SOILS

A new global view

T. R. Paton
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Frontispiece A texture-contrast soil, near crest of long gentle hillslope on Mesozoic sandstone, Sydney region.

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CRC Press
Taylor & Francis Group
Boca Raton London New York

CRC Press is an imprint of the
Taylor & Francis Group, an informa business

CRC Press
Taylor & Francis Group
6000 Broken Sound Parkway NW, Suite 300
Boca Raton, FL 33487-2742

ISBN-13: 978-1-85728-465-2 (pbk)
ISBN-13: 978-1-138-46870-2 (hbk)

Front cover: A Texture-contrast soil, near crest of long gentle hillslope on Mesozoic sandstone, Sydney Region (T.R. Paton).

Library of Congress Cataloging-in-Publication Data

Catalog record is available from the Library of Congress

T&F informa

Taylor & Francis Group
is the Academic Division of T&F Informa plc.

Visit the Taylor & Francis Web site at
http://www.taylorandfrancis.com

and the Routledge Web site at
http://www.routledge-ny.com

DOI: 10.1201/9781003420361

Contents

Acknowledgements

We are grateful to the following individuals and organisations for granting permission to reproduce copyright materials:

Figure 1.1, CSIRO; Table 1.1, *Principles of geochemistry,* John Wiley; Figure 1.4, *Quarterly Review of Biology,* The University of Chicago Press; Figure 2.1, *Clays and Clay Minerals,* Clay and Clay Mineral Society; Figures 3.2, 5.2, 5.3, 8.1, *Search,* Control Publications; Figures 3.3, 8.3, 8.4, 8.5, *Australian Journal of Soil Research,* CSIRO; Figures 4.1 and 9.3, *Journal of Ecology,* Blackwell Scientific; Figures 4.2, 4.3, 4.5, 4.8, 4.9, 4.10, 6.5, 6.6, 6.7, 6.8 and Table 4.2, *Geoderma,* Elsevier; Figures 4.7, 4.13, A1.1, A1.2, A1.3, A1.4, A1.5, A1.6, A1.7, A1.8, A1.9, T. R. Paton, *The formation of soil material,* Chapman & Hall; Figure 4.12, *Catena,* Catena Verlag; Figure 6.1, *Landslides and related phenomena,* Columbia University Press; Figure 6.3, *Overseas geology and minerals resources,* HMSO; Figure 6.4, Louisiana State University Press; Figure 7.1, *Geographical Magazine,* Savedish Enterprises; Figure 8.2, *Australian Journal of Experimental Agriculture and Animal Husbandry,* CSIRO; Figure 8.6, Linnanean Society of New South Wales; Figure 9.1 and 9.2, Table 9.1 and 9.2, *Geological Magazine,* Cambridge University Press; Figures 9.4, 9.5, 9.6, 9.7, 9.9, *Journal of Soil Science,* Blackwell Scientific; Figures 9.8 and 9.10, The Biscuit, Cake, Chocolate and Confectionery Alliance; Figure 9.11, INEAC, SERDAT, Brussels; Figures 10.1, *Soil Science Society of America Journal,* SSA Association; Figures 10.2 and 10.3, *The Geographical Journal,* Royal Geographical Society; Figure 10.4, T. L. Péwé; Figure 10.5, *Glacial and fluvioglacial landforms,* Longman; Figure 11.5, *Mountain Research and Development;* Figure 11.6, The estate of H. S. Gibbs, *New Zealand soils,* Oxford University Press.

Colour plates: P. B. Mitchell (Plates 1–2) and T. R. Paton (Plates 3–8).

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Introduction

. . . every discipline, as long as it used the Aristotelian method of definition has remained arrested in a state of empty verbiage and barren scholasticism, and that the degree to which the various sciences have been able to make any progress depended on the degree to which they have been able to get rid of the essentialist method. *Karl Popper*

Critique of soil zonalism

Over the past 40 years there has been an ever-increasing concentration by pedologists on soil taxonomy, with the main aim being to classify soils, in the form of profiles, for a range of pragmatic purposes. Meanwhile, the understanding of soils in the sense of how they have been formed – i.e. **pedogenesis** – has become increasingly neglected and submerged in an ever more precise morphological system and its associated sea of neologisms. As Peters (1991: 40) has so aptly stated:

> Sciences generate jargon. This terminology is necessary to express complex and novel ideas clearly and succinctly, but terminology often becomes a linguistic barrier around the science, protecting its practitioners from too close a scrutiny by the public or other scientists and providing the camaraderie offered by a common but private language. In weak sciences, jargon substitutes for ideas and theories so that even a hopelessly uninformative field can share some of the trappings of success.

