

## THE DECIPHER OF ANCIENT INSCRIPTIONS.\*

WE hope our readers will not be frightened by the titles of the books written below. Unlike as they appear, they have something in common; they involve a contest on important principles. We might add to them Baron Bunsen's *Egypt*, without really enlarging the scope of this article, which designs to say something on the controversy which Sir G. C. Lewis so perseveringly and laboriously pursues. We regret that the shortest and wittiest of these books, the decipherment of the Bruttian inscription, is not actually published, but only printed; hence we cannot refer to it as to that which the reader knows or may know. It has a delicious flavour of Latin criticism, which would sadly evaporate in translation; and to reprint the whole in Latin as it stands, might gratify a frac-

tion of our readers to the displeasure of the rest. Perhaps our best compromise will be, to give a summary of the purpose and result of this clever pamphlet, the authorship of which is no secret.

It informs us that Brown, Jones, and Robinson, academicians of Oxford and Cambridge, travelling in Germany, fell in with the celebrated Baron Munchausen, who presented them with a transcript of an inscription which he had copied in Calabria, stipulating that it should be published. The trio devolved on Brown the task of editing it, who forthwith betook himself to the study of Mommsen's explanation of the Oscan inscriptions, and Husehke's of the Umbrian. After this he found himself able to discover the sense of the mysterious lines, which ran thus:—

HEYDIDDLEDIDDLE  
THECATANDTHEFIDDLE  
THECOUIUMPEDOVERTHEMOON  
THELITTLEDOGLAUGHED  
TOSEESUCHIFINESPORT  
ANDTHEDISHIRANAUA YUITHESPOON

Mr. Brown's first task was to resolve this continuous writing into words, which he accomplished as follows:—

Hey diddle diddle | The catand, The fiddle, | The covium  
pedover, The moon | The littledo glang hed | tose esuch  
fines portand, The dishrana vay | vith The spoon.

He gains a clue to the general purport of the inscription from the reiteration of the remarkable word THE, which he readily conjectures to mean 'GOD,' by comparison of the Greek *Theos*. This assures him that it is a religious inscription. COVIUM he explains by a

comparison with Bivium and Trivium of the Latins, to be 'a place where several roads meet.' In THE MOON he discovers Deus Mundi. Regarding the tablet as probably recounting some sacrificial oblations, he interprets HED by Hoedus, a kid, and TOSE by Taurus, (*Toru*,

\* *An Historical Survey of the Astronomy of the Ancients*. By the Right Honourable Sir George Cornwall Lewis. London. 1862.

*Suggestions for the Application of the Egyptological Method to Modern History*. London. 1862.

*Inscriptio Antiqua in Agro Bruttiorum nuper reperta*. Edidit et Interpretatus est Joh. Brownius. Oxon. 1862.

*An Inquiry into the Credibility of the Early Roman History*. By the Right Honourable Sir G. Cornwall Lewis. 1855.

*Oskische Studien*. Von Dr. Theodor Mommsen. 1845. Nachträge. 1846.

*The Persian Cuneiform Inscription at Behistun, Deciphered and Translated*. By Major H. C. Rawlinson. (Vol. x. Part iii. of the *Journal of the Royal Asiatic Society*.) 1847.

Umbrian), the exchange of *s* and *r* being notoriously common in the Italian languages. ESUCHI FINES PORTAND opens its own meaning with great ease from Greek and Latin; namely, Esuch is  $\epsilon\sigma\omega$ , within; the final guttural being sometimes ad libitum in Greek (as  $\eta\ \eta\chi\epsilon$ ,  $\omicron\ \omicron\chi\epsilon$ ), though always vanishing in Latin. Thus the three words are equivalent to Intra fines portand(a). The last word SPOON, is evidently  $\sigma\pi\omicron\upsilon\delta\eta$ , libation. After thus roughly denoting the prominent words, we recur to the beginning. The record was likely to open with the name of the pious donor, who directed the double sacrifice of kid and bull, to be carried within the sacred limits. Mr. Brown acutely discovers in Cicero the name of Caius Heius or Heyus, a noble and wealthy citizen of Messina, who had a very ancient hereditary chapel, of great dignity. The first word of the inscription must surely contain this name. The second word, repeated, it seems, for energy, must contain the verb; and no doubt expressed that 'Heyus bestowed.' Mr. Brown, adopting a harsh process of derivation from Huschke, imagines that DIDDLE may be compounded of the Latin *Dedi* and the Greek  $\theta\epsilon\lambda\omega$ , so that the exact sense is, 'Heyus gave willingly.' The words FIDDLE and LITTLEDØ he resolves by the

same process, the former into Fave and  $\theta\epsilon\lambda\omega$ , the latter (or rather its element Little) into Lita—and  $\theta\epsilon\lambda\omega$ ; so as to mean 'favoured willingly' and 'wished as atonement.' CATAND he confesses to be obscure, and after a hesitating guess which would compare it with the Latin Quotannis, he ingeniously interprets it from  $\kappa\alpha\tau\alpha\nu\tau\eta\varsigma$  to mean propitious. In PEDOVER he discovers the Latin root *Ov-um*, an egg, combined with the Æolic preposition *Peda*, (Æolism we know to have prevailed in Italy), and the verbal termination *-er*, common in Umbrian. By learned quotations concerning 'the supper of Hecate,' he establishes that the Deity was honoured by eggs at the cross-road, which confirms the interpretation of Covium, and justifies us in rendering PEDOVER, *particeps ovi factus est*. The termination of *Littledo* is satisfactorily explained from the Gothic, as being that of the perfect tense. On GLAUG light is thrown from  $\gamma\lambda\upsilon\kappa\epsilon\varsigma$ , sweet, pleasant. DISHRANA is beautifully resolved by Greek into  $\delta\iota\varsigma$  and  $\rho\acute{\alpha}\iota\nu\omega$ , as meaning 'twice sprinkled,' viz., by the blood of the two animals. VAT is supposed to be connected with the word *Beatus*, blessed, and VITU with  $\beta\acute{o}\theta\rho\varsigma$ , a trench. Thus, on the whole, we obtain the translation,

Heyus gave willingly willingly.

