method of volume discontinuity. In his apparatus, the pressure was transmitted to a specimen of the pure metal in direct contact with the piston, the distortion of the cylinder containing the specimen being determined experimentally. From measurements of the force transmitted to the piston and the area over which the piston acted, the pressure could be calculated. It is difficult to estimate the accuracy of the pressure measurement; but the total frictional effect obtained by measuring the transition point for both increasing and decreasing pressure was about 6 per cent.

By using the fixed pressure points, the change in resistance with pressure of manganin wire can be expressed as an equation of the second degree. Although the departure from linearity depends upon the history of the wire, experience shows that, in general, the deviation is only about 2 per cent at 25,000 bars. The gauge can be made very sensitive to small pressure-changes when used in conjunction with suitable electrical measuring apparatus; but it will be obvious that its accuracy depends almost entirely upon the absolute accuracy with which the fixed pressure points are known.

At pressures much above thirty thousand bars, all available pressure-transmitting liquids either become very viscous or freeze, and the manganin gauge can no longer be used. Pressures in this range have, therefore, to be measured directly, it being assumed that, in a thin specimen of material under pressure, stresses sufficiently close to hydrostatic conditions can be realized. Corrections for distortion of the apparatus have to be computed; and, since the material is often stressed beyond its normal elastic limit, no great accuracy can be claimed for the result. At pressures of 100,000 bars, the error is probably of the order of \pm 10 per cent.

Thus it can be seen that the error involved in the measurement of pressure is a function of the pressurerange under consideration. Below twelve thousand bars, it is possible to increase the precision of a freepiston gauge by reducing the distortion of the piston and cylinder assembly; this can be accomplished by using the stronger materials of construction now available and by providing more effective external support for the cylinder. It is doubtful if the range of the gauge can be greatly extended, because the large increase in viscosity of pressure-transmitting fluid results in a serious loss of sensitivity. At pressures above thirty thousand bars, when all liquids have frozen, true hydrostatic conditions no longer exist and stress differences within the compressed solid of the order of its plastic shearing stress are to be expected. In this range, the error in the pressure measurement resulting from the variable frictional forces opposing the motion of a piston is greater than that due to the uncertainty in the distortion of the cylinder. Thus, any attempt to increase the accuracy of the measurements must be directed towards reducing the frictional forces. One method of doing this is to decrease the cross-sectional area of the material under compression; such a procedure, however, makes the accurate assessment of volume changes during compression very difficult, especially if the apparatus undergoes appreciable creep.

Although Bridgman measured the freezing pressure of mercury more than forty years ago, no accurate redetermination has been made since then. Indeed, the value has been accepted and used to calibrate various primary and secondary gauges, some of which have a greater sensitivity than the accuracy

In view of the difficulties involved in the direct measurement of pressure, it would therefore seem desirable for the present to base all work upon an arbitrary standard scale of fixed pressure points which can be corrected from time to time as more accurate data become available.

THE SOUTH ATLANTIC: LAND BRIDGES OR CONTINENTAL DRIFT?

`HE proceedings of the symposium on "The Role of the South Atlantic Basin in Biogeography and Evolution", held by the Society for the Study of Evolution in New York during December 28-29, 1949, have now been handsomely published under the title of "The Problem of Land Connections across the South Atlantic, with Special Reference to the Mesozoic''*. Most of the contributions are remarkable-considering the speculative possibilities and heady enthusiasms inherent in the subject-for their scientific rigour, and several of them are of fundamental importance to everyone who is interested in the tangled complex of problems which come under discussion. These problems are essentially twofold : whether the southern continents received all their faunas and floras from the north or have been at times in direct contact with each other; and whether, if such contact existed, it was effected by land bridges or by contiguity, afterwards broken by continental drift. The chairman and editor, Dr. Ernst Mayr, points out that the symposium was organized, not to defend or disprove any particular hypothesis, but for the presentation and discussion of current evidence in one restricted but critical field of a much broader subject. In his conclusion he cheerfully admits that we are still confronted by many unsolved problems; but he claims with ample justification that the symposium has resulted in a much-needed correction of fact and clarification of concept. Dr. Mayr himself recalls the methodological principle that biological conclusions must be based on biological evidence, and geological conclusions on geological evidence. In particular, he directs the geologists' attention to the many pitfalls that lurk in biological interpretations-for example, in conclusions drawn from faulty classifications or from underestimates of the dispersal powers of organisms over long periods of geological time.