The roots of this pedogenic atrophy can be traced back to the climatic determinism and the associated soil zonalism of the early years of the twentieth century, which will now be discussed together with some of the major problems that arose from zonalism and how, in attempting to solve these problems, the present situation arose.

The concept of soil zonalism has dominated pedology throughout the twentieth century and, although its origin can be traced back to Russian

DOI: 10.1201/9781003420361-1

scientists of the late nineteenth century, it has been through the United States Department of Agriculture (USDA), especially C. F. Marbut and his successors, that the greatest impact has been made. One of the earliest and most complete statements about the nexus of ideas that go to make up soil zonalism is contained in the USDA *Yearbook of agriculture* for 1938, *Soils and men*, (USDA 1938: 948–92). According to this, soil is produced over **time** by the action of **climate** and **living organisms** upon **parent material**, as conditioned by local **relief**, which results in the formation of **zonal**, **intrazonal** or **azonal** soils. The most important characteristics of zonal soils result from the effects of the active factors of climate and living organisms, on well drained parent materials of mixed mineral composition, over a long period of time, whereas in the case of intrazonal soils the dominant factors are passive, either parent material or relief. Azonal soils are closely related to their parent material, with few if any characteristics ascribable to pedogenesis.

The main emphasis in this zonalistic scheme is the operation of the active factors downwards from the surface, which leads to the development of two genetically related **horizons**: the surface, or A horizon, from which fine-grain materials are removed in the process of **eluviation**; and the zone immediately below this, where the eluviated material is deposited to form the **illuvial** or B horizon. Together these two horizons form the **solum**. In suitable conditions, continuation of this eluvial–illuvial process leads to an increasing contrast between the two horizons, which ultimately results in the formation of a coarse-textured residual A horizon over a heavier-textured B horizon: the aptly termed texture-contrast soil.

Despite the fact that texture-contrast soils were generally accepted as forming by illuviation (USDA 1938, Thorp & Smith 1949, McCaleb 1959, Stephens 1962), there was little hard evidence to support this contention, and since 1938 a major task of pedology has been to acquire such evidence. Thus, Brewer (1955), in trying to assess the degree to which clay illuviation was involved in the development of a yellow podzolic soil in southern New South Wales, used zircon as a stable mineral to measure the degree of bedrock alteration and so determined the amount of clay that could have been produced by this process. The results showed that less clay was present in all horizons than could have been formed by alteration. Hence there was no need to invoke clay illuviation to explain the clay maximum at the top of the B horizon. Similar conclusions were reached by Green (1966),using the same techniques in the study of a red-brown earth on similar granodiorite bedrock in northern Queensland. Oertel (1961, 1968, 1974) established that gallium proxies for aluminium in clay minerals, and hence could be used as a marker of clay movement in profiles, as could the concentration

ratios of alumina, ferric oxide and titania (Oertel & Giles 1967). In both cases no evidence was found of any significant clay translocation contributing to the clay maximum at the top of the B horizon.

These were indirect investigations of clay movement. A more direct method was provided by the recognition of clay skins, or **cutans**, as resulting from the deposition of illuvial clay (Frei & Cline 1949, Brewer 1964, Cady 1965). Brewer (1968) used this approach to investigate the amount of illuviated clay in ten texture-contrast soils (five red-brown earths and five red podzolics). Total clay was determined by particle-size analysis, and illuviated clay estimated by point-counting on random strips across thin sections. In three of the red-brown earths there was virtually no illuviated clay, whereas in the other two no more than 5–6% of the clay supposedly lost from the A horizon could be accounted for as illuviated clay in the B horizon. In the red podzolics the amount of illuviated clay was greater, but it was distributed down through the subsoil and even into the bedrock, rather than being concentrated in a clay maximum at the top of the B horizon.

A much more fundamental investigation on the use of cutans as a means of quantifying illuviation was made by McKeague et al. (1978, 1980, 1981) and McKeague (1983), who tested whether or not a Bt horizon (a subsoil layer containing ≥1% illuvial features) could be consistently identified, either in the field or in the laboratory. It can be concluded from their investigations that there was so much variation between individual investigators, in deciding what was and what was not an illuvial cutan, that the method did not provide an effective way of quantifying the amount of illuvial clay.