God (is) propitious, God (is) favourable,

God partaker of eggs at the cross-road, God of the world.

God chose kindly for atonement a kid (and)

Bull, to be carried within the precincts.

God, twice sprinkled, blesses the trench of divine libation.

The satirist, in order to ridicule Huschke's derivation of *persklum* in the Eugubine monument from *precor*,  $\iota\sigma\chi\omega$  and the termination *-lum*, has purposely given a far less specious interpretation than was open to him, of Diddle, Fiddle, and Littledo. In fact every reader will see that Fiddle might have been explained as *Fidelis*, so that the second line would be rendered 'God propitious, God faithful.' But enough of this. Before we conclude, we shall express our opinion, whether any serious inferences can

be drawn from this pleasant piece; and if so, how far, and under what restrictions.

Sir G. C. Lewis's new work, on the *Astronomy of the Ancients*, is divisible into two nearly equal parts, very heterogeneous. The first 255 pages are on Greek and Roman astronomy, down to 100 A.D. In this portion the author treads on his own historical ground, and, like most other historians, is satisfied that his narrative is true. He does not profess to write as a mathematician, yet

he writes what might have been written by a mathematician. On the one hand, we certainly have no thought of controverting his statements, nor does he tempt a reviewer to oppose him; for no one can have less disposition to paradox, or is more devoted to unpretending good sense. On the other hand, it is difficult to find in this plain, serviceable, elaborate, and learned narrative, any salient points of interest to unlearned readers; so that, in fact, we have little to say about it. But the second part of the work, containing 260 pages, is almost entirely *negative* in its tendency. Whether he deals with Babylonian and Egyptian astronomy, with Egyptian or Assyrian chronology, with interpretation of ancient monuments, or with Phœnician navigation, the general conclusion which turns up on all sides, is, that there is no adequate ground for believing this and that on which learned men talk learnedly and argue absurdly. It is, collectively, a following out into the regions of the East the same principles of scepticism which the author had developed so elaborately in his two octavos concerning the Roman history. In the same direction is the ingenious pamphlet concerning the application of Egyptological principles to English and French history. To this whole subject then we propose at present especially to address ourselves. It involves two widely different branches,—first, the criticism of ancient documents which it is conceded that we understand;—secondly, the question whether we have attained a right understanding of them. In each, Sir Cornwall leans strongly to the sceptical side.

Whatever conclusions may ultimately prevail, we cannot but think that his protest is on many grounds called for, and is of very wholesome tendency. To make the protest requires much moral courage, and must be wholly ineffectual unless it come from a man of undoubted learning. We do not wish to use an offensive word; but the aspect of things is as though a

*clique* of learned men, who play into one another's hands by mutual praise, have for many years had the game too much to themselves. They often make enormous demands on our credulity, and they assume the high tone of *science* while dealing with what to others appears highly uncertain literature. We may date the latter assumption from 1847, at the British Association, which then met at Oxford. By the great influence of the late Baron Bunsen, whom we cannot mention without respect and honour, backed up as it was by the earnest and rightly intentioned influence of the late Prince Consort, the men of literature for the first time succeeded in effecting an entrance into that association for science. We by no means blame the very harmless and reasonable desire of using the opportunity, afforded by the meeting of so many intelligent men, to attract their attention; but we think that all the incident expenses should have been provided by a separate fund. In that year alone the Report of the Association was burdened by nearly two hundred closely printed pages, perhaps equivalent to three hundred and sixty pages of an *Edinburgh Review*. Nearly a quarter of this was Bunsen's elaborate essay on the light thrown on ethnology by the recent Egyptian researches. In the next year the first volume of his voluminous work on the *Place of Egypt in History*, was laid before the British public, which threw some light on the new meaning which we are expected to put on science. On the linguistic part of that work we speak with the highest respect. The results may be as uncertain as Sir Cornwall appears to be convinced; but there is no ambiguity, no shuffling, no veiling of anything in obscurity; but a thoroughgoing effort to exhibit the vocabulary of the old Egyptian, with the sounds expressed in European type. Yet the critical part of the book may well excite doubt and surmises in those who pretend only to broad and, it may be, superficial views. The first remarkable thing to us is the

totally new relative position which he assigns to chronology. In Greek and Roman researches (which Sir Cornewall will allow to be *ιστορια*, if they are not *historia*) we are accustomed to believe that early chronology is far less trustworthy than early accounts of fact. That there was an era at which the separate towns of Attica, which previously had been petty independencies, coalesced into a single nation, probably Sir Cornewall and Mr. Grote will alike concede to us; and we hardly think they will treat us as very credulous for believing, until it is disproved, that the chieftain who effected this union was called Theseus; yet we cannot pretend to know the date within two or three centuries. So the Dorian invasion of the Peloponnesus was a great memorable historical fact; yet the date is very uncertain. Again, we are unable to doubt that there was an invasion of ancient Rome by a Sabine people, who had a different language and religion from that of the previous inhabitants, and that they succeeded in imposing their religion and their political organization; nor can we see that it is any crime against reason and historical science to believe that the chief Sabine legislator was called Numa. But again; we hold the *date* of Numa to be extremely uncertain, and of very little importance. But Bunsen, regarding the chronology of Greece before the Olympiads, and of Palestine before King Solomon, to be simply non-existent, looks to Egyptian chronology as likely to 'impart a new element of vitality' to the other national histories. We there find him to regard Champollion as having been led into 'a fatal error' (p. 221) of seven

centuries by misunderstanding a remark of Syncellus (which no one before Bunsen understood) concerning the 'Sothiac cycle of Manetho.' Now, what have we received of Manetho to give so high value to his chronology? Certain dry lists of kings, all of which need to be corrected by Bunsen. The process is thus briefly indicated by him (pp. 97, 98): 'Manetho's historical work comprised a period of 3555 years . . . . The sums of the reigns in the individual dynasties make up considerably more than 3555 years; consequently (!) they were not all consecutive, but must some of them have been contemporary.' When we find so learned and really able a man reason thus, we see that it is not superfluous for other learned men to write elaborately on the other side. Sir Cornewall has shown an exhaustive diligence, and has written with great force on this whole question. A few of his summaries, which are concise enough to quote, will have interest with the reader (p. 343):

According to the report of the priests to Herodotus, the divine dynasties governed Egypt for at least 5600 years; and these reigns were stated (*Herod. ii. 145*) to have been the subject of contemporaneous registration, not less than the subsequent human reigns. *Men*, the first (human) king, and 330 *unnamed* successors, reigned 10,320 years . . . and 12 *named* kings from Moeris to Sethon, reigned 400 years. . . .

