

original, or that its diction has been in any way modified or modernized, we may be prepared to listen to this; but nothing can shake the demonstration of its original date and geographical accuracy. The historical critics have thus at least one dated document from which they may, if so disposed, make a new departure in their investigations.

I do not propose to write a commentary on Genesis, and therefore in my next paper shall move onward to the narrative of the Deluge, which, if I mistake not, can now be very fully illustrated by geological and archæological facts, and referred to its true position as pre-Mosaic history.

J. WILLIAM DAWSON.

EPILOGUE.

IT is a fundamental point to prove that *Ἰουπαιάς* in *Luke* iii. 1 is an adjective; and, while I omit much that ought to be said on my side (especially as to the telling passage, Josephus, *Ant.* xiii. 11, 3), there is one argument which cannot be omitted.¹

Hitherto, in order to be quite safe, I have conceded that *Ἰουπαία* perhaps occurs as a noun in the fourth century;

¹ It is rather embarrassing that a scholar of so much higher authority than myself as Dr. G. A. Smith should interpose in the middle of my argument, to settle the question against me, as has happened in this case. My concluding remarks were crushed out of the February number by want of space, and were intended, in their slightly enlarged form, to appear in the March number. I am sorry that, though he tells me he is so, I cannot recognise in Dr. Smith an ally in this matter; and, if the editor will permit, I shall append a note, as brief as I can, to state reasons for thinking that he has mixed up two different questions and looked from two varying points of view. My point is that *Luke* iii. 1 is right, not by a side-issue (as Dr. Smith admits to be possible), but by virtue of facts and of the customary and regular usage of the country. *Luke* iii. 1, 2 is one of the two most important passages for the future biographer of the author; and it seems strange to me that the evidence given in it to date the composition has never (so far as I know) been observed. For the controversy with Mr. Chase, the geographical question raised by Dr. Smith is immaterial. He merely shows that *Luke* is perhaps wrong geographically; but he admits the adjective in iii. 1.

but I shall now try to show that no examples occur even then. In Epiphanius, *Hæres.*, 19, which I said was "not entirely certain" (see p. 52), the word is adjectival, occurring in a list where all the names are of that type, τῆς Ναβατικῆς χώρας καὶ Ἰτουραίας.¹ Eusebius, as quoted by Schürer, has Ἰτουραία ἢ καὶ Τραχωνίτις. But the corresponding entry is Τραχωνίτις χώρα ἢ καὶ Ἰτουραία, and as both entries are indubitably explanatory of *Luke* iii. 1, it is probable that χώρα should be inserted in the former. This is almost conclusively proved by the translations of Jerome, quoted on p. 53, *Ituræa et Trachonitis regio*² and *Trachonitis regio sive Ituræa*. There remains then no single passage in ancient literature to justify the noun, which has been forced on *Luke*.³

It is therefore safe to assert that τῆς Ἰτουραίας καὶ Τραχω-

¹ Incidentally we notice that the whole enumeration, "the Nabatic district, and the Ituræan (district), and the Moabitic and the Areilitic (Gad)," is inconsistent with Dr. Schürer's localization of "Ituræa." It denotes the Peræa, Nabatea on the south, Moab and Gad in the centre, and the Ituræans on the north; and Epiphanius evidently did not think of an Ituræan country in Lebanon. But Dr. Schürer dismisses all the Christian authorities as being prejudiced and determined to make *Luke* iii. 1 accurate. But surely Epiphanius, in discussing this heresy, was not thinking of the accuracy of *Luke* iii. 1; he was using independent authorities.

² The text here should probably be corrected to *Ituræa quæ et Trachonitis regio*. I wrote a note to this effect in my former paper; but omitted it, in order to leave no opening for criticism. The Greek makes the emendation almost certain. [Most of this article was in type in the beginning of January, when, as already explained, I was dependent on Dr. Schürer for the quotations from Eusebius; but after term began, Mr. Souter investigated the text for me in the Cambridge University Library. Lagarde gives the text Ἰτουραία, ἢ καὶ Τραχωνίτις. χώρα ἧς κ.τ.λ. It appears, therefore, that Dr. Schürer, accepting Lagarde's false punctuation, translated this as "Ituræa, which is also Trachonitis: a region of which," etc. But after our investigation, we cannot doubt that it ought to be translated, "the Ituræan or Trachonitic country, of which," etc. I observe that Ortelius, *Thesaur. Geograph.*, recognises that in Greek Ἰτουραία cannot be used as the name of a country, though he thinks that in Latin *Ituræa* can have that sense. The interpretation quoted by Lagarde, *l.c.*, p. 193, Ἰτουραία, ὁρεινή, seems to be merely an inference from the Greek term Τραχωνίτις.]