It was obvious from these studies that clay illuviation from A to B horizons was an inadequate general explanation for the formation of texture-contrast soils and that the method for quantifying the most direct evidence for illuviation (cutans) was also not without its problems. Despite this, however, the solum persisted as a central pedological concept, so that whatever other explanations were suggested they needed to operate within an eluvial–illuvial framework, of defined A and B horizons. As long ago as 1949, Simonson (1949) realized that in the case of the red/yellow podzolics of the southeastern USA the A horizon was too thin to have provided the amount of clay that occurred in the B horizon. It was, therefore, proposed that clays were formed from primary minerals in the B and C horizons and were destroyed in the A horizon, so that a clay maximum resulted at the top of the B horizon. This idea was widely accepted and applied as a genetic explanation in many parts of the world even though no evidence had been produced bearing on the different conditions that must prevail in the B horizon and A horizon, if clay formation was to occur in one place and clay destruction in the other. A variant of this model was proposed by Brewer (1955)

and Green (1966), who suggested that the products of clay breakdown in the A horizon were removed in solution by drainage along the A/B horizon boundary; once again, however, no direct evidence was offered in support of this contention. Another possibility advanced was that the evidence of clay illuviation, that is the cutans, had been destroyed in those situations where B horizon clays were capable of sufficient seasonal expansion and contraction (Nettleton et al. 1969, Chittleborough & Oades 1979, Chittleborough et al. 1984a,b). However, the evidence used (that of the degree of expansion of the B horizon clays) was indirect and circumstantial and no more convincing than any of the other evidence.

In brief, it can be concluded that none of these ideas concerned with the redistribution or differential destruction of fine-grain materials within an originally homogenous mass offered a reasonable solution to the problem of the formation of texture-contrast soils. Nor did the complementary idea of inheritance from bedded material prior to differential alteration as advanced by Oertel & Giles (1967) and Oertel (1974), for whereas there was some possibility that it might apply in very special circumstances in areas of recent deposition, there was no chance of it being a general solution to the problem, especially away from sites subject to riverine or aeolian activity. This conclusion was strengthened when Chittleborough & Oades (1979), in reinterpreting Oertel's (1974) data, maintained that the topsoil and subsoil of the texture-contrast soil involved had developed from what was originally homogenous material.

Chittleborough & Oades (1980a,b) suggested a new variant of clay illuviation, based on the observation that the clay fraction was apparently bimodal – a coarse fraction of lower mobility and a fine fraction of higher mobility, which were distinguishable both chemically and mineralogically – to demonstrate a relative enrichment of fine clay coinciding with the B horizon maximum. This was applied to river valley terrace sequences in southeastern Australia (Chittleborough et al. 1984a,b,c) on the supposition that the age of the soil increased with terrace height, and that all the soils were created from similar sedimentary materials, so that the soils of the highest terrace had been subject to illuviation for the longest period of time and hence had the greatest texture-contrast development. However, the process of differential illuviation of fine- and coarse-grain clay was questioned even within the papers where it was originally proposed when it was conceded that the profile differences could equally be explained by alteration throughout the profile with a loss of fine clay from the A horizon. Furthermore, Walker & Hutka (1979) in studying soil profiles and sedimentary sequences in southeastern Australia had already established that there was a poor correlation between the distribution of fine (particle-size) and illuviated (point-

counted) clays. Later still (Walker & Chittleborough 1986) the importance of clay bimodality was called into doubt when no evidence was found of it in the B horizon clay fraction of 20 texture-contrast soils. Indeed, a better interpretation would seem to be that there had been an enrichment of fine clay towards the top of the B horizon as a result of increased alteration, rather than by illuviation from the A horizon.