Manetho gives to the divine and semi-divine dynasties a duration of 24,925 years. . . . Eratosthenes enumerates 38 Egyptian kings of Thebes, beginning with Menes. They reign 1076 years. . .

The four schemes [of Herodotus, Manetho, Eratosthenes, and Diodorus] harmonize in making Menes the first human king of Egypt, but they differ widely in the dates which they assign to him. . . .

Date of Menes according to . . . .	{	Herodotus, B.C. 11,400.
		Manetho, ,, 5,702.
		Eratosthenes, ,, 2,600.
		Diodorus, ,, 5,000.
The average duration of reigns in these several schemes are . . . .	{	Herodotus, 33 $\frac{1}{3}$ years.
		Manetho, 11 $\frac{1}{2}$ ,,
		Eratosthenes, 28 ,,
		Diodorus, 9 $\frac{1}{4}$ ,,
Number of reigns from Menes to Psammetichus according to . . . .	{	Herodotus, 343.
		Manetho, 439.
		Diodorus, 470.

If we attempt to compare the names of the kings and to determine their mutual correspondence in the several lists, the discrepancy is still greater, and the confusion still more hopeless. . . . *In the later period*, subsequent to the reigns of David and Solomon, and in part even

later than the poems of Homer and the era of the Olympiads, *this discrepancy is most remarkable.* The nearest approach to agreement is between Herodotus and Diodorus; but how near that approach is will appear from the following:—

*Herodotus.*

1. Moeris . . . . .
2. Sesostris . . . . .
3. Pheros . . . . .
4. Proteus . . . . .
5. Rhampsinitus . . . . .
6. Cheops.
7. Cephren.
8. Mycerinus.
9. Asychis.
10. Anysis.
11. Sabacos.
12. Sethon.
13. Dodecarchy.
14. Psammitichus.

\* \* \*

*Diodorus.*

1. Moeris.
2. Interval of 7 generations.
3. Sesösis I.
4. Sesösis II.
5. Interval of numerous kings.
6. Amasis.
7. Actisanes.
8. Mendes.
9. Anarchy of 5 generations.
10. Ceten or Proteus.
11. Remphis.
12. Interval of 7 generations, during which Nileus is king.
13. Chemmis.
14. Cephren.
15. Mycerinus.
16. Bocchoris.
17. A long interval.
18. Sebaco.
19. Anarchy and dodecarchy.
20. Psammitichus.

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(P. 347.) These discordant schemes all profess to be derived from the same authentic source. They cannot be reconciled by any legitimate methods of criticism, and yet there is no satisfactory ground for preferring one to another.

To ordinary men it might seem that the very last thing which can be elicited out of such sources is a trustworthy chronology. But there may have been a king Moeris, a king Cephren, a king Mycerinus. However dry and uninteresting to know merely their names, or to know that one dug a lake and two of them built pyramids, it still may be believed without folly or caprice; but to settle at what date, from such authorities, is surely a hopeless task. At the same time, Sir Cornwall produces abundant reasons for holding with a very loose hand many things currently received concerning early Egypt; though we do not pretend always to know\* rightly what the professed Egyptologists hold. He seems to be quite justified in say-

ing (p. 351) that 'the Egyptians did not permanently subjugate any people beyond the valley of the Nile.' The book of the Old Testament itself is to us sufficient proof of this from the time of Moses downward. Moreover, if at any earlier period there had been a permanent dominion of Egyptian kings in Arabia, Asia, or the Cyrenaica, there would surely have been left numerous monuments similar to those of Egypt itself, instead of being, as now, quite isolated and exceptional. Their whole system of religion, their polity and organization, appear to have been intensely local, and probably could not have been maintained at all when transplanted; moreover, the aversion of the Egyptians to foreign contact and to the sea before the

\* A very calm judge, Mr. Kenrick, who has worked hard at the hieroglyphics, says (*Egypt*, vol. ii. p. 63),—'The warlike expeditions of the Egyptian Kings no doubt included Ionia, &c.:' yet we cannot discover that he believed in any permanent extra-African dominion, even of Sesostris.

time of Psammetichus, is notorious. The exploits of Rameses or Sesostris may or may not have had some foundation of fact; but we are unable to believe *more* than that an Egyptian king temporarily overran part of Asia.

The Assyrian chronology is hardly less vulnerable than that of Egypt. Few persons, we suppose, will consider it worth while to criticise such a table as the following from Berosus:—

(Sir G. C. L., p. 401.)

Kings of Assyria.