Dr. G. G. Simpson's illuminating discussion of "Probabilities of Dispersal in Geologic Time" (pp. 163-176) is one of the high points of this invaluable publication. Simpson deflates many a speculative balloon by stressing the impossibility of inferring two simultaneous conclusions from what is actually known of the geographical distribution of a given group of organisms-namely, the land connexions of the time, and the probabilities of dispersal for the organisms concerned. To obtain a solution for either unknown,

*Bulletin of the American Museum of Natural History, Vol. 99, Article 3: The Problem of Land Connections across the South Atlantic, with Special Reference to the Mesozoic. (Proceedings of the Symposium on "The Role of the South Atlantic Basin in Biogeography and Evolution", held at the Fourth Annual Meeting of the Society for the Study of Evolution, New York City, December 28 and 29, 1949.) By E. Mayr (editor), M. Ewing, W. H. Bucher, K. E. Caster, C. O. Dunbar, M. Kay, G. G. Simpson, D. I. Axelrod, T. Just, W. H. Camp, P. J. Darlington, jun., A. E. Emerson, B. Schaeffer, D. H. Dunkle, E. H. Colbert and A. S. Romer. Pp. 79-258+2 plates. (New York: Amer. Mus. Nat. Hist., 4952.) 3 dollars.

it is necessary to postulate either the existence (or absence) of land connexions or the probabilities of dispersal. Usually the latter postulate is made, the implication commonly being that p is either 0 or 1. Simpson illustrates the very serious errors that may arise from confusing 'very low' with 'zero' and points out that estimates of probabilities, even when least subjective, may be wholly invalidated by the effect of geological time. Just as probability of dispersal is affected by the size of the population, so the passage of time multiplies the opportunities for dispersal. "If the probability that some member of a population during the course of a million years the event would be probable, p = .63, and "in the course of 10 million years the event would become so extremely probable as to be, for most practical purposes, certain, p = .99995". Simpson concludes that evidences of random dispersals at scattered times indicate that probabilities of dispersal were so exceedingly low as to suggest the intervention of a major barrier. Ten years ago Simpson¹ directed attention to the relevant fact that, of the known Triassic reptiles of South America, only 43 per cent of the families and 8 per cent of the genera are known in Africa, with no species in common. "These figures," he concluded, "are decidedly inconsistent with any direct union of corresponding parts of South America and Africa."

Among other general contributions, Dr. M. Ewing presents a useful summary (pp. 87-91) of geophysical evidence (up to 1950) and concludes that both seismic and gravity data show the great similarity of the Atlantic crustal structure to that of the Pacific, and their great dissimilarity to the continental crust. So far as such evidence goes, it is consistent with the hypothesis of the permanence of continents and ocean basins. But the Atlantic data are still too sparse to justify regional conclusions. Moreover, the discovery near the surface of crustal layers having the elastic properties of simatic rocks raises the question of what has happened to the many kilometres of sediment that must have accumulated over the original ocean floor, if the latter has existed for $\sim 3 \times 10^9$ years. Have they been deeply buried by repeated coverings of basic lavas and sills, and metamorphosed beyond seismic recognition ? If so, their radioactive contents might account for the unexpectedly high heat flow encountered through recently tested parts of the ocean floor. If not, then the seismic evidence recorded by Ewing could be interpreted as against the 'permanence' and 'land

bridge' hypotheses and in favour of 'drift'. Prof. W. H. Bucher sets out to deal with the general problems of "Continental Drift versus Land Bridges" (pp. 93-103), but his treatment is largely confined to the thesis that the topography, depressions, mountain ranges and structural relations of the ocean floor cannot be harmonized with Wegener's assumption that the simatic crust is or has been so weak that sialic bodies could drift through it. The criteria are not strictly so decisive as Bucher claims, since he fails to consider fully the effects of metamorphism and altogether ignores the hypothesis of sub-crustal convection currents; nor does he even mention any of the evidence which favours the 'drift' hypothesis. Both Ewing and Bucher admit that vertical movements of considerable amplitude have demonstrably taken place, but Ewing limits the amplitude by the requirements of isostasy, while Bucher limits the area involved to that of a land

bridge. Neither considers the possibility that the sub-crustal silicate mantle may not necessarily be as homogeneous in either space or time as theorists postulate. Localized changes of composition or phase, or of both, at depths below the so-called 'level of compensation' would adequately account for the vertical movements which seem so puzzling to geophysicists.