A second problem that generated much debate concerned the role of surface erosion and deposition in relation to soil genesis, for even though such processes were excluded from pedological consideration in the zonal model of soil formation, the fact that they operated was undeniable. It was, therefore, necessary from a zonalist point of view to demonstrate that such surface erosion and deposition did not interfere with the processes of pedogenesis. This was attempted in two quite distinct ways; first by Nikiforoff (1949) and then by Butler (1959). Nikiforoff postulated that on gently rolling hillslopes the rate of surface erosion and deposition was in balance with the processes of soil formation, so that, on the upper part of such slopes, the surface of the A horizon was removed by erosion at the same rate as the top of the B horizon changed into A horizon material, for according to Nikiforoff, the A and B horizons had to be maintained at a standard thickness. In other words, in the case of such non-cumulative soils the profile sank into the landscape at the same rate as the surface was eroded. On the lower hillslopes deposition took place at an equally slow rate, such that it equalled the rate at which these sediments became A horizon material. At the same time, to maintain an A horizon of standard thickness an equivalent thickness of basal A horizon was turned into B horizon, so that the profiles of these cumulative soils rose in the landscape in time with the deposition. Over the slope as a whole, there was an equilibrium between erosion/deposition and pedogenesis and a mature or zonal soil developed, which persisted independently of time; it ceased to have a history and from a pedological point of view surface erosion and deposition were finessed, so as not to have any significance. This proposition is still generally accepted as it is an implicit component of soil zonalism, despite the fact that the whole thing depends on a very subtle balance between what are in effect independent variables. In addition, it has never been clearly stated whether it is the non-cumulative or cumulative soil that is zonal. Yet, to consider them both as zonal, given their very different origins, is a paradox that has never been considered.

Butler (1959) also argued for the non-pedological status of surface erosion/deposition, but in a rather different way. From the common occurrence of buried soils he concluded that Nikiforoff's ideas of a balance between continuous erosion and soil formation were inapplicable and that

rather than being in a state of equilibrium land surfaces were subject to periodic change, such that at one extreme they were very stable when soil formation could occur and at the other were unstable when erosion and deposition was dominant. As with Nikiforoff, the process of surface erosion and deposition was separated from pedological processes, but in this case it was done by using the time factor, which caused Butler's work to have strong historical implications, deftly avoided by Nikiforoff. Butler's postulated periodicity depended upon the recognition of a recurrent sequence of events, referred to as K cycles and defined as being the time interval covering the formation, by erosion and deposition, of a landscape surface and the development of soils on that surface. K cycles were distinguished from one another by numerical subscripts, so that K_1 was the first cycle back from the present, K_2 the second and so on. Figure I.1 shows how two K cycles would

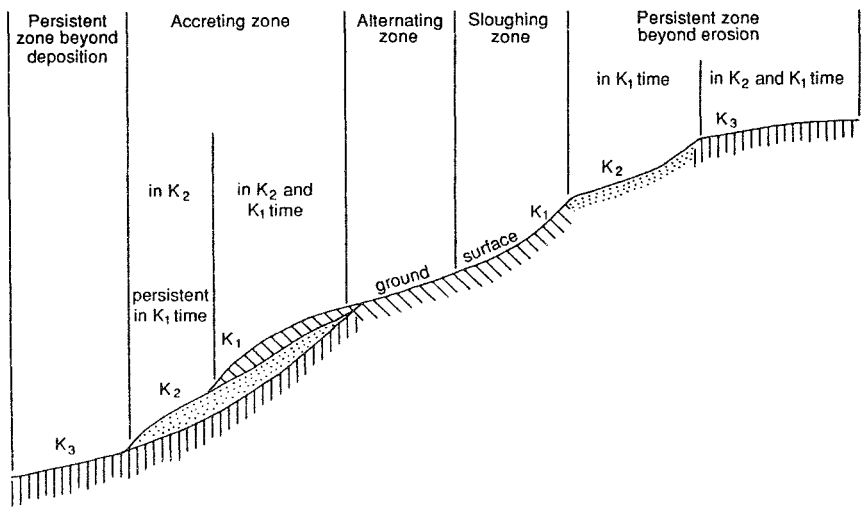

Figure I.1 Periodicity in soil development (after Butler 1959).

have affected a particular hillslope, starting with K_3 as an established ground surface. The K_2 instability brought in erosion on the steeper section of the hillslope and this advanced up slope eroding the K_3 ground surface, while farther down slope the K_3 surface was buried by the debris from this erosion. The K_2 stable phase began when erosion and deposition stopped and soil development began on the K_2 ground surface and advanced farther on the persistent K_3 surface. Later the K_1 instability led to renewed upslope erosion and downslope deposition that was comparable to, but less extensive than, that which occurred in the K_2 cycle. Stabilization of the surface accompanied by renewed soil formation completed the K_1 cycle. It was

obvious in this approach that both relic and buried soils were characteristic of these periodic cycles of change, which enabled a history of development to be worked out. The soils developed on the K_1, K_2 and K_3 surfaces were taken to represent a sequence of relic soils of increasing age, such that K_3 soils in the Australian situation were frequently texture-contrast profiles and were viewed as mature or zonal soils, which had developed on a long stable surface.