1. Alorus	reigned	36,000 years.
2. Alaparus	„	10,800 „
3. Amelon	„	46,800 „
4. Aminonon	„	43,200 „
5. Amegalarus	„	64,800 „
6. Daonus	„	36,000 „
7. Euedoraachus	„	64,800 „
8. Amempsinus	„	36,000 „
9. Otiartes	„	28,800 „
10. Xisuthrus	„	64,800 „

In fact, the only part of the traditional account worthy of attention, is the scheme of chronology from Nabonassar downward; but there is a far deeper difficulty in the utter discord of ancient accounts as to all the most fundamental points in the Assyrian Empire. Sir George C. Lewis, p. 419, says:—

The discordance between the accounts of the profane writers of antiquity respecting the history of the Assyrian Empire is so great, that it is scarcely possible to institute any comparison between them. They seem to relate to different countries, so rare are the points

<i>Berosus.</i>	<i>Years.</i>
1. Asordanus . . . . .	8
2. Sammughes . . . . .	21
3. Sardanapalus . . . . .	21
4. Navopolassar . . . . .	21
5. Nabuchodrossor . . . . .	43
6. Amilmarudoch . . . . .	2
7. Neriglissar . . . . .	4
[Laborosoarchod, 9 months.]	
8. Nabodenus . . . . .	17
9. Cyrus . . . . .	9

of agreement. They differ in the duration of the Empire; the time and mode of its foundation; the time and mode of its overthrow; the names of the kings, their acts, and the durations of their reigns. In this state of confusion and conflict, modern chronologists . . . have resorted to two contrivances. They have . . . supposed a double Assyrian Empire in relation to time,—an old and a new Empire: they have likewise supposed a double Assyrian Empire in relation to space,—one whose capital is Nineveh, another Empire whose capital is Babylon. Grant these hypotheses, and four discordant schemes of Assyrian history are provided each with a separate compartment.

Sir Cornwall further argues that the modern harmonizers have no right to assume that the kings of the 'Astronomical Canon' from Nabonassar to Cyrus are kings merely of Babylon, contemporaneous with the kings of Assyria.

This expedient [he says] is in the highest degree arbitrary; for when a continuous line of kings terminating in Cyrus and the other kings of the Persian Empire is presented to us, the natural supposition is, that the predecessors of Cyrus were, like him, the sovereigns of a great Empire, and not the chieftains of a single city. It is, moreover, important to compare the last eight names before Cyrus in the list of Berosus and in the Astronomical Canon. The names of Asordanus and Asaradinus nearly agree in sound, but the lengths of their reigns differ. In the seven next, the agreement of the years of the reigns is so close, that it cannot be fortuitous. The names of the kings likewise correspond closely in Nos. 4, 5, 7, 8.

<i>Astro. Canon.</i>	<i>Years.</i>
1. Asaradinus . . . . .	13
2. Saosduchinus . . . . .	20
3. Ciniladanus . . . . .	22
4. Nabopolassar . . . . .	21
5. Nabocolassar . . . . .	43
6. Illoarudamus . . . . .	2
7. Nericasolassar . . . . .	4
8. Nabonadius . . . . .	17
9. Cyrus . . . . .	9

Now, Berosus does not profess to give a merely Babylonian dynasty. The kings in his series belong to the Assyrian Empire. . . . It follows that the kings of the Canon cannot be considered as exclusively Babylonian kings.

We are disposed here to take one step in the direction of the critics whom Sir Cornwall is refuting.

He so far relaxes his general unbelief as to say (p. 428):—'The Astronomical Canon probably *con-*

tains authentic materials; but it is a complete historical puzzle.' If he admit thus much, and does not treat it as an arbitrary fabrication, the name Cyrus at the close proves something to us. Cyrus was son of Cambyses, a Persian: and it is notorious that Nebuchadnezzar was King of Babylon. The ordinary story, that Nineveh was captured, and its Empire divided between Medes and Babylonians in the time of Nabopolassar (if it be received), at once explains the Canon. For Nabopolassar and Nebuchadnezzar, remaining kings of Babylon, were no longer subject to a *suzerain* at Nineveh, but became themselves *suzerains* to the greater part of the Assyrian Empire. As the name of Cyrus at the end of the list will never prove that the kings named above were Persians, and not Babylonians, much less will it prove that they were Assyrians, not Babylonians. On the other hand, Sardanapalus is as notoriously Assyrian as Nebuchadnezzar was Babylonian: so too does Asordanus seem to be the Biblical Assyrian King Esarhaddon. A Sardanapalus is reputed to be the last King of Nineveh. The Canon therefore, *primâ facie*, gives a list of successive Emperors, without distinction of lineage or capital city. The earlier emperors, down to Sardanapalus inclusive, reigned at Nineveh; those from Nabopolassar to Nabonedus, at Babylon; but Cyrus at Shushan and Pasargadæ. If it be asked why the Canon runs up only to Nabonassar, we of course cannot answer: yet the conjecture is not unreasonable, that a more accurate computation of time began from this king, as with the Greeks from the Olympiads.

Sir G. C. Lewis certainly will not claim originality for his scepticism of these Assyrian and Egyptian reigns: in fact, we believe it is not too much to say that thirty or forty years ago they were treated as quite worthless to history. But his able and racy exposition of the facts has a freshness, fulness, and vigour which make it completely his own. His summary stands thus concerning the Assyrian question (p. 431):

VOL. LXVII. NO. CCCXCVII.

Like the Egyptian chronology, the Assyrian chronology comes down to us in the shape of divergent lists of kings, dissociated from history; and these are subjected by modern critics to a free and discretionary treatment, in which names and numbers go for little. . . . In general, ALL that is recorded of an Assyrian king is his name and the length of his reign. It is easy to identify him with another king, either better known or equally unknown: and if the reigns differ in length, to alter the text, or to suppose that the father admitted the son to a share of his power during the latter part of his life, and that this concurrent period is omitted in one statement and included in the other. Voltaire said that etymology is a science in which consonants go for little, and vowels for nothing. It may in like manner be said that chronology, as treated by the restorers of Assyrian and Egyptian antiquity, is a science in which numbers go for little, and names for nothing.

