©1984 Geologie en Mijnbouw 0016-7746/84/6301-0113 $ 2.50/0

COMMENT AND REPLY ON

STRUCTURE AND GENERAL CHEMISTRY OF TABLE MOUNTAIN SANTA BARBARA PHOSPHATES, CURAÇAO, NETHERLANDS ANTILLES

COMMENT

WIM HEIJNEN[1] & TON TEN HAVE[2]

INTRODUCTION

The purpose of STIENSTRA's (1983) study was to provide new and detailed information on the distribution of mineralization within the phosphatized body of Table Mountain Santa Barbara, Curaçao. Instead, STIENSTRA has blended a confusing mixture of previously published and unpublished data (TEN HAVE & HEIJNEN, 1978; TEN HAVE ET AL., 1980, 1982), which he has poorly understood and to which he has only incompletely referred. He has furthermore made some unjustifiable correlations resulting from rash processing of drilling data and dubious observations based on measurements of indistinguishable strata. In addition, STIENSTRA's assertion that his paper is the first result of a research project on the phosphates of the Leeward Netherlands Antilles is historically inaccurate. His study is the sequel to a project initiated some years ago by DR. C.G. VAN DER MEER MOHR, which had already resulted in two earlier publications (TEN HAVE ET AL., 1980, 1982) and in an unpublished internal report (TEN HAVE & HEIJNEN, 1978). Thus STIENSTRA's (1983) paper is the third one in this project and only the first one since it has been financially supported by WOTRO. Following are detailed comments on the scientific content of STIENSTRA's paper.

DISTRIBUTION OF PHOSPHATE

Mathematical processing of drilling data enabled STIENSTRA to define 15 phosphate units, some of which can be traced over 200 metres or more. This clashes with field observations from TEN HAVE ET AL. (1982), who reported the occurrence of isolated phosphate pockets, whose longest diameters range only from 0.5 to 20 m. For instance, examination of STIENSTRA's figure 5 (from which a horizontal scale is missing) in combination with his figure 3A, yields an estimated length for Section II, from ND 147 to ND 270, of about 500 m. Since the average distance between two successive drill holes along this line is roughly 70 m, it is evident that STIENSTRA, when drawing correlation lines, has ignored the generally accepted rule in geology that it is unacceptable to correlate local, small-scale phenomena over far larger distances than the data can justify. Subdivision of the phosphate body into 15 distinct units (STIENSTRA's figures 5 and 6) is highly questionable. It appears that STIENSTRA has injudiciously drawn correlation lines to add credence to a hypothetical model for stratification control. This claim is discussed further in the following section.

In fact, two main conclusions can be drawn from the existing drilling data. Firstly, the main phosphate body occurs between 98 and 128 m above sea level, with minor phosphate occurrences mapped as low as 77 m and as high as 176 m above sea level. Secondly, a large part of the Seroe Domi Limestone–Curaçao Lava Formation contact plane can be traced as illustrated in STIENSTRA's figure 3B. Incidentally, that figure is remarkably similar to our figure in Appendix II (TEN HAVE & HEIJNEN, 1978), to which STIENSTRA does not refer.

STRATIFICATION

One of the 'new' ideas proposed by STIENSTRA is based on DE WIJS's (1962) conclusion that phosphatization was mainly governed by primary stratification (Abstract p. 275). We consider this suggestion insupportable. STIENSTRA himself writes at the end of his paper (p. 282) that stratification seems in part to define phosphatization but that zones of maximum mineralization appear to transsect stratification.

[1] Instituut voor Aardwetenschappen, Rijksuniversiteit Utrecht, Postbus 80021, 3508 TA Utrecht, The Netherlands.
[2] Koninklijke Shell Exploratie en Produktie Laboratorium, Volmerlaan 6, 2288 GD Rijswijk, The Netherlands.

STIENSTRA's claim (p. 282) that the mineralized zone is controlled by the orientation of both the limestone-basalt contact, which dips between E and SW (STIENSTRA, figure 3B), and the pattern set by bedding and colour-stratification, which dips roughly N (STIENSTRA, p. 279), appears contradictory. It is a leap of fantasy to proceed to the further claim that this agrees with a strong correlation between depositional carbonate facies and phosphatization, as determined by TEN HAVE ET AL. (1982). These authors, in fact described erratically distributed coralliferous limestone lenses within a micritic matrix with no stratification at all.