Turning to more specific evidence on South America and Africa, Prof. K. E. Caster gives an admirable and well-documented review (pp. 105-152) of the stratigraphical and palæontological data up to the end of the Mesozoic. Like others before him, he is so impressed by the Devonian-Triassic parallelism between the opposing lands that he favours a broad continental linkage up to the end of the Triassic. Nevertheless, he inadvertently raises a serious doubt as to the validity of what Dr. A. S. Romer (p. 253) calls "the strongest single piece of evidence for South American transatlantic connections". This is the occurrence of the supposedly freshwater reptile Mesosaurus in similar formations in South Africa and Brazil. Caster, however, records that Mesosaurus occurs in a foetid limestone containing marine invertebrates, from which he infers that it was a nektonic animal inhabiting a seaway with a stagnant bottom. In a most stimulating discussion of Caster's paper, Prof. C. O. Dunbar makes the suggestion that, if Mesosaurus was thus adapted to a 'pelagic' life, it may well, like modern seals, have made long journeys to sea. In any event, it is improbable that migration could have been easy, since of the three African genera only one-and that of a different specieshas been found in Brazil.

Another important point made by Dunbar is that, in the light of our present knowledge, the pre-Devonian record has no bearing whatsoever on the problem in hand. This is amply confirmed by recent work in dating the Pre-Cambrian. Caster states that the presence of Collenia "affords the best evidence to date for correlating strata in the oldest terranes on the two sides of the Atlantic basin". Unfortunately, the African occurrences to which he refers are in rocks which range in age from about six hundred million years to more than two thousand million years². Dunbar also expresses the opinion that, until more is known about the reproduction of Glossopteris and its associates, it is scarcely necessary to drift continents or raise broad land-bridges to account for the distribution of this flora.

Prof. Marshall Kay also discusses stratigraphical evidence bearing on continental drift (pp. 159–162). His main point is to demonstrate from the distribution of Cambrian trilobites—on which weighty arguments have been based—that similarity of faunas does not in itself require close association later broken by 'drift'.

Dr. Mayr is satisfied (pp. 85 and 256) that there is no need to postulate former land connexions between South America and Africa to explain the distribution of mammals or birds, the available facts being "diametrically opposed to the possibility of such a connection". However, this clear-cut conclusion refers only to the late Mesozoic and Tertiary. For testing earlier connexions, fishes and reptiles are more suitable. Dr. E. H. Colbert reports fully on the Mesozoic reptiles (pp. 237–249). He thinks there may have been "a connection of sorts" during the mid-Triassic, but admits that no southern connexion would be essential if other evidence were forthcoming to prove that North and South America were then connected. The Jurassic evidence is inconclusive, while for the Cretaceous it is "rather unequivocally in favour of an active faunal interchange between South and North America". Discussing Colbert's paper, Dr. A. S. Romer (pp. 250-254) agrees about the Cretaceous and Jurassic but, although fully conscious that most hypothetical land bridges are not merely delusions but also snares to the unwary, he mildly disagrees about the Triassic. He writes : "I find myself here, after consideration of the evidence [which he carefully analyses] rather strongly inclined towards belief in the existence of a southern intercontinental connection between South America and South Africa in the Triassic. To my embarrassment ; for in such a 'leftish' position I am disturbed . . . by the company (of bridge builders, radical continent shifters and Gondwanaland collectivists) which this may entail". Dr. B. Schaeffer reports on the freshwater fishes (pp. 227-234), his conclusion-from what is admittedly a scanty record-being that there is no evidence of direct interchange between South America and Africa during any part of the Mesozoic.

In a most valuable paper on "Variables affecting the Probabilities of Dispersal in Geologic Time" (pp. 177-188), Prof. D. I. Axelrod illustrates his views with special reference to the effects of climatic and water barriers on angiosperm distribution. He claims that the continents have been in their present positions since the Middle Cretaceous and probably earlier : "The floras and the faunas, not the continents, have been moving during the past ages". Angiosperms, however, are not decisive for pre-Cretaceous times, and Dr. W. H. Camp's discussion of their phytophyletic patterns (pp. 205-212), though stimulating and suggestive, carries us no farther back. Dr. T. Just deals with the older and more critical fossil floras (pp. 189-204), but he finds the evidence insufficient for the drawing of firm conclusions.