The illustrations in detail fully bear out this cutting remark. We do not see how to deny that the inquirers into the singular documents of antiquity, whom Sir Cornwall writes professedly to disparage, damage their cause extremely by making any effort to elicit a trustworthy basis out of such materials. Let them be satisfied with learning, as a basis to build upon, that *such and such were old Egyptian or Assyrian names of kings*: for this may be of immense importance. If four or five writers had given fragmentary accounts of the English dynasties, all inconsistent, a future student might yet reasonably believe on their joint authority that Edward, Henry, William, Alfred, had been names of English kings; and if such names occurred in a number of Latin royal documents, after Latin was a lost language, he might have obtained a valuable clue to interpretation. If one of them began, *In tertio anno regni regis Edwardi*, and another began, *In quarto anno regni regis Henrici*, a future decipherer—though of very moderate powers, and quite unaware of being a genius—from the mere knowledge that Edward and Henry were names of English kings, might guess rightly at the outline of the

meaning; for the similarity of sound between *regni* and *regis* would suggest to him that it meant 'kingship of the king,' and the contrast of *tertio* and *quarto* would mark ordinal numbers referring to the year of the reign. This would make him believe that *anno* meant year; after which he would be sure that *In* was a preposition, of sense clear enough. The apposition of only two clauses will thus often suffice to suggest a reasonable interpretation; but of course, after this, a verification is needed. If, then, after the student had got thus far, he had met a third, and even a fourth document opening in nearly the same way (suppose, 'In *quinto* anno regis *Gulielmi*,' where *regni* is dropped), he would instantly feel that he has the key for understanding it. Nevertheless, as yet he knows nothing of *quinto*, *quarto*, *tertio*, except that they are ordinals; and cannot yet determine whether their common ending *-o* is the mark of a case, or, as in Italian, an ordinary ending of nouns in every case. But on further comparing *Eduardi*, *Henrici*, *Gulielmi*, he would justly infer that the final *-i* was a mark of the genitive: for the genitive sense is needed, and there is no particle to express it. Instantly, this conjecture would be confirmed by the word *regni*, which is also in *-i*. On the other hand, since *regis* stands before *Henrici*, &c., he would presage that in *regis* the *-is* is a mark of the genitive, and that the nouns of the language differ one from another in declension. On the other hand, if *regis* had always followed the name, as 'regni *Henrici* regis,' the inference would be invalid, until it were known whether a word in apposition follows our western law of concord. Yet the above single illustration will suffice to show the rapid and decisive results which a decypherer may deduce from the smallest beginning, if the materials are large, and favourable by their homogeneity and formal repetitions.

We cannot disguise from ourselves that Sir Cornewall, with his immense grasp of learning, takes

very limited views of this whole subject. He seems to have been unduly prejudiced against the research by the logical weaknesses to be often expected from enthusiasts. Who will spend his best intellect and valuable hours for months or years on a study, if he has not an enthusiasm?—and who can make discoveries without excitement and some elation? It is not by plodding and consecutive reasoning alone that discovery goes on: sheer guesses, one after another, are made, and are discarded on proof of error, until at last one is found of which the inquirer finds no disproof. If one, and only one, hypothesis has stood the test, the mere negative fact is of weight with him; but he cannot hope that it will weigh with a reader: he is therefore tempted to snatch at secondary evidence, which is no evidence at all. Mommsen has explained the word *herest* in Oscan to mean *voluerit*, and it appears every way probable; but when he refers to the Greek *αίπέω* as equivalent, he adds nothing to the argument, and does but afford to an incredulous reader something to smile or carp at. Sir Cornewall deals with the whole question *à priori* (pp. 378, 9), in a manner to which we cannot assent. He says it is a necessary condition for the intelligibility of a language that it have been preserved *unbroken*, as Latin and Greek have been; but that if the Basque were to become extinct and its dictionaries and grammars lost, nothing could recover it. He proceeds to insist, that the received laws for decyphering a secret alphabet presuppose that the language is understood, and that we know the comparative frequency of certain words in it. This process, he adds, if the document be long, is almost infallible: but it does not determine the meaning of a single word. 'The united ingenuity of the most skilful decypherers in Europe could not throw' [*i.e.*, has not been able (!) to throw] 'any light upon an Etruscan or Lycian inscription, or interpret a single sentence of the Eubugine tables.' Conceding for a moment the truth

of the last sentence, we should say that the whole argument takes a very partial view of the conditions of the problem. He has unfairly taken the Basque language as his illustration, but we claim to take another. Suppose all the languages of modern France to be totally lost, and that Italians tried to explain inscriptions, some in classical French, others in the Provençal dialect, others in the Breton. They would quickly understand numerous words of the Parisian tongue, but be sorely puzzled by others. It would soon appear that the Provençal was intelligible, where the Parisian was not; and (we say it under correction) it may reasonably be believed, that enough would be made out of it to enrich the student with a new store of Provençal words, furnished by the inscriptions. These might in turn aid to explain passages of the Parisian French, until the conjunctions and other apparatus of grammar were fully cleared up. After this, if the documents were numerous and favourable, there is no reason *à priori* why the inscriptions should remain unintelligible, though many isolated words would still be known by conjecture only, the materials being never extensive enough to verify *every* thing. Just so, we have numerous Homeric words known only by conjecture, since the links of tradition were not complete. But thirdly, as to the Breton inscriptions, they would no doubt be as great a riddle to the modern Italians, as the Etruscan are to the most learned: nevertheless, if any one who had full mastery of the Welsh addressed himself to the Breton, no one has a right to say that he might not succeed. We have not yet got any clue to the Etruscan. Perhaps we never shall; but this cannot be decided *à priori*. If we knew fully all the languages of the Caucasus, they might, or they might not, aid us. We fully admit that the Basque, once lost, would probably be lost for ever; because we know of no other language at all like it. But the main point, which Sir Cornewall entirely sets aside, is,

that—never mind by what wild guesses you solve your riddle—if your materials are large, you can always know whether it be truly solved or not. It is true that the Greek and Latin have come to us by an unbroken tradition; but if all evidence of this fact were lost, it would not impair our *certainty* that we understand these languages. We have the evidence in the things themselves. Sir Cornewall, as in history, so here also, immoderately runs down internal evidence.