STIENSTRA states correctly (p. 278) that a primary stratification can hardly be distinguished. Nevertheless he presents a set of limestone bedding plane measurements. Critical review of his 49 data points has shown that 30 of these are colour beds and lineations, 7 are indistinct planes and only 12 are bedding limestones. Furthermore, STIENSTRA has failed to demonstrate convincingly that the colour bands indicate stratification rather that diagenetic overprinting. DE WIJS's (1962) suggestion that organic matter may have been a partial cause of the colour banding has already been questioned by TEN HAVE & HEIJNEN (1978). In that paper we furthermore described a south-dipping contact between massive limestone and a more porous unit, which is clearly transsected by a red-pink colour band. This and other similar examples observed in the field, suggest a diagenetic origin for the colour variations.

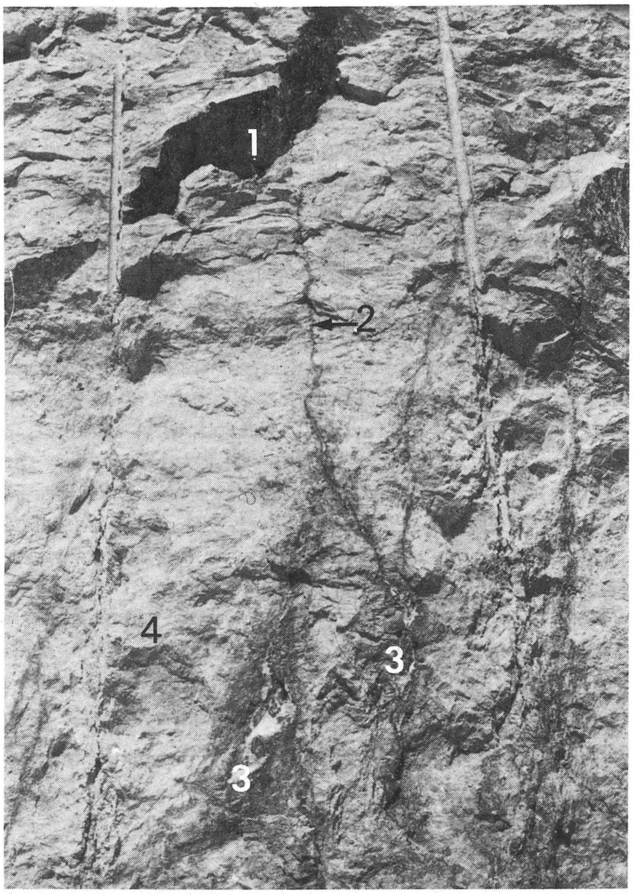

Fig. 1
Relationship between fractures and phosphatization. Permeable fractures acted as fluid paths for phosphate-rich solutions from the top of the Table Mountain downwards into the formation. The area shown in the photograph is about 15 m^2. 1. Fracture coated with phosphatic material. 2. Solution channel filled with phosphatic material. 3. Phosphatized unit. 4. Non-phosphatized (or slightly phosphatized) Seroe Domi Limestone.