³ It is quite possible that the people had ceased to be known by this name in the fourth century; but I do not intend to assert that it was so. Schürer points out that Ituræi existed as late as 254-59: Vopiscus, *Vit. Aurel.*, 11.

νίτιδος χώρας cannot have the meaning which Mr. Chase assumed that it must have, for the following reasons, all of which we have discussed.

(1) *Ἰτουραία* is only an adjective, never a noun. To say that Luke used it as a noun is as much an error as to say that in the expression "the Bedouin and desert country" Bedouin is a noun, and the name of a land.

(2) Taking it as an adjective, we find that Luke is correct as a historian and perhaps even as a geographer, but taking it as a noun we find him making a false statement about the sovereignty of Philip.

(3) Eusebius, who on the interpretation of the words of Luke is the most satisfactory authority that could be found, confirms our interpretation in reiterated statements, slightly modifying the expression to make the meaning still clearer. He also confirms Luke in the geographical point.

(4) There never was a country Ituræa with a recognised and defined character; nothing existed beyond "the district over which the semi-nomadic Ituræans (Bedouin) roamed," *i.e.* *ἡ Ἰτουραίων*. Hence we see why *ἡ Ἰουδαία*, a real country, is correct; but *ἡ Ἰτουραία* is so pointedly and carefully avoided by all ancient authors. It is therefore a mistake in method on Dr. Schürer's part to begin by assuming that a country Ituræa exists, and then try to localize it.

Now that Ituræa has been demonstrated to be a figment, I repeat my assertion that the Greek words *must* have the meaning which Lightfoot and I have attributed to them, and that the rendering as a noun which Mr. Chase clings to is grammatically unjustifiable.

I claim to have in one more instance demonstrated Lightfoot's intuition and sense for the Greek of the period. He first, so far as I know, showed what was the proper way of taking these two passages.¹

¹ But, if the history of this interpretation be investigated, it will probably be found that several of the older scholars were right. Mr. Souter has sent me some quotations.

The next question is about τὴν Φρυγίαν καὶ Γαλατικὴν χώραν. I should try to prove the correctness of my interpretation on the following grounds, which I put together in rather haphazard order.¹

(1) Even if we allow that Φρυγίαν here may be a noun, the North-Galatian theory is inconsistent with the rule as to the use of the common article which the author of Acts observes.

(2) In the second century local usage Γαλατικὴ χώρα was pointedly distinguished in sense from Γαλατία, and could not (as the North-Galatian theory demands) be used as a mere synonym for Γαλατία.

(3) In the second century local usage Φρυγία χώρα was probably distinguished from Φρυγία used as a noun.

(4) The well-marked purpose of the paragraph xvi. 6-10 is turned into a false rhetorical device on the North-Galatian theory.

(5) The North-Galatian theory does not take διήλθον in the sense which is characteristic of the usage of this writer, and moreover, it makes Paul act in a way quite out of keeping with his ordinary method of travel and work.

(6) The North-Galatian theory lands every one of its advocates in geographical absurdities.

Some of these reasons are in themselves far from conclusive, and the North-Galatian theory can always be maintained by any one who is willing to accept a large allowance of gaps and dislocations and "omissions" in the narrative. These have to be so numerous that formerly I drew the inference absolutely which still seems to me necessary if the North-Galatian theory be adopted: such a narrative is not original first-hand history. But in every one of these cases the South-Galatian theory takes the terms in the way that is characteristic of the author's usage and of the first cen-

¹ The arguments are those which came before me during the Christmas vacation, my only available time for investigation at present.

tury ; it finds no " omissions " or dislocations ; and it shows how the changes of the second century led necessarily to the general misunderstanding in which the events have since been involved. I hope hereafter to prove by the same argument the South-Galatian theory and the first-hand and first-century character of *Acts*.

But from the length to which this exposition has already run, it is obvious that controversy must here cease on my side, leaving the word to Mr. Chase. Especially the fifth of this list of reasons cannot be stated adequately except by a long investigation (which is fully written, and in part printed long since), while the last is difficult to put in reasonable compass without incurring the charge of dogmatic self-confidence.