There is nothing like a real example. The pamphlet on Heydiddle-diddle seems intended to ridicule Mommsen and others for their Oscan and Umbrian labours. Let us speak a little of the Oscan. Its traditions are almost wholly lost. We will suppose that we set out from three petty facts concerning it:—1. Livy says that *Medix tuteius* was the title of the chief magistrate of Campania; 2. Paulus Diaconus informs us that the Oscans said *Pitpit* for Quidquid. 3. Festus explains *Petorrita*, a four-wheeled carriage; as probably derived from the Oscan word *Petor*, four, if not from the Gaulish. The second fact, to one acquainted with the phenomena of Greek dialects, instantly leads to a valuable conclusion. We may be certain that in the place of Quis, Qui, Quum, Quando, Quem, we shall find words in Oscan which vary from these chiefly by having *P* in place of *Qu*. Next, since in *Petor* for Quatuor we have the same phenomenon as in *Pitpit* for Quidquid, we make sure that (just as in the relation of Welsh to Irish, or of Greek to Latin) the Oscans must have said also something like *Pempe* or *Pente* for Quinque. This is instantly confirmed by the name *Pontius* among the Samnites, which we now see is the same as *Quintius* of the Latins, and that *Ponti* is likely to be the more exact word. Further, we generalize from the above, by inferring, that the Oscan is, in the vague sense, Indo-European, and will be likely to retain words which run through this group of languages, such as *est* for 'Is.' Start-

ing from this miserably small beginning, let us take up the Bantian inscription and examine some of its clauses. We must premise that the words on the tablet are separated from one another by a point (which we request Johannes Brownius to remember); and at intervals larger spaces are left, as if to denote division of paragraphs. Moreover, not only is the word *Meddis* several times found in it, but *Prumeddixud*, *Senateis*, *Censtur*, and the abridgments *Pr. Q.* which seem to be the Latin Praetor, Quaestor. These are *à priori* reasons for believing it to be a political inscription. (It will presently appear that *Censtur* does mean Censor). Observe also that we infer the preposition *Pru*=*Pro*, from the word *Prumeddix*—*Pro*praetor: and we see that the language admits of composition, unlike to the Hebrew and Arabic. Let it further be remembered, that a law or edict is likely to have the phrase, 'If any one,' very frequently. Such topics, and their aid to interpretation, appear quite to have escaped Sir Cornewall. From the tablet we first extract:—'*Suae pis contrud exeic jefacust*'—*Pis* must be *Quis*, as we have seen: surely then *suae pis* means 'sive quis,' or 'si quis.' If so, the termination *-ust* marks a verbal tense, being derived from *est*. This at once gives us a rendering: 'Si(ve) quis contra haec fecerit,' and we see that the reduplicate *jefu* of the verb follows legitimate Indo-European analogies, and final *e* of *exeic* is as in *Haec, Illuc*. Well: this is but a guess. Like other guesses, it needs to be verified. Let us proceed farther. On looking through the document, we find *suae pis* to recur very often, and to be uniformly followed at a short distance by a word ending in *-st*, sometimes in immediate connexion, as: '*Suae pis pertemest pruter pam*' . . . which it is not very wild to render, 'Si quis pertigerit (or attinuerit) ultra quam.' . . . Here *Pam* answers to *Quam*, as it ought; but *Pruter* retains its *P* as in Ionic Greek *πρωτέρω* (further), if it be not Latin Praeter. Again, we have

*Pruter pam medical inom didest*, which (to jump at the sense of *inom*) might mean 'Praeterquam (or Ultra quam) magistratus ei dederit.' Here we see *Didest* is to *Dederit*, as *Jefacust* was to *Fecerit*. Presently, we observe, *Pod pis dat*; which must mean 'Quod' (or Quando) quis dat,' if *Didest* mean *Dederit*. Again: *Pr. censtur Bantsae [nep fu] id; nei, suae Q. fust, nep censtur fuid, nei, suae Pr. fust*.

We fear our Latin reader will hardly believe his own accomplishments, when we tell him—so great progress has he made in Oscan under our teaching,—that he is competent to judge of the conjectural reconstruction of the letters here lost. For, first, *suae Q. fust*, must mean, 'Sive quaestor fuerit,' which gives us the root *Fu* equally with *Es-t*, and shows us the predicate placed before the copula verb, as in Latin. Next, *fuid* must be the *fuat* of Plautus, equivalent to *Sit*, so that *nep censtur fuid* means, 'Ne censor sit,' and *Nei—nep*, are a double negative. Evidently then for [ ] *id* we rightly read *nep fuid*; and the sense is: 'Let not the Praetor be (also) Censor of Bantia' (observe, the genitive *Bantsae* precisely like Latin; if indeed it be not Censor at Bantia); 'nor, whether he be Quaestor, let him not be Censor, nor (yet) whether he be Praetor.' This clause of the law is against plurality of offices. Is all this indeed a dream?

But we have not invested all of our capital! We are forgetting the adjective *tuticus*. *Medix tuticus*: what is it? Praetor urbanus? or perhaps Arbitrarius publicus, as the old title of the Roman dictator was *Magister populi*. The termination *-icus* is just like Latin and Greek, and suggests that the element *tut* means *Populus* or *Urbs*. Will anyone scoff at our connecting it with the old German word for *People*, whence *Teutones* is generally derived, as well as *Tedesco* and *Deutsch*? Be that as it may, we find on the Bantian tablet, *Paei eizeis fust, paeanceansto fust, toutico estud*, which Mommsen (whether he was first, we know not) renders 'quod ejus

fuert, qui incensus fuerit, *publicum* esto?' whatever belongs to the man, who has neglected to be registered, let it be confiscated.—Is the reader still sceptical? We have not space to reason any longer, but we will write down a few more bits of translation, observing that so many of the lines are broken at one end, as to destroy hope of full interpretation without new materials.

*Pon censur Sansae* [read *Bansae*] *tantam* [sic] *censazet*, *pis ceus Bantins fust, censamur* . . . Quum censor *Bansae* populum censebit, quisquis civis *Bantinus* fuerit, censeatur. . . . Here we have *tantam* where we expect *toutam*. In another place occurs *pous touto devutuns*, quibus populus dividunt,—according to Mommsen. The accusative in *-am* and before the verb, the future in *-zet*, for *-bit* will be noticed; also the passive subjunctive (?) in *-amur* for *-atur*, the plural in *-uns*, for Latin *-unt*, Attic *-ουσι*: again the nominatives in *-is* and *us*, *pis ceus* = quisquis civis. Can any one read thus much, and deny that we are learning something?

