close to the eighteenth century style in making the translation.

I should like to end with a few words about James Hutton (1726-1797), Hutton was born the year before Newton died, and he died the year that Lyell was born. When I chose the year 1988 for this Symposium I did so deliberately, with the knowledge that it was the bi-centennial of the appearance in the *Transactions of the Royal Society of Edinburgh* of Hutton's *Theory of the Earth* paper. It was partly to expand, explain and rebut contemporary criticisms of this paper that Hutton wrote and published his book *Theory of the Earth* in 1795, with the original paper as the first chapter.

It was never my intention to use the Symposium as a platform for a critical review of Hutton's work; to have done so adequately would have taken us far beyond geomorphology, and would have required a different cast of speakers and writers. Nevertheless, you will find that several authors make reference to

Hutton in their efforts to place him properly within the milieu of late-eighteenth century science, and one author (Dennis Dean, Chapter 4) considers specifically his subsequent influence on the development of our subject and traces his impact as far as William Morris Davis.

We should do no service to Hutton, or to ourselves, if we heaped uncritical adulation on his name. In consequence I ask you all to keep an open mind on what his achievements may really have been, and to re-read his work not just for the insight he had on the workings of the world, but also with an eye to the constraints that the eighteenth-century world placed upon him. Then ask yourself what constraints the present century places upon our perceptions of the world.

Keith Tinkler

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On the nature of geo-history, with reflections on the historiography of geomorphology

Gordon L. Herries Davies

Indeed I am, perhaps wrongly, inclined to look upon all geological theories as having their being in a mythical region, in which, with the progress of physics, the phantasms are modified century by century.

(Letter from Alexander von Humboldt to Louis Agassiz, Berlin, 2 March 1842, Quoted in Elizabeth Cary Agassiz, *Louis Agassiz : his life and correspondence*, London, 1885, volume 1, pp. 345-6).

Julius Cæsar's Gaul was divided into three parts; so too is my paper. In the first part I will discuss the significance which the year 1669 must hold for us. In the second part I will suggest that we have not thought sufficiently deeply about the methodology which earth-scientists have employed since 1669. In the the third part I will pose the question 'What is the import of these conclusions for the historiography of geomorphology?'

Part the first

I begin in a spectacular location - in the Basilica of San Marco in Venice. In the atrium of the Basilica one of the cupolas is adorned with a magnificent thirteenth-century mosaic dedicated to the *Genesis* story, and it is beneath that cupola that I wish you to join me. Above us we see beautifully depicted all the familiar events recounted in the three opening chapters of *Genesis* down to the expulsion from Eden. Nearby another series of mosaics carries the *Genesis* story forward in a glorious sequence illustrative of Noah, the Ark, and the events of the Flood. Those mosaics encapsulate the history of the earth as that history was understood within the Christian World for a period of more than a millennium and a half. It was an interpretation of geo-history which displayed, and which, through Creationist Science, continues to display, a remarkable quality of

durability. That interpretation of geo-history was, and still is, grounded upon the Scriptures, and there we have the key to an understanding of its durability. It is an interpretation of earth-history which rests upon the secure foundation of a supposedly divinely inspired text itself possessed of eternal validity. It is an interpretation of geo-history which therefore holds for the faithful a satisfying immutable quality. But, as we all know, during the second half of the seventeenth century the Scripture-based interpretation of geo-history began to face a challenge - a challenge arising from the emergence of a novel idea which eventually was to revolutionise the writing of the story of our earth. To us that idea appears to be both obvious and simple, but in the seventeenth century the idea seemed to be startling in its originality. The idea was simply this: first, let it be assumed that the rocks, minerals, fossils, and landforms of the earth's surface bear trace of the historical vicissitudes through which they have passed, and second let it be assumed that human observers may learn to decipher those historical messages so as to compile for themselves synoptic accounts of geo-history. In short, the idea was to treat rocks, minerals, fossils, and landforms as lenses through which we might peer into the past.

This was the novel approach pioneered in Nicolaus Steno's *Prodromus* of 1669¹ and Steno's familiar series of six diagrams depicting the history of the Tuscan landscapes forms a fascinating contrast to the history of the earth as it is depicted in the mosaics of the Basilica of San Marco. In view of Steno's effective advocacy of this fresh approach to earth-history, I have elsewhere suggested that this revolutionary ascription of historical significance to rocks, minerals, fossils, and landforms should be the subject of eponymy and should be known as 'THE STENONIAN REVOLUTION.'² That is a term which I propose to employ here, and lest there be any doubt I will reiterate that THE STENONIAN REVOLUTION is to be defined as the emergence and general acceptance of the notion that from rocks, minerals, fossils, and landforms there may be read an account of past events in the history of the global surface.

It is through our adoption of the principles of the Stenonian Revolution that we have discovered a powerful alternative to the Scripture-based interpretation of geo-history. It is through our adoption of the principles of the Stenonian Revolution that we have been able to compile the wondrous tale which the modern historian of our earth has to tell - a wondrous tale of marine transgression and plate collision, of fiery vulcanism and icy glaciation, and of fearsome reptiles being catastrophically exterminated by the arrival of bodies from space. This we now hold to be the true pattern of geo-history. It is the kind of history which is taught in our classrooms and which is enshrined in our literature; it is the kind of history which is demonstrated at the interpretative centres of our national parks and which is presented upon our screens in the latest T.V. science spectacular; it is the kind of history which constitutes the very warp and weft of our prevailing paradigm within the earth-sciences. And this entire ornate geo-historical superstructure rests upon a principle enunciated

¹ *De Solido Intra Solidum Naturaliter Contento Dissertationis Prodromus*. An English translation by Henry Oldenburg was published in London in 1671 under the title *The Prodromus to a Dissertation Concerning Solids Naturally Contained within Solids*. Another translation, by J.G.Winter, is in *University of Michigan Studies: Humanistic Series*, XI, part 2, 1916, pp. 165-283.

² The term was first used in a lecture delivered at the William Andrews Clark Memorial Library in Los Angeles on 3 November 1984. The lecture will shortly be published by the library.