This is the penalty of replying to a critic. If one investigates a point thoroughly, one incurs the charge of going off on side issues or of wearying the public. If one omits any side or aspect of the facts bearing on any point, one is exposed to the charge of omitting facts of vital importance, and consequently of giving an inaccurate view of the case. On the Ituræans I suffer on both charges ; to many I seem to have said too much, while others find that I have omitted much that ought to be said if the case is to be fairly judged ; but, where I have refrained from discussing, I fully considered the points omitted.

Briefly, then, while acknowledging fully that my discussion of the subject is inadequate, I cannot find that Mr. Chase has added a single fact, or taken a view that helps me to complete my many defects in a single case. In the estimation of several acquaintances, his strongest point was the noun *Ἰτουραίας* in Luke iii. 1 ; and I have shown in a too short and allusive argument that the word can only be an adjective.

In order to avoid the charge of having first made a statement and then shrunk from arguing it out, a word must be

added on each of the two other points referred to in my first article.¹

The question as to *μὲν οὖν* practically comes to this. Does the paragraph begin with xvi. 6, as Westcott and Hort, etc., say, or with xvi. 5, as Tischendorf, etc., hold? There are good authorities on each side; and the South-Galatian theory is independent of the question. In my humble judgment the artistic flow of the narrative is ruined by Tischendorf's arrangement. Mr. Chase differs. Our Cambridge friends have emphasized in *THE EXPOSITOR*, January, 1893, the extraordinary care shown by Hort in regard to the minutest detail and comma of his text. His own friends and pupils are my authority for believing that, when he placed a break of his largest character, in one case at least, between a *μὲν οὖν* and a following *δέ* (altering thereby a text given by other great scholars), he had considered the point with the same care and cool judgment that characterised the rest of his work, and that he deliberately concluded that this arrangement (far from "obscuring the connexion," as Mr. Chase thinks it does) was calculated best to bring out the sense, the logical connexion, and the literary form of his author. I venture to agree with his judgment, consciously now, formerly unconsciously.

The question as to the sequence of the verbs and of the thought in xvi. 6-8 opens up a wide investigation. I maintain (asking liberty to complete and to improve the statement) my former point of view. Although the South-Galatian theory is quite reconcilable with the interpretation of *κωλυθέντες* as giving a reason for *διήλθον*, my personal preference is for the view already followed in my book. I venture to think that the construction is characteristic of the author, and characteristic of the period and of the development of style that mark it. I am ready to argue

¹ What is here said is not written in haste, but rests on a pile of MS., and is the result of as dispassionate a study as I am capable of making.

that both present and aorist participles are sometimes used by this and other authors along with a verb to indicate an action closely connected with that of the verb (often one that arises directly out of that of the verb), but subsequent to it logically and (in the general view) chronologically. And a more extreme statement is also, in my humble opinion, correct; even a past participle is used in that way in Latin. This usage is not known to me before Livy,¹ and it is perhaps characteristic of, and caused by, the change of thought and expression that accompanied the changed circumstances of life and manners under the early Empire. I would venture to suggest to Mr. Chase that a study of the gradual development of the view held on this point of syntax by that excellent scholar, O. Riemann, would be instructive. Meantime, I might express my view in the last words² which he wrote on this subject before his premature death (though he goes even further, and is less guarded in his statement than I am): il arrive souvent chez Tite-Live que le participe *passé*, actif ou passif, au lieu de marquer un fait *antérieur* à celui qu'exprime la proposition principale, marque une circonstance qui *accompagne* ou *suit* l'action principale." Thus in Livy, xxvii. 5, 9 we find *in Siciliam tramisit . . . Lilybæum revectus*, and in Acts xvi. 6 we find *διήλθον τὴν χώραν κωλυθέντες*. The Livian usage is the more extreme of the two, for *revectus* is the extreme limit and end of the action described in *tramisit*, while *κωλυθέντες* is coincident in time with the latter part of the action of *διήλθον*. (See also Virgil, *Georg.*, I. 206, etc.)

Were this question to be argued out, numerous examples which justify in the completest way my interpretation of Acts xvi. 6 might be quoted. That interpretation may be

¹ Riemann, however, says only, cet emploi du participe passé semble être plus fréquent chez Tite-Live que chez Cicéron ou chez César.