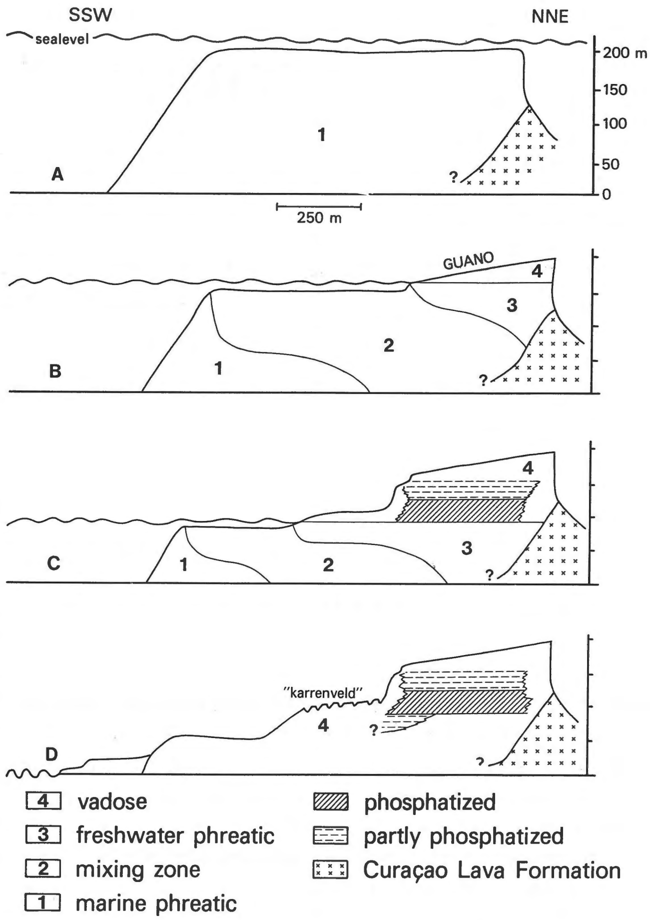

Fig. 2
Generalized model for phosphatization controlled by sea level fluctuations.
A. Hypothetical starting situation.
B. First guano cover and subsequent phosphatization of Seroe Domi limestones about 150 m above present day sea level. Note the association with the terrace-like escarpment at 158 m.
C. Situation after most prolific phase of phosphatization. Note the correlation between phosphatized zone and presence of the terrace around 100 m above present day sea level.
D. Present day situation of Table Mountain.
(after Ten Have et al., 1980, p. 246)

FISSURES AND FRACTURES

STIENSTRA's statement (p. 282) that fractures and fissures are not genetically related to phosphatization is incorrect. Indeed, later fractures are found to intersect phosphatized zones. Such fractures are often filled with calcite cement and represent a post-phosphatization diagenetic feature (TEN HAVE & HEIJNEN, 1978; TEN HAVE ET AL., 1982). However, numerous karst pipes, fractures, fissures and solution channels, filled or coated with phosphate, also occur. A direct relationship between a large phosphate-coated fracture, small phosphate-filled solution channels and phosphatized units has already been observed by TEN HAVE & HEIJNEN (1978, photo 14). The same photograph is printed in the present paper (Fig. 1). In our view such fractures and channels have played an important role in transporting the phosphate-rich solutions from the top of the Table Mountain, where guano was deposited, downwards into the formation. They formed ideal conduits for fluid flow through the relatively impermeable carbonate rock matrix.

GENETIC PROCESSES

The idea of sea level control on the distribution of phosphate is not new. A correlation between the presence of terraces and the overall distribution of phosphate rock in the Table Mountain had already been reported by GRUTTERINK (in: HUTCHINSON, 1950) as early as 1928. Fifty years later, aided by additional data, the same observation was expressed in more detail and related to sea level (and associated mixing zone) by TEN HAVE & HEIJNEN (1978), TEN HAVE ET AL. (1980); and summarized in figure 1 of TEN HAVE et al. (1980), which is redrawn here as Fig. 2; the second paper is not referred to by STIENSTRA. STIENSTRA's figure 7 differs from the present Fig. 2 in the addition of stratification. STIENSTRA claims in his text that mineralization transsects stratification but does not support such a claim in his figure 7.

The geometry of a mixed water salinity lens is subject to large variations, depending on climate, amount of rainfall, tides, topography, etc. (STEINEN ET AL., 1978). In any case, the geometry of a mixing zone is completely independent of stratification. Mineralization associated with a mixing zone will therefore cross-cut stratification (an analogous situation occurs with dolomitization in the mixing zone). By suggesting the opposite, STIENSTRA's figure 7 is both confusing and contradictory to his own observations.

CONCLUSIONS

(a) Many of the conclusions reached by STIENSTRA (1983) are debatable. (b) Some of the data and concepts, upon which many of his claims are based, have been misinterpreted. (c) Much previously published and unpublished material has been incorporated into STIENSTRA's model without reference. (d) The model itself is considered to be highly questionable and, at least in part, contradictory.

ACKNOWLEDGEMENTS

We are grateful to Bret Mattes for correcting the English manuscript and to Mr. J.R. Meye for making the drawings.

REPLY

PIETER STIENSTRA[1]

INTRODUCTION

'C'est le ton qui fait la musique…'

HEIJNEN & TEN HAVE have correctly assessed from apparently close reading of the text that the aim of the investigation published by STIENSTRA (1983) was to present new and detailed information on the distribution of mineralization within the phosphate body of Table Mountain Santa Barbara, Curaçao. Their subsequent arguments indicate, however, that they have completely failed to appreciate both the scale of detail and the context of this study.