Despite the fact that erosion and deposition were treated very differently in the Nikiforoff and Butler schemes, pedogenesis was treated in exactly the same way, in that it was restricted to vertically operating processes in both cases. This led to unsustainable, but rather different, endpoints. In Nikiforoff's case the resulting soil was a mature or zonal profile, which once developed was independent of the passage of time. Much the same kind of soil developed on older (K_3) Butlerian surfaces, but instead of being independent of time it was assigned an age of something like 30 000 years, because comparable soil profiles on higher stream terraces had been so-dated. In both cases the resulting soils, which in many cases were texture-contrast, represented a rather sterile endpoint. In Nikiforoff's case the demand was for a balance between two independent variables, erosion/deposition and pedogenesis, over a period of time sufficient for a zonal soil to develop, whereas Butler needed, in the case of K_3 soils, a complete cessation of surface erosion/deposition on a particular landscape segment over a similarly long time period. In view of the efficacy of near-surface processes (see Chs 3–6), neither of these suggestions is really credible.

Other problems also occurred. Thus, the zonalistic introduction of parent material as a factor of soil formation instead of bedrock was also responsible for several difficulties. In *Soils and men* (USDA 1938: 949) it was stated:

> The first step in the development of soil is the formation of parent material, accumulated largely through rock weathering. The parent rock is a relatively inert storehouse of future soil material rather than an active factor in soil formation.

The implementation of such a concept depended on an ability to discriminate consistently between parent material formation and soil formation, and this represented a major difficulty particularly where, as was often the case, both processes were taking place together. It is apparent from the *Soil survey manual* (USDA 1951: 147) that these difficulties were recognized, for it was stated:

> We may conceive of weathering and soil formation as different sets of

> processes even though the sets have many individual processes in common and more often than not go on together. Yet a nearly convincing case may be made for considering both together as soil formation, beginning with parent rock as the independent variable in the set of five genetic factors instead of parent material as here defined.

However, having seen all the difficulties and a way of rationalizing them, there was a retreat back to the 1938 concept of parent material.

From this discussion it can be seen that no satisfactory answer has been found to a range of pedological problems. The most important of these concerns the formation of texture-contrast soils. Closely associated is the problem of surface erosion and deposition, where two models have been developed to demonstrate that these processes operate independently of pedogenesis and neither of them have credibility. Another major obstacle was caused by the introduction of the concept of parent material, which led to the exclusion of bedrock alteration from pedogenesis and introduced a large degree of equivocation into pedology. Most particularly it caused increased emphasis to be given to the influence of climate and living organisms (the active factors) at the expense of bedrock and topography, which has had profound consequences on pedogenic understanding.

These problems are all associated with zonalism and its profile-restricted view of pedogenesis, and yet at the present time the zonal system is hardly mentioned except in some introductory texts (Bridges 1978, Gibbs 1980). Indeed, many pedologists would argue that the zonal scheme has been largely superseded by a new morphological system, in which the problems discussed previously are no longer significant. This "new" scheme (*Soil taxonomy*, USDA 1975) maintains that eventually by concentrating on soil morphology and the more refined definition of classificatory units a better pedogenic model will emerge. The newness of approach is most readily seen in the general classification where the three orders of *Soils and men* (USDA 1938), zonal, intrazonal and azonal, are replaced by ten very different orders. However, this change is more apparent than real, for despite the new names all of the orders, except for the inceptisols, can be recognized as having been derived from classificatory units at the suborder and great soil group level in *Soils and men*. Even more significantly differentiation between the orders is still based on maturity as reflected in the degree of horizon contrast and assumed age, and no acknowledgment is made of the great amount of work critical of the concept, which was discussed earlier. In effect the soil orders of *Soil taxonomy* were arrived at by a slight shuffling of the higher classificatory units defined in *Soils and men*, but even more fundamentally