In a brief survey (pp. 213-216) of living invertebrates in relation to Mesozoic South America, Dr. P. J. Darlington, jun., demolishes the idea that they provide any relevant evidence. In particular, he denies that paussids, which have been used as evidence of former land connexions, tell us anything significant about the history of South America. Prof. A. E. Emerson presents a highly original paper on "The Biogeography of Termites" (pp. 217-225) and reaches several tentative conclusions, the first being that "it is unnecessary to postulate any great change in continental masses and their connections since early Mesozoic times"

To end on a personal note : I should confess that, despite appearances to the contrary, I have never succeeded in freeing myself from a nagging prejudice against continental drift; in my geological bones, so to speak, I feel the hypothesis to be a fantastic one. But this is not science, and in reaction I have been deliberately careful not to ignore the very formidable body of evidence that has seemed to make continental drift an inescapable inference. In recent years the weight of evidence has become less oppressive, and this symposium has left me with the general impression that a few land bridges or linkages by island stepping-stones would probably suffice for the biogeographical problems. Matching of orogenic belts on opposing sides of the Atlantic is an exercise that has not been indulged in by the participants of this symposium. I have been collecting evidence from the Pre-Cambrian of South America, Africa³ and India⁴ for several years, and I think it is now safe to say that India cannot have been where Wegener placed it relative to Africa. The evidence on the other side, however, is still ambiguous. A remarkably good 'tectonic fit' between parts of South America and Africa can be recognized. but so far no correlation by age is possible, because the South American age determinations⁵ have not been correlated with the tectonics and have not been checked by isotopic analyses. No doubt this temporary frustration will be overcome, and the matter will be decided one way or the other. At least we know how the problem can be settled.

Meanwhile, there remains the most serious enigma of all : the Permo-Carboniferous glaciations. Dunbar points out that the late Palæozoic glaciations in low latitudes present "a problem still unsolved, unless we accept continental drift". But if we accept continental drift only to explain these and other still older glaciations, it becomes no more than an ad hoc hypothesis. As such, it may still be justified as a stimulant to research, but it may also stand in the way of progress by distracting attention from the real problem. Can the meteorologists not come to our assistance and tell us whether or not widespread equatorial and low-latitude glaciation is possible while high latitudes for the most part enjoy a genial climate ? The curious feature is that the evidence, so far as it goes, suggests that it was the distribution of ice in the Pleistocene that was exceptional, not the Permo-Carboniferous distribution. Southern Africa, in particular, was repeatedly glaciated during Pre-Cambrian times. While so many contradictory voices confuse judgment, one cannot do better than commend Dunbar's wise dictum that "it is unsafe to reject, a priori, either continental drift or foundering of broad land bridges". ARTHUR HOLMES

¹ Simpson, G. G., Amer. J. Sci., 241, 1 (1943).

⁵ Simpson, G. G., Amer. J. Sci., 241, 1 (1943).
² Macgregor, A. M., Trans. Geol. Soc. S. Africa, 43, 9 (1941). Ahrens, L. H., Trans. Amer. Geophys. Union, 33, 193 (1952). Ahrens, L. H., and Macgregor, A. M., Science, 114, 64 (1951).
³ Holmes, A., Amer. J. Sci., 248, 90 (1956); Rep. 18th Int. Geol. Cong., London, Pt. 14, 258 (1951).
⁴ Holmes, A., Leland, W. T., and Nier, A. O., Amer. Min., 35, 20 (1950).

⁵ Marble, J. P., Rep. (1948-49) Com. Geol. Time, 72 (1949).

THE FARADAY SOCIETY, 1903-53 By DR. F. C. TOMPKINS

ON February 14, 1903, a small meeting took place in the rooms of the now defunct Faraday Club at St. Ermin's Hotel, Westminster, and a new scientific society was inaugurated. A little later "The Faraday Society" was formally founded, having as its objects the promotion of "Electrochemistry, Electrometallurgy, Chemical Physics, and Kindred Subjects". The Society celebrated its fiftieth anniversary this year on April 16 at the Royal Institution, which houses the famous laboratories where Faraday's researches were carried out.

In 1903, as to-day, there were dangers, both financial and otherwise, in creating yet one more society and one more journal. Indeed, Sir William Ramsay viewed the prospects with marked disfavour, for he could not find sufficient time to read the stream of scientific literature already appearing. Despite this, the project made a good start under the presidency of Sir Joseph Swan, ably supported by such eminent men as Prof. Crum Brown, Prof. F. G. Donnan, Lord Kelvin, Sir Oliver Lodge, Dr. Ludwig Mond, Lord Rayleigh, Sir James Swinburne and others. Within a year there were 254 members, with