HEIJNEN & TEN HAVE take exception to my statement in the acknowledgements that the article (STIENSTRA, 1983) is 'the first result of a research project on the phosphates of the Leeward Netherlands Antilles'. With that statement I intended merely to indicate that the paper represented preliminary conclusions of a larger research project. HEIJNEN & TEN HAVE's interpretation of that sentence as my claim to priority can only be attributed to misreading. It has never been in question that numerous other workers have contributed to an understanding of the Table Mountain rock phosphates. Their published and unpublished articles are fully detailed in the section 'Earlier investigations' in the Introduction of STIENSTRA (1983), whereas they have also been mentioned many times in other parts of the text. In the paragraph on genetic processes HEIJNEN & TEN HAVE specifically mention the omission in my article of one of their papers

[1] Comparative Sedimentology Division, Institute of Earth Sciences, State University of Utrecht, Budapestlaan 4, 3508 TA Utrecht, The Netherlands.

(TEN HAVE ET AL., 1980). My publication, however, was not intended to present a complete bibliography on the published and unpublished works of HEIJNEN & TEN HAVE. The summarized repetition of their 1978 ideas in a (1980) congress abstract was therefore not mentioned.

It must be pointed out that the WOTRO-project is of much wider scope than any research carried out before, since it aims at the description of most of the phosphate rock types on all of the Leeward Netherlands Antilles.

In this context my research on Table Mountain Santa Barbara has followed a 'large scale' geochemical approach. HEIJNEN & TEN HAVE faile to understand this point and that underlies many of their arguments. To the careful reader it will become clear that the character of the features described as well as the genetic model presented in my paper differ both in quality of detail and reliability from those of earlier authors. This is in part due to the much greater number of data used.

DISTRIBUTION OF PHOSPHATE

HEIJNEN & TEN HAVE do not appear to appreciate the differences in scale and character that exist between the petrologically defined phosphate distribution in pockets, as they described on earlier occasions, and the geochemically defined macro-zonation within the whole phosphate body, presented by STIENSTRA (1983). Part of this misunderstanding could probably have been avoided by a somewhat fuller definition of the units described as 'zones of maximum mineralization'. These zones have been defined by their contents in P_2O_5 ($> 4.59\%$), and concentration of fluorine. I used that data from 501 drilling logs, available from the Curaçao Mining Company, to correlate trends in phosphate content, fluorine concentration, and, occasionally, porosity between successive drill holes.

It is obvious that these zones of maximum mineralization are no formal stratigraphic units. They belong to the so-called attribute-defined units, which are based upon special attributes of the component strata (KRUMBEIN & SLOSS, 1963, p. 333). The definition of large-scale geochemical zones of maximum phosphatization does not, however, give information on the way the phosphate is distributed within these zones on a smaller, petrographic scale. It does not, therefore, necessarily exclude the presence of isolated phosphate pockets, a petrographic feature described on Table Mountain by TEN HAVE ET AL (1982). The two phenomena are of a different character and scale.

HEIJNEN & TEN HAVE consider that my results are based on insufficient data. This claim apparently originates in the erroneous assumption that I used the same 200 drilling logs (a part of the so-called ND-series) available to TEN HAVE & HEIJNEN (1978) to reconstruct the phosphate zone. However, I had all of the ND-logs (300) and about 1500 more (the so-called C-, X-, Y- and Z-series) at my disposal. From this enormous amount of drilling data I carefully selected 501 drilling sites, which compose a close network, and investigated their logs. From these a selection of 203 drilling logs was studied in great detail. The selection of these logs was based on the presence of as complete a record as possible with regard to the distribution of phosphate types, phosphate grade, fluorine content, and recovery. Correlation, and reconstructions were primarily made on the basis of these logs, which were arranged along many sections. When necessary, the other drilling logs, with less complete records, were consulted. 'Less complete' in most cases means shallow drilling depth or low recovery (in part due to technical problems). This method yielded a close three-dimensional network of sections that guaranteed the reliability of the correlations.