² The italics in the quotation are given as in Riemann's edition of Livy, xxvi.-xxx., p. 482.

wrong, or it may be right; the question is a fair one for discussion, and I shall read with care any reasons Mr. Chase has to advance showing that it is wrong. But when he says that a writer who spoke as, on my understanding, the author of Acts xvi. 6 did, "would be incapable of writing half a page of intelligible narrative, . . . it would not be worth while to waste our energies in studying his writings any more; they would remain beyond, because below, criticism," he merely betrays deficiency in knowledge of language and style in the period under discussion.

One other point. Mr. Chase says my words, "they passed through Mysia," are wrong, and that the Greek means, "they skirted Mysia without passing through it" (p. 409 *n.*). I maintain that my translation is correct grammatically and necessary geographically. In discussing St. Paul's methods of travel, I have examined the whole passage, have traced his path step by step through Mysia, showing that this is the *necessary*¹ sense of the Greek, and is guaranteed by a local tradition, which can be traced back probably as far as the second century, and possibly to the Apostle's friend, Onesiphorus; but this, like many other things, must wait. Meantime, I can only say that, in one point after another on which Mr. Chase is so confident in his statements and so free in his condemnations, I find no quite sufficient support for them either in the width or in the accuracy of his argument. His good intention and honesty of purpose, which led him to undertake the "task of testing theories and checking hasty conclusions," are obvious to every reader; but these qualities, excellent as they are, are not by themselves sufficient for the discharge of that most difficult, important, and responsible task.

¹ I do not mean necessary grammatically; on that ground Mr. Chase is quite justifiable, of course; but necessary on a wide view of the practice and usage of terms denoting travel in *Acts*. Also Mr. Chase does not explain how Paul could possibly reach Troas by "skirting" Mysia. Unless he went by sea he must have gone through Mysia.

Before passing from this subject, let us touch on the question of style; and devote a moment to settling our ideas about the author of Acts, and his style and rank as a writer. Let us put aside all prepossessions and estimate this chronicler according to his own claims as an historical authority.¹ This author (whom with Mr. Rendall, *EXPOSITOR*, 1893, p. 333, I believe to have been connected with Philippi²), appears to me to deserve a very high rank. His language will bear the most microscopic examination, and will repay it. The selection and arrangement of his materials show consummate art; and when we are struck with any apparent omission, or any seeming awkwardness, we should always scrutinize the place with redoubled care, for in such cases the seeming fault will perhaps be found due to a misapprehension of the writer's aim. He has observed several nice rules of language, thoroughly Greek in spirit, yet peculiar to himself in the form he has given them in order to satisfy delicate considerations of clearness and sense. Careful examination of these usages makes it possible to argue that the book is the composition of one hand, but that more than one written authority lay before the writer and influenced his expression; that the writer claims, and intends to bring out by various subtle touches (including the use of *we* and other devices) his claim, to have been present with Paul on certain occasions; that he describes with peculiar care, and

¹ This is a part of Roman social history, and is "taken for the moment out of the theological domain." My aim is to treat the author of *Acts* exactly as I would treat the author of the *Libri ab excessu Divi Augusti*. Thinking nothing about his theology, but only about his history and topography, I find in him many details which are redolent of the first century, and are (so far as my opinion is of any value) anachronistic and impossible in a writer of the second century. It may be that his facts are not all correct: in some cases the balance of evidence now accessible seems to be against his correctness. But I cannot find that first century historians were all unimpeachably accurate in their narrative; and such inaccuracies as occur are as intelligible in a writer of 60-90 as in a writer of 150.

² I shall advance other arguments besides his to this effect.

leads up with remarkable art to, the occasion when he first met the Apostle; and that his general plan is such that, if Paul had founded an important series of churches in any country, the author would not have passed over the fact in silence, except through ignorance, which would be fatal to the supposition of intimate acquaintance.

On this last point it is necessary to put very clearly the difference between Mr. Chase and myself. He says on p. 412, "Professor Ramsay cannot believe that, if St Paul really penetrated into Northern Galatia, St Luke would have given so little information about his visit there." This he meets by referring to other cases of "little information"; and he quotes Lightfoot, "nothing is more striking than the want of proportion in the *Acts*." On this subject an expression of Aristotle's rises to my mind. He says that scientific knowledge starts from the wonder felt that a thing should be so: it culminates in the state where one would wonder if the thing were not so. So with the silences of *Acts*, with which, as Mr. Chase says, "every student must have been struck" (p. 413). They are dictated by his plan, and form part of his intention, whereas silence about Galatian churches, if an important group existed, is inconsistent with that plan. The stages by which Judaic Christianity became the Church of the Empire and of the world are the subject of this prose epic; and idealized proportion, not the want of it, is its most striking characteristic. I should be surprised to find the foundation of the Galatian churches dismissed (as it is on the North-Galatian theory) with the same notice as the journey across Pisidia, which resulted in nothing and had no effect on history. That would be out of keeping with my conception of this historian's character and literary faculty; but, as Mr. Chase says, he cannot agree with me "as to what could or could not be written by a Greek author 'with any literary faculty.'" We are thoroughly agreed that our conceptions

of this historian's style are absolutely diverse; and it must, I believe, always be the case that an adherent of the North-Galatian theory will take a lower view of the style and art of this author than I do.

