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## **Status of Geomorphologic Research on South America**

The intensity of geomorphologic research on South America by North American investigators seems to be inversely proportional to the distance between a given study area and the United States. A relatively large number of purely geomorphologic studies have been made in Mexico, the Bahamas, and Cuba; and several can be singled out for Central America and northern South America. Only a few deal with middle South America and even less pertain to the extreme southern part of the continent or other remote areas such as the Galapagos and Malvinas.

This erstwhile neglect of distant lands has been counterbalanced to an extent by our European colleagues whose travel seems to have taken them to some of those areas where we have been least active. Consequently, in order to achieve some degree of completeness in the regional coverage of geomorphic research in Latin America foreign sources must often be consulted, most of them either German or French.

### **Previous and related research**

If we divide geomorphologic processes into the conventional categories involving fluvial, glacial, eolian, marine, and volcanic phenomena then it is evident an imbalance exists in the amount of field research in South America. While it would be inaccurate to imply that any of these subjects has achieved adequate attention, it might be useful to point out that there are certain neglected categories where much remedial work will be needed in the near future, particularly with regard to the gathering of observational data for comparative study based on existing new theories.

The disparity in research emphasis indicated elsewhere in this paper is made especially apparent by the example of fluvial processes. South America contains some of the world's most important rivers -- it would seem only logical that earth scientists would have already made maximum use of the opportunity to study these drainage systems -- particularly so when we consider that many of these rivers remain in a relatively unaffected primeval state. This condition exists

nowhere in the United States.

By comparison there is a surprising amount of contemporary literature on eolian subjects. Much of it is of reportorial nature (g.g. Smith 1956, 1963; Simons and Erickson 1953; Kinzl 1958) and seldom incorporates more than a descriptive account, Exceptions can be found in the studies of Hastenrath (1967) . Gay (1961), Amstuta (1958) and Finkell (1959) all of whom have applied meaningful quantitative techniques to the dunes of southern Peru. Their interest can be attributed partly to the publications of earlier writers such as Sears (1895), Pampeckj (1906), Douglas (1909), and Barclay (1917) who drew attention to the presence of these features. Bowman (1916, 1924) deserves special mention for his classics on the Peruvian Andes and the Atacama desert.

We also can point to a scattering of important articles on glacial, periglacial, and alpine morphology in general. They have been almost exclusively concerned with conditions in South America, although the effects of Pleistocene glacial cycles can still be observed in parts of Mexico and Central America, and the still active glaciers of the Sierra Nevada de Santa Marta have been practically ignored by us.

The publications of Kinzl (1933, 1942, 1958) have added greatly to our knowledge of former and present glaciation in the Peruvian Andes, an area in which French geomorphologists have recently been highly productive (Dollfus and Treart 1959). Oppenheim (1945) briefly notes evidence of Peruvian glaciation which he treats in the traditional manner. Paskoff (1962) has made a much-needed comparative study of Pleistocene glaciation in the Andes and Atlas Mountains. Among the best studies ever conducted on the Pleistocene of the Andes are those of Hammen (1960, 1961) in Colombia, based mainly on palynological work.

Since the reconnaissance expedition of Lieutenants Herndon and Gibbon (1854) for the United States Navy, only a few hydrologic reports of the lower Amazon drainage have appeared. With more than a dozen tributaries of major proportions, each having myriad confluents that are equivalent to our own Ohio or Missouri River, it is appropriate to say that essentially nothing is known about these systems other than the fact that they exist. Our best maps do not show their courses accurately. Major diversions of the Ucayali, for example, now make it impossible to use WAC charts for VFR navigation in parts of eastern Peru, and the cultural data shown date from the rubber boom at the turn of the century.

When we consider the unique character of the Patagonian rivers and the peculiar

behavior of the Bermejo-Pilcomayo-Salado complex in northern Argentina, it seems curious indeed that they have not been studied. The world's bloodiest (per capita) war was recently fought over an issue involving a gross misunderstanding of the physical character of the Rio Parana. Other first order systems that remain essentially unknown include the Orinoco (especially the delta with its unusually high tidal range) and its tributaries in Colombia and Ecuador.

### **Some problems**

An analysis of the Pan American Institute of Geography and History roster of professional geographers who have indicated an interest in Latin America provides a significant insight into the problem of scientific manpower. Of the 113 respondents to a questionnaire, only 3 admit to having a special interest in geomorphology. Of course, it is obvious that these figures can be rather misleading. From my own limited contact with fellow Latin Americanists it seems likely that these roster figures should be increased four-fold to approximate the true status of man power interest in this field.