It is typical of the level of HEIJNEN & TEN HAVE's criticism that they chose Section II to support their claim that the average distance between successive drill holes is too large for reliable correlation. It is indeed true that the average distance in Section II is roughly 70 m, 73 m to be exact. HEIJNEN & TEN HAVE chose, however, not to mention that the average distance for Section I is 38 m, for Section III 48 m, and for Section IV 36 m. In fact, 45% of all correlations were made over distances of less than 40 m. Furthermore, as has been indicated earlier, in many cases the actual distances between successive data was even less, due to the availability of additional 'less complete' drilling logs.

'Correlation is the demonstration of equivalence' (KRUMBEIN & SLOSS, 1963, p. 332). 'It requires the establishment of closed correlation traverses and correlation networks' (op. cit., p. 338). I applied these and other general rules of correlation correctly, and subsequently could define the zones of maximum mineralization and their internal geochemical organization, as presented in the paper in question.

With regard to the reconstruction of the Seroe Domi Limestone–Curaçao Lava Formation contact, it is hardly surprising that independently reached reconstructions, based in part on the same data, show some notable similarities. However, owing to the much larger amount of data I used my reconstruction does differ considerably from the earlier one, especially in the eastern part of the region.

STRATIFICATION

In their paragraph on stratification HEIJNEN & TEN HAVE display a remarkable disability to quote correctly. I (STIENSTRA, 1983, p. 275) did not claim the relative dominance of stratification on the phosphatization process. I attributed a role of at least similar importance to the hydro-geochemical environment as defined by the phreatic fresh water–salt water mixing zone (op cit. pp. 275, 282, 283). Furthermore, I stated that the mineralized zone as a whole, as well as the different phosphatic units, follow the trends of both the limestone–basalt contact, and the pattern set by bedding and colour stratification, which is illustrated in figure 6 (op cit. pp. 281, 282). This differs considerably from HEIJNEN & TEN HAVE's erroneous quotation in their comment, which states that 'the mineralized zone is controlled by ··· the pattern ···'

The supposedly 'contradictory' statement that bedding and colour stratification dip to the north arises simply from careless reading by HEIJNEN & TEN HAVE. The original article states quite clearly that it is the axis of the syncline that has a northerly dip. Furthermore, it is mentioned that the syncline appears to be closed to the north.

These, and other, mis-quotations by HEIJNEN & TEN HAVE do not contribute to the quality of their argument, and make it difficult for me to reply with the necessary conciseness.

With regard to the measurements presented, I purposely assigned qualitative terms to them. I have suggested that the colour bedding may–at least in part–be congruent with primary stratification (STIENSTRA, 1983, p. 278-279). This is supported by field observations which indicate that in many cases colour changes coincide with petrological changes (like in porosity and bioclast content). It is furthermore based on the theory that the pink-red colours are caused by the presence of minor amounts of organic matter, as presented by DE WIJS (1963), and also put forward by HEIJNEN & TEN HAVE themselves when they described the red-pink limestones which envelop phosphate pockets (TEN HAVE & HEIJNEN, 1978, p. 52). TEN HAVE & HEIJNEN did not analyze the organic matter in their samples (op cit., p. 115). Therefore, they are hardly in a position to state now that the red-pink colour is not caused by it.

FISSURES AND FRACTURES

HEIJNEN & TEN HAVES find STIENSTRA's statement that fractures and fissures are not genetically related to phophatization' incorrect. It must firstly be pointed out that this is hardly an accurate representation of my original statement that 'the patterns of fissures and faults and phosphatization, respectively, do not suggest a direct genetic relationship' (STIENSTRA, 1983, p. 282). Secondly such a conclusion is in perfect agreement with the earlier views of HEIJNEN & TEN HAVE as presented in their report of 1978 (p. 57, 144). In that investigation TEN HAVE & HEIJNEN were able to find only one small phosphate pocket that was in direct contact with a fissure. It is this single example that is illustrated in their comments (Fig. 1). It is not a little surprising to see that after seven years HEIJNEN & TEN HAVE have now reversed their earlier views as to the importance of fissures in the phosphatization process, particularly since, to the best of my knowledge, they have not during this time carried out any additional fieldwork in the area.

The limited importance of fissures and faults is once again illustrated by the low P_2O_5 content I found in two samples of red-brown oolitic fissure filling: 7.50% (sample C6) and 1.32% (sample N6b). In the phosphate zone about 60% of the measured mean P_2O_5 values vary from about 14% to about 28%. One would expect similar or even higher values in the 'ideal conduits' of the phosphate brine.