The controversy with Mr. Chase is ended on my side. Undertaken unwillingly, carried on with growing distaste, it seemed to me a duty. Whether it was so others may judge. If I have in any case spoken too sharply, I regret it. But while I would gladly have refrained from speaking at all, I am constitutionally unable, when I have to speak, to do anything beyond saying bluntly and plainly what I think. The task of expressing myself is so difficult that it absorbs my whole thought, and nothing exists consciously in my mind except the overwhelming eagerness to explain clearly what has to be stated. Mr. Chase says that I have not shown "the care and accuracy that are incumbent on a scholar." The accusation is, in my estimation, almost the gravest that can be made in the situation; and it is the only one, perhaps, that could at present have roused me to complete the work I began and intended to leave unfinished.

A word must be added, before closing, on the wider question (purposely left out by me) initiated by Dr. G. A. Smith as to the names Trachonitis and Ituræi.

Dr. Smith, who thinks that Dr. Schürer "has clearly shown that Ituræa and Trachonitis were originally distinct," starts with the assumption that there was a country Ituræa, *i.e.*, he in the beginning assumes the very point at issue. He wrote his paper in the belief that Josephus used the name, and that therefore there was a country to which the name applied. Then, at the last moment, he concedes in a note, p. 236, that Josephus did not use the name; but still he retains all the argument whose sole foundation is the false reading of Josephus. While he

emphasizes the looseness and variability of names in that land, he yet finds that this name Ituræa (whose very existence is disputed, and is given up by himself) extended "as far as the border of Trachonitis," though it cannot be proved that they ever overlapped. That he allows to be quite possible, but the express statements of Luke and of Eusebius that they did overlap do not, in Dr. Smith's estimation, suffice to convert the possibility into a demonstration: "we have no proof that their names ever overlapped."

I have not space to show in detail how Dr. Smith's actual statement of the ancient evidence is affected by his assumption that a "distinct territory" Ituræa existed. Had there been a "land of the Ituræi" distinct from other geographical districts, there would have been a name for it. Trachonitis is a Greek foreign name; what did its Semitic inhabitants call themselves? Surely Ptolemy's phrase *Τραχωνῖται Ἀραβες* compared with Dio's *Ἰτουραίων Ἀράβων* shows that Luke and Eusebius are right in giving Ituræi as the rough current designation of the people of Trachonitis. The whole distance from Anti-Lebanon to Trachon is twenty-eight miles (p. 236 *n.*); yet, when names were puzzling and varying, and Trachonitis extended far beyond Trachon, and "Ituræa" extended far east of Anti-Lebanon (p. 236, *l.* 12), I cannot agree that there is no evidence that they ever did more than reach exactly up to one another.

Dr. Smith repeatedly endorses Dr. Schürer's argument as to the Lebanon: since Ituræi are several times mentioned in the Lebanon in the last century and a half B.C., therefore the Lebanon is the real Ituræa, and the references to Ituræi in other districts are due either to extension of that people to south and east, or (according to the latter) to errors of Christian writers bent on supporting Luke iii. 1. They allow no place in their reasoning to the possibility or probability that a warrior-tribe of nomad Bedouin took advantage of the weak state of government in Syria while

the Seleucid rule was dying, to overrun part of the more settled and peaceful country. Dr. Schürer's words, *alle historische Zeugnisse weisen auf's bestimmteste nach dem Libanon*, resemble the argument of a man who should urge that, because indisputable historical testimony shows the Arabs in Syria during the seventh to ninth centuries, therefore the Arabs were, strictly speaking, a Syrian people who extended their hold over part of the country towards the south.