Still, if there actually are 50 earth scientists interested in Latin American geomorphology probably no more than half are actually undertaking field work. This means that if we appointed "Coordinators of Geomorphologic Research" beginning with Mexico, there would not be sufficient interested scientists in this nation to assign one to each Latin American country. There is nothing especially remarkable in this observation -- it merely reflects the fact that out of some 1,400 registered United States geographers, only a small percent have any special knowledge of any foreign area at all.

Many questions are still unanswered with regard to karst formations found distributed throughout the Caribbean islands, and parts of South America (Urbani, 1969). New insights in this field are long overdue and will probably depend upon an entirely different approach, possibly involving detailed stratigraphic and petrographic analysis of the original carbonate facies environment. So far comparative studies on a world-wide basis have not produced an acceptable unifying theory explaining genesis of karst landforms.

One of the most difficult unresolved problems in Latin American geomorphology revolves around the subject of Pleistocene glacial fluctuations. Recent developments in absolute dating techniques (e.g. Emiliani, 1957, 1966; Broecker, 1969; Donn, 1967) have polarized thinking along two radically different lines.

Topographic evidence from other parts of the world has been variously interpreted and presented to cross-purposes depending upon the viewpoint of the writer. What little work has been done in Latin America along these lines has largely been couched in terms of the traditionalist four-part European system (c.f. Dollfus, 1965, and Kessler, 1963). We now have a new theoretical framework for the reconstruction of multiple Pleistocene glacial fluctuations and there are abundant opportunities to test these hypothesis by carefully controlled field work in South America.

The development of alluvial geomorphology in the United States has reached a point where we now know a great deal about the character of large, meandering rivers and their floodplains. Fisk and Russell (1955) have thoroughly demonstrated the role of clay plugs just as Leopold et al. (1964) have done much to clarify the dynamics of river hydrology. Unfortunately, these findings have not been extended to any of the major river systems of Latin America where it is obvious that some anomalous conditions exist, particularly in the Amazon basin. Here is a challenge of truly herculean proportions.

Lack of accurate field data is largely responsible for our present misunderstanding of geomorphic conditions in the South American shield areas. Weathering and heavy vegetative cover tend to obscure the details in many parts of the Guianas and Brazil. However, recent air photos of the relatively open areas where the Brazilian shield extends into northeastern Bolivia reveal that we need to drastically reorient our thinking, both geologically and geomorphologic ally. These photographs show that the peripheral shield zone has not experienced the degree of diastrophic stability we traditionally associate with these provinces of ancient crystalline rock. Quite to the contrary, they reveal several phases of intense tectonic activity in the form of intersecting fault trace sets, folded, superposed sedimentary outliers and -- indeed, evidence of what appears to be late Tertiary or Pleistocene volcanic activity. So far virtually all information pertaining to these features has remained unpublished (e.g. Wegman, 1959; Craig, 1959) although Denevan (1962, 1966) and Plafker (1964) have done field work on the outermost fringes as did Troll (1936) and Wilhelmy (1958). These investigations only mention in passing the existence of such features as "lajas" (exfoliating granite outcrops) and in some cases (e.g Plafker) give physical explanations for what actually may be man-made features.

The interior of the Gran Chaco is another neglected area. Again it is a case where difficult accessibility and evident lack of economic potential has discouraged

exploration. This ignorance of even topographic detail in what constitutes one of the major physiographic provinces of South America is reflected in the fact that most maps and all geologic treatises ignore the presence of substantial mountains (e.g. Cerro Capitan Ustares) in what is considered to be an erosional basin.

This discussion of geomorphic problems in South America has been conducted so far from a purely academic viewpoint. For intellectual reasons these are perhaps some of the problems we should like to see solved - but it is very doubtful that the course of events in Latin America will permit us to indulge ourselves in these splendid puzzles. It is much more likely that we are going to be forced to address ourselves to more prosaic problems such as those dealing with tropical agriculture, the formation of laterites, and the processes associated with the formation of tropical soils. Land use surveys (Romney, 1959) and resettlement schemes (Drewes, 1961) should have high priority among concerned Latin governments. It is inevitable that the central problem will soon become that of defining the optimum utilization of these finite areas. Ground water studies represent a closely related need and are of much concern in parts of Peru, Chile, Brazil, and the Caribbean. Here then are the subject areas in which geomorphologists must assume responsibility, concentrate their research, and make their contribution.