Furthermore DE WIJS (1962), after ample research, concluded that the relationship between fractures and the ore-body is 'not simple and not cogent'.

Of similarly limited importance are the solution holes and the karsts. These supergene structures are usually not deeper than 10 m, and therefore do not even reach the zone of maximum mineralization!

GENETIC PROCESSES

It will be obvious to every one who compares figure 7 of STIENSTRA (1983) with figure 2 of HEIJNEN & TEN HAVE's comments that it is very difficult indeed to maintain that both figures are similar but for the stratification, as the latter authors argue. Furthermore, the reader will appreciate that figure 7 makes a point quite different from figure 2.

It will also be clear that, especially in figure 7b, the average dip of the zone of maximum mineralization is less steep than the average dip of the stratification. Both trends, therefore, do intersect. This will even be more obvious if one reads 'set of limestone beds' instead of 'limestone bed' in figure 7 (symbol 5).

CONCLUSIONS

a). Much of the criticism of HEIJNEN & TEN HAVE on my methods and results (STIENSTRA, 1983) is based on misinterpretation of important sections of the article, as they themselves have illustrated by many misquotations in their comments.

b). Part of the apparent misunderstanding of HEIJNEN & TEN HAVE might be due to their limited knowledge of all available drilling data.

c). HEIJNEN & TEN HAVE obviously fail to appreciate the differences in scale and character between the geochemical zonation within the rock-phosphate body of Table Mountain Santa Barbara (STIENSTRA, 1983), and the petrological features described in their own papers. These results, however, are not mutually exclusive.

d). HEIJNEN & TEN HAVE's assumption that much previously published and unpublished material has been used in the paper in question without due references is misleading, unjustified and therefore insulting.

ACKNOWLEDGEMENTS

I am greatly indebted to Dr.E. Nickel for his continuous support in the cause of the project on the phosphate deposits of the Leeward Netherlands Antilles (project W 75-215, Netherlands Foundation for the Advancement of Tropical Research, WOTRO). Furthermore I acknowledge the valuable comments on the manuscript by members of the staff of the Comparative Sedimentology Division, Institute of Earth Sciences, Utrecht. Especially the remarks by Dr.T. de Mowbray, Dr.A. van Gelder and Dr.E. Nickel, contributed considerably to the final version.

REFERENCES

De Wijs, H.J. 1962 First report on the Santa Barbara phosphate property, Curaçao–Unpubl. Rept.–N.V. Mijnmij Curaçao.

De Wijs, H.J. 1963 Second report on the Santa Barbara phosphate property, Curaçao–Unpubl. rept.–N.V. Mijnmij Curaçao.

Hutchinson, G.E. 1950 The biogeochemistry of vertebrate excretion–Survey of existing knowledge of biochemistry 3. Bull. Am. Mus. Nat. Hist. 96: 594 pp.

Krumbein, W.C. & L.L. Sloss 1963 Stratigraphy and Sedimentation, 2nd ed.,–W.H. Freemand and Comp., San Francisco: 660 pp.

Steinen, R.P., R.K. Matthews & H.A. Sealy 1978 Temporal variation in geometry and chemistry of the freshwater phreatic lens: the coastal carbonate aquifer of Christ Church, Barbados, West Indies–J. Sediment. Petr. 48: 733-742.

Stienstra, P. 1983 Structure and general chemistry of Table Mountain Santa Barbara Phosphates, Curaçao, Netherlands Antilles–Geol. Mijnbouw 62: 275-284.

Ten Have, T. & W. Heijnen 1978 Fosfatisatie van de Neogene Seroe Domi kalksteen formatie in de Tafelberg Santa Barbara, Curaçao, Nederlandse Antillen–Unpubl. Rept. Sediment. Dept. Univ. Leiden (The Netherlands): 158 pp.

Ten Have, A., W. Heijnen & E. Nickel 1980 Alterations in guano-phosphates and carbonates–a study on phosphate-carbonate diagenesis on Curaçao, Netherlands Lesser Antilles, Caribean Sea–Int. Assoc. Sediment., 1st European Meeting, Bochum: 244-246 (abstract).

Ten Have, T., W. Heijnen & E. Nickel 1982 Alterations in guano phosphates and Mio-Pliocene carbonates of Table Mountain Santa Barbara, Curaçao–Sediment. Geol. 31: 141-165.