The Ituræi were (as Dr. Smith describes them in his eloquent and picturesque way) the one warlike tribe of the whole region. They were Ishmaelites, as he says, or Bedouin, as I have called them. The true home of such a race is, I venture to think, not the long settled and well-governed land between Lebanon and Anti-Lebanon,¹ but the country stretching from Anti-Lebanon south-east as far as the situation assigned them on Kiepert's maps. They were at home where we find them in earlier centuries, and in later centuries along with the other Ishmaelite tribes, engaged in continual warfare with even Reuben and Gad (1 Chron. v. 19), stretching far enough south to be named along with Moab by Epiphanius, and associated with the Arabs, in repeated references. I cannot see how language like this can justify Dr. Schürer in making the Lebanon district their proper and sole home. Dr. Smith seems to me to express the exact facts, when he says, p. 236, "Such language cannot refer to the main range of Anti-Lebanon, but must mean districts to the east of that, and therefore we must conclude that the Ituræans extended a good deal further east than Schürer seems willing to admit." These fierce and warlike nomads ranged over the eastern lands; no country was named after them, but districts called by various geographical names, Auranitis, Trachonitis, etc., were equally infested

¹ A people whose centre was there would not have preserved their rude, warlike, barbarian freedom, throughout the strict government of the powerful Seleucid kings. That the Ituræi extended from the east up to Anti-Lebanon is conceded on my side.

by them. When the Syrian administration was weak they pushed their power even into Galilee and into Cœle-Syria; when government grew stronger they were driven back to the east. As Roman administration advanced, it pursued its usual policy, first putting these frontier tribes under the rule of kings dependent on the empire, such as Philip, and finally incorporating them in the empire. As the empire advanced, nomadism disappeared, and the population of Auranitis, Trachonitis, etc., settled to the arts of peace, cities sprang up where nomad encampments had once been the rule; and the Ituræans disappeared, for the nomadic name is always dropped by the reformed nomad.¹ Hence we find that Ituræi are hardly spoken of as existing later than the third century.

The passage of Luke iii. 1 gives us the clue to understand this whole historical process. I vainly tried to form any connected historical idea of the Ituræans until that passage showed the true path. Then every other reference becomes clear and natural. Without that passage, the subject remains as obscure, perplexing, and inconsistent with itself as it seems to me to be in the discussions of Dr. Schürer and Dr. Smith.

I shall not dispute with Dr. Smith about the value of Eusebius's evidence, being independent of it. I merely point out that he practically denies that a statement by Eusebius has any topographical value. Eusebius, he says, makes mistakes.² So, I may add, does Strabo in regard to Asia Minor; but I reckon Strabo by far the highest authority on Asia Minor. It is one thing to make an error

¹ Even in Turkey, where progress is so slow, the nomadic Turkmen tribe settles down into the Turkish villager; and the name Turkmen is dropped (unless difference of religion preserves it in the memory of the neighbourhood, for many Turkmens are Kizil-bash and abhorred by the orthodox Turks).

² Dr. Smith was misled by a bad edition of Josephus: had Eusebius always an immaculate text? Is it not notorious that good MSS. were hardly procurable and that erroneous texts were the rule in ancient times?

in a minute point; it is a very different thing to identify two large countries that are quite distinct from one another; and, if Eusebius does this in the case of a country (to use Dr. Smith's expression) which once even included his own city Paneas, what value remains for his evidence in other cases? But Dr. Smith knows infinitely more on that point than I do; his proof will be given in his eagerly expected *Geography*. He is not likely to make the common error of demanding from a fourth century author the kind of evidence we expect from one of the nineteenth; demanding in him the accuracy which we are now-a-days so apt to require from every one except ourselves.

W. M. RAMSAY.

THE PREMIER IDEAS OF JESUS.

IV. THE CULTURE OF THE CROSS.

It has been said, with a superb negligence of Judaism, that Jesus discovered the individual; it would be nearer the truth to affirm that Jesus cultivated the individual. Hebrew religion had endowed each man with the right to say I, by inspiring every man with the faith to say God, and Jesus raised individuality to its highest power by a regulated process of sanctification. Nothing is more characteristic of Jesus' method than His indifference to the many—His devotion to the single soul. His attitude to the public, and His attitude to a private person were a contrast and a contradiction. If His work was likely to cause a sensation, Jesus charged His disciples to let no man know it (St. Matt. ix. 30): if the people got wind of Him He fled to solitary places (St. John vi. 3): if they found Him, as soon as might be, He escaped (John vi. 15). But He used to take young men home with Him, who wished to ask questions (St. John i. 39): He would spend all night with a perplexed scholar