## **Priorities**

From the foregoing summary of existing literature we note that the development of geomorphologic knowledge on South America has been very uneven. In some areas the difficulty has been one of quality rather than quantity. In no areas, however, do we seem to have reached a level of achievement equivalent to that found in other continents. Two concurrent avenues of improvement may be suggested.

First of all it is clear that a profound systematic compilation and synthesis must be made of all that has been learned to date in these countries. This project is self-defeating if carried out by only a few specialists. The corpus of publications is already so large and diverse that constant addition of new data would render the ongoing summary obsolete before it could be completed. As a first approximation to the manpower needs of this particular task, I would suggest that at least eight or ten geomorphologists be selected to cover those areas where they have special knowledge, interest or experience. The division should be on a geographic rather than subject basis for the initial effort. Evaluations by subject area specialists

could then be made from these preliminary reports.

Secondly, it is obvious that we must further by all means the availability of maps and aerial photographs. We already have an excellent inventory of photo and map coverage in the form of a series of atlases prepared by the Pan American Union (but even some of these highly useful references are now in need of updating).

At this point it is appropriate to digress for a moment to comment on the status of geology in Latin America. Substantial progress in geomorphology cannot be made independently of geology since these disciplines are inextricably interrelated. Relatively speaking geology is in a robust state of health. But unfortunately there is a great discrepancy between what is known and what is published. This situation is due to the retention of Victorian business principles now utterly intolerable in a world struggling to achieve environmental equilibrium. To speak more plainly let me say that immediate steps must be taken to unlock the vast storehouse of geological data presently guarded by mining and petroleum companies some of which have been operating in Latin America for over a century.

I propose the formation of an international commission to receive, compile, summarize and translate this wealth of information now mouldering uselessly in secret company files, guarded in the hope that it some day may provide a financial advantage through barter, trade, or sale to competitors. Hundreds of geologists have labored down through the years to explore, sample, and map vast areas of Latin America. Their prodigious efforts have resulted in thousands of regional maps, reports, and field notes that seldom are of any benefit to the nation involved or to the scientific community in general.

This same commission can be charged with the responsibility of advancing the status of air photo coverage and large-scale geologic mapping. It is necessary to point out, however, that if air photo coverage, mapping, and (ultimately) geomorphic analysis are not considered on the basis of their potential socioeconomic importance then the project as broadly envisioned here will certainly fail. We have neither the hemispheric resources nor sufficient nationalistic interests to insure cooperation with a plan that ignores political and economic realities for idealistic academic aspirations.

Assuming for the moment that the argument just presented is valid and the basic development plan worthy of implementation, the question becomes how shall it

be administered? Who is going to do all this work and how is it going to be done?

The status of geomorphic research on South America is, in the final analysis, a function of research in South America. This in turn is a reflection of the efficacy of higher education in these countries. Well, we have ample evidence that the Latin American educational system as now constituted does not lead to prominence in the earth sciences. The reasons are well-known and involve cultural attitudes that are extremely difficult to change. But, if we are sincerely interested in fostering development of this kind in Latin America it should be obvious by now that it will be necessary to invoke some drastic changes.

The first change has to do with research policies that exist in North American Universities. We simply must stop our mounting of grandiose, institute-based, multi-disciplinary research programs in underdeveloped countries. This scientific paternalism has been used as a means of "fostering" growth in the social and earth sciences for nearly a century and we have abundant proof that it does not produce a satisfactory result. If it did, we would not be holding this symposium. Furthermore, I can predict that if we continue and increase our patronizing efforts along presently constituted lines it will slowly but inevitably result in the total atrophy of creative initiative in the scientific personnel of these Latin nations. To make the point even clearer let me ask, for example, how many Peruvian scientists participated in the Hiram Bingham Yale expeditions of 1911-1914? How many Argentine or Chilean scientists were present on the I.G.Y. projects of 1957? Have Colombian geomorphologists been involved in selection of the new Trans-Isthmian sea-level canal? The answers respectively are: none, a few, and no.

The system we now have may result in wonderfully well-trained personnel in the sponsoring nations but does little or nothing to encourage independent research by indigenous investigators. Somehow we must find a way to withdraw our overwhelming presence, train the local people in the latest techniques, and stimulate them to attack the infinite array of scientific problems in their own country.

On the other hand, there is no reason why North American scientists who have special interests in Latin America should not receive direct, tangible support by both governments concerned. But this policy should be followed only as long as the results of these efforts are fully reported and made available to the appropriate sponsoring academic or governmental agencies of the host country. At the same

time, every reasonable effort should be made to incorporate national coworkers into these programs where it is practical to do so.

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