*Pr. suae Praefucus pod post exac Bansae fust, suae pis op eizois* . . . Praetor sive Praefectus quando post hac *Bantiae* fuerit, sive quis apud eos . . . [*Suae* is here *Sive*, not *Si*. *Eizois* seems to be ablative, as in Greek *παρ'αυτοῖς*.]

*In suae pis ionc meddis moltaum herest, moltaum licitud*;—et sive quis eum magistratus multare voluerit, multare liceto;—Here *ionc* adds *c* to eum, as in the *istunc*, *illunc* of comedy; and the position of the words is just like Latin. It is, we admit, mere guesswork to say that *moltaum* means 'multare,' and that *-aum* final marks the infinitive; yet it is a valuable guess. So again, *ni lipid*, ne habeat (compare *fuud*, *fuat*). Again: *Suae pis contrul exaic prudipust*, where we would not render the last word 'prohibuerit' with Mommsen, but 'sive quis contra haec praeeperit, praeripuerit.' *Pas ex ascen ligis scriptas set, ne phim prudipid mais zicolois X nesimois*: *ejus id eadem lege (gen.) scriptum*

sit, necubi praecipiat plus jugeribus decem optimis. Several words here are uncertain, yet we see *mais* = *majus*, and the ablative in *-ois*, (like Greek) to express 'than' after the comparative. Our final quotation shall show the Oscan words for four and five; *petiroperit, neip mais pomtis*: *quatuor tenus* (?) *neve magis quinque*.

So much will suffice to show that the interpreters of Italian inscriptions do not (collectively) deserve to be satirized; and that (when the materials are tractable) rapid, numerous, and important results may be deduced, concerning a language which Sir G. C. Lewis will admit to be 'dead' in the strictest sense. The *Bantian* tablet is perhaps intrinsically the most useful relic of Oscan, but it is more broken than that of *Abella*. From these two sources chiefly, a pretty complete set of demonstrative and relative pronouns is made out, and on the whole much grammatical information.

In the *Umbrian*, we confess ourselves to be scholars less than forty-eight hours old, and our knowledge of what others have done is limited to what we find in *Lepsius's* edition of the inscriptions (1841), which has little beyond a few grammatical remarks. As far as we have yet been able to find, he only translates two short sentences that are essentially different, yet we do not see how any reasonable person can doubt their substantial correctness. The endless reiterations of the *Litany* give a great facility for analysis up to a certain point, but trouble one by an apparent vacillation as to spelling, and an arbitrary omission of final *m*. Besides, there is (if the deciphering be correct) a real uncertainty of pronunciation as to *r*, *rs*, *s*; comparable to that of *pp* *ps* in old and new Attic. Having made a clean breast as to our own recent ignorance of the subject, we may amuse the reader by adding, that on casting our eyes on the *Litany* for the first time, we could not resist the conviction that we understood much of it! and this, though the words are in vast

majority wholly un-Latin and new. For instance, it reiterates the formula, *Di Grabovie, tium subokau*. That *Di Grabovie* is the vocative appellation of a god, is certain: we have in like manner *Jure Grabovie, Marte Grabovie*. How then can we help understanding the words quoted, to mean, *Deus Prævalide, te veneramur; or, tibi supplicamus?*

Not to occupy the reader too much with details in which his enthusiasm may lag behind ours, we will here only add that the professed students of such remains are of two classes. The one sort may have suggestive genius, but they are careless to verify; and perhaps snatch more at the material knowledge, the (so-called) historical facts to be suddenly gathered, than at the more subtle and spiritual truth inherent in the forms of language. They would have us believe, that a storehouse of new information concerning empires or religion is suddenly open; and are not always free from secondary influences and motives, which seem to tinge their scientific zeal. The other class, like—(but we shrink from the quasi-judicial process of commending some and omitting other names)—are as patient and thoroughgoing as they are ingenious; and are fully aware that however valuable is the guess, to guess well falls far short of proof. But we fancy we hear some disciple of Sir Cornewall coldly reply: 'Truly said; and proof is impossible.' To this we demur: nay, against this we protest; as palsyng effort. Where materials are very narrow and imperfect, no single mind, we admit, nay, and no co-operation of mutually conscious minds, can give verification; nevertheless, it may possibly be had from the coinciding results of minds working separately. The public believed that a true cuneiform alphabet was made out, when it learned that Major Rawlinson had arrived substantially at Lassen's results in entire ignorance of Lassen's labours. When the Asiatic Society requested four students separately to translate the inscription called that of Tiglath-

pileser, it showed itself fully to understand in what quarter we are to look for verification. The result was far from satisfactory; but it may hereafter be improved, if students become more numerous.

If only two, working quite separately, were to alight on the same translation of words wholly unknown—as, if a second person were (as we above) to render the often reiterated formula of the Umbrian Litany, '*Deus Prævalide, tibi supplicamus*'—every one will see how high probability of truth this would carry.

In what serious sense then can we accept the clever satire which we set forth in the opening? against what class of critics is it justly directed? First and chiefly, against interpreters like the Rev. Mr. Forster, whom (strange to say) it seems to patronize in its closing paragraph. A gentleman finds marks in the rocks of Mount Sinai; he makes out of them an alphabet so as to satisfy himself, though others contend that he reads in a wrong direction. The language is wholly unknown, as are its relations to other tongues. The subjects of the inscriptions are equally unknown, and they are very short; yet the interpreter comes out with a perfect interpretation by a process which no one but himself understands, and which has no pretence to verification. A similar case is that of the late Dr. Donaldson, who fluently translated the two celebrated lines of the Agyllan pottery by the help (as he says) of the Scandinavian languages, without condescending to establish *à priori* what Agylla, a Pelasgian colony, had to do with Scandinavia, nor pretending that *à posteriori* any confirmation existed, or imagining any test of the truth. A third instance is in Grotfend's headlong omniscience concerning Oscan and Umbrian, which Lepsius has calmly and keenly reproved. Yet Grotfend did good service by his original guesses concerning the cuneiform character; nor would we discourage the mental activity of any man as long as he is sincerely seeking for truth, and is not led

astray by vanity into mendacious pretensions. It is not needful here to expose the unfairness of Heydiddlediddle in imagining an English ditty *to be found inscribed in Bruttia*; it suffices to say, that the extreme *shortness* of the piece makes all verification of guesses, however good, obviously impossible. Had it been longer, it would have refuted the interpreter.