Soil taxonomy remains based on zonal pedogenesis to the same extent as the classic zonal statement given in *Soils and men*. There is, however, a major difference, for whereas in 1938 the emphasis was explicitly on zonal pedogenesis, with classification being derived from it, by 1975 the emphasis was almost entirely classificatory, with zonal pedogenesis being implicit at best. There was a move away from a genetic understanding of soil towards an ability to classify it, a move that had been signalled in the 7th approximation (USDA 1960: 4) where it was stated:

> Since the genesis of any soil is often not understood, or is disputed, it can be used only as a general guide to our thinking in the selection of criteria and forming of concepts. Generally a more or less arbitrary definition of a pedon serves the purpose of classification better at this time than a genetic one.

The continuing belief by a considerable number of pedologists that, by using this approach of more precise definition in soil classification, a better pedogenic model would eventually emerge reflects a belief in **induction**. This means that objective and unbiased conclusions can only be reached by measuring and describing what is encountered without having any prior hypotheses, or preconceived expectations. This viewpoint had been decisively repudiated as long ago as the middle of the nineteenth century, when it was shown to lead to an intellectual impasse and one moreover that no scientist had followed, or ever could follow (Mayr 1982). The most that can be achieved by its application through *Soil taxonomy* (USDA 1975) is an ever-finer splitting of the classificatory units with no possibility of anything novel being generated. *Soil taxonomy* is only the leading example of what has become a general morphological bias. Thus, at a global level there is the FAO–UNESCO (1970–80) *Soil map of the world*, and many countries have developed their own systems. All of these contribute to a general pedogenic atrophy, for all the data involved are constrained by the same zonal criteria and hence have the same limitations. It is as a result of this situation that the authors felt the need for a re-evaluation of pedology, with a return to fundamentals, which means pedogenesis, for without having a firmly based model of soil formation there can be no meaningful classification. Such a reappraisal obviously required a new viewpoint and this came from two sources. First of all there were detailed soil investigations made on a landscape rather than a profile basis, in areas not directly affected by Pleistocene glaciations. Initially this was in Africa on the foundation created by Geoffrey Milne and subsequently in Australia. Supplementing this were a series of process studies by geomorphologists on surface erosion and deposition.

This has enabled a new approach to be made regarding the processes of soil formation and the resulting soil materials, which is dealt with in Part I of this book (Chs 1–6).

The second source is plate tectonics, which has so revivified the whole of Earth sciences over the past 30 years, and yet it has been ignored by pedologists, despite the fact that it has completely reformulated our concept of the Earth's surface and in particular how continents are viewed, the very foundation on which pedology rests. In Part II (Chs 7–11) this omission will be rectified and the process/material model of Part I will be expanded on a plate-tectonic base to account for global soil distribution.

PART I

THE PROCESSES OF SOIL FORMATION AND THE RESULTING SOIL MATERIALS

Broadly, the processes involved in soil formation belong to one or other of two major groups. One of these deals with the way in which minerals, formed deep within the Earth, react and adjust to near-surface conditions, which can be referred to as **epimorphism**. An understanding of epimorphism can most conveniently start from a consideration of the chemical composition of the Earth's crust, where only eight elements (oxygen, silicon, aluminium, iron, calcium, magnesium, sodium and potassium) account for more than 98% of its weight (Table I.1). Despite being markedly less

Table I.1 The commoner chemical elements of the Earth's crust (after Mason 1966).

	Weight (%)	Atoms (%)	Volume (%)
O	46.60	62.55	93.77
Si	27.72	21.22	0.86
Al	8.13	6.47	0.47
Fe	5.00	1.92	0.43
Mg	2.09	1.84	0.29
Ca	3.63	1.94	1.03
Na	2.83	2.64	1.32
K	2.59	1.42	1.83
Total	98.59	100	100

dense than the other elements, oxygen is by far the dominant constituent and this becomes even more marked when density differences are eliminated, by dividing the weight percentage figures by the element's atomic weight, which shows that out of any 100 crustal atoms nearly 63 are oxygen. If account is taken of the relatively large size of the oxygen atoms (see Fig. A1.5) they will be found to make up nearly 94% by volume of the Earth's crust, which means that the crust can be regarded simply as a packing together of oxygen atoms, with all the other elements accommodated within the packing voids. However, such an accumulation is only made possible by the

DOI: 10.1201/9781003420361-2