We might weary our readers if we proceeded to dwell, as we should wish in this connexion, on parts of Sir Henry Rawlinson's labours, which we do not profess to know further than from some of his own publications. Our readers may not all be aware of the great length of the Persian inscription at Behistun. Any one who reads it in English type, especially if acquainted with the Greek grammar, may see at once that its translator, whatever his possible mistakes, is treading on firm ground, and has correctly interpreted so many and important words, that a tolerably complete interpretation is within reach of ordinary minds, advancing by routine. The Assyrian and Babylonian inscriptions are now a vast mass; but until they are all converted into English type, Western scholars cannot deal with them: moreover, the students *must* be very few. Those students therefore have no claim to be surprised, much less to be angry, if we, the many, who are thoughtful, yet necessarily ignorant of their lore, have uncomfortable misgivings that they are going much too fast. We have a very high opinion of Dr. E. Hincks' acuteness, and a keen relish for some of the results which he brings out; but we were put out of breath by an able paper of his concerning the language of Accad, communicated a few years back to the British Association. He therein tells us that the Accadites are the people from whom the Assyrians learned their art of writing, which is syllabic, not alphabetical; that ample materials now exist for unravelling their language *by means of* the Assyrian; and he gives us specimens of its

relation to Greek, Latin, and Sanscrit. This comes to us as wonderful, side by side with the vast discrepancies between the four interpreters of Tiglath-pileser's monument. In order that these studies should be put upon a safe basis, they must be more brought out into the light of criticism. We do not speak of the Behistun inscription, but of the other documents, enormous as they are in mass. Two processes may be suggested concerning them, which (if the present distresses pass off and prosperity return) might be supported by special subscription if the thing were taken up by influential persons. One process, the more expensive and perhaps the less widely useful, would be, to produce by photography diminished facsimiles of all the documents, carefully arranged and numbered for reference according to their locality. The other, which would allure ten times as many persons to the study, is—to print the same in European types so selected that one of our types (or one syllabic combination) shall uniformly denote one, and one only, native character. For characters which are supposed to be abbreviations, or to represent ideas not words, arbitrary signs must be used. In regard to Egyptian, if this cannot be done generally, let it be done as far as it can, or in those documents which best admit it. We confess disappointment and annoyance, that when Bunsen laid before the public a very thick volume laden with disquisition, containing also a considerable and important vocabulary of Egyptian, he gave us no single page by which we could judge for ourselves on what basis the great fabric rests. That Coptic has been a most valuable aid, we cannot doubt; nor does Sir Cornwall move us by showing that different inquirers assign to it different degrees of importance. The same phenomenon would appear, if several critics wrote on the amount of aid given by Latin to the interpretation of Oscan. But in any case, as Coptic is undoubtedly the modern off-

spring of Egyptian, the case of Etruscan, which has no offspring, or of the Basque, if that were to be lost, is no fit parallel.

The want of materials accessible to common critics, is that which creates an uneasy suspicion that all is not sound in these remarkable investigations. Add to this, that no new light is shed on history proportional to the pretensions. We do not forget (and we invite Sir Cornwall to reflect on it), that the Persian inscription at Behistun at once developed the fact that Darius, son of Hystaspes, had to re-conquer the Empire for the Persians. Mr. Grote, deviating from all previous historians, had already pronounced that this was the true interpretation of Herodotus; that the Medes had revolted against the Persian dynasty, and the whole Empire was in confusion. In profound ignorance that Mr. Grote maintained this, or that it could be made out from Herodotus, Rawlinson brought out from Darius's own avowal the same fact in a still stronger form. So much we say that we may not appear totally to disparage the historical value of these monuments. Yet, in fidelity to truth, we must add a few words, which we fear will be very exasperating to orientalists, who would read history from courtly records and royal pictures. Let us throw a part of our odium on a highly intelligent gentleman, who many years ago was British Ambassador at the Court of Teheran: for he it was that first made us incredulous. He stated that every year the Court historian solemnly read out to the king and to the assembled divan, a record of the events of the past year, which always consisted in a glorification of the sovereign and of his prosperity. If we can trust our memory of a distant conversation, he said that the events of the Perso-Russian war figured as Persian successes. But of one thing we are sure, and assert it positively: He attested to us that

the free gifts sent by the King of England to the King of Persia in the way of compliment, were all recorded by the historian as *tribute*; so that if a future inquirer had no other knowledge of the history than these documents might give, he would suppose the Empire of Persia in the nineteenth century to have included the distant island of Britain. What reason have we to suppose the sculptorial or pictorial Court historian in ancient Assyria or Egypt to have been a whit more truthful than those of modern Persia? Bunsen and Niebuhr, as profoundly as Sir G. C. Lewis, believe the early military history of Rome to be a tissue of falsehood. Was oriental royalty surrounded by an atmosphere more favourable to truth than Italian aristocracy?

We cannot read Sir H. Rawlinson's translation of the deeds of a certain Assyrian King (he will forgive our not remembering a name the pronounciation of which has been several times largely changed) without intense distrust; distrust, not of the interpreter, but of the record itself. Evidently it was the duty of the recorder, year by year, to say something to the king's glory. The monotony of his work betrays its origin, when every year he crosses the river and conquers a new people. As for pictures, we should as soon believe that a king was three times the height of common men, because he is so painted, as believe in his conquests over red-haired Scythians and vast negro nations, because a picture represents a troop of them brought to him in chains, or a string of animals ushering in the spoils of Africa. Caligula is not the sole Emperor who has celebrated imaginary triumphs. We need to see the disasters of an empire carved in stone by order of its king before we accept such monuments as a faithful source of knowledge concerning its foreign relations.