

the historical sense is much stronger. But to Barnabas Judaism is blank failure from the very beginning, when Moses broke the Tables of the Law in despair. Every ceremonial ordinance of Judaism was but the witness of a spiritual precept: it had no value, even temporarily, in itself. This is the extreme to which no New Testament writer proceeds. Nor was Barnabas followed in this respect.

The immediate purpose of our survey of the Epistle of Barnabas will have been attained if we have made it reasonably certain that the description of the Two Ways is an integral part of the document, conceived in the same spirit as the rest, marked by the same clumsiness of construction, drawing upon the same literary sources, and repeating again and again phrases which the writer has previously employed. There is no reason *a priori* for imagining that this section of the Epistle is borrowed from an earlier author: on the contrary, all the internal evidence goes to shew that the Two Ways, which plays so great a part in later Christian literature, is the original composition of the writer whom we call Barnabas.

*The chapter on the Didache will follow in the next number.*

## THE EVIDENCE OF ASTRONOMY AND TECHNICAL CHRONOLOGY FOR THE DATE OF THE CRUCIFIXION<sup>1</sup>

IN this paper I deal with questions connected with the regnal years of Tiberius, the Jewish calendar and the astronomical phenomena that governed it, and the eclipse mentioned in Luke xxiii 45, so far as they affect the determination of the date of the Crucifixion. I do not discuss questions connected with the accuracy of the different gospels or notes of time other than those given in terms of regnal years, days of the week, or Jewish festivals. For instance, the age of Jesus at the beginning of his ministry (Luke iii 23) and the forty-six years of John ii 20 do not concern me. There is little or nothing new in this paper, but the standard discussions seem always to have overlooked some part or other of the published material, and I hope, therefore, that it may be of service to review what is given us by the lines of evidence which I have mentioned.

In Luke iii 1, 2 we read 'Now, in the fifteenth year of the reign of Tiberius Caesar, Pontius Pilate being governor of Judaea, and Herod being tetrarch of Galilee, and his brother Philip tetrarch of Ituraea and

<sup>1</sup> A lecture delivered to the University of Oxford, December 4, 1930, and to the Society for the Promotion of Roman Studies, October 17, 1933.

of the region of Trachonitis, and Lysanias the tetrarch of Abilene, Annas and Caiaphas being the high priests, the word of God came unto John, the son of Zacharias, in the wilderness'. The names of the several rulers imply a date between the spring of A.D. 26 and the spring of A.D. 34. The one precise date, and there can be no doubt that it was intended to be a precise date, is the fifteenth year of Tiberius. The meaning of this date has been discussed by many scholars, including two eminent scholars of British nationality, Sir William Mitchell Ramsay and the late Professor Cuthbert Turner, from whose opinions I am going to express dissent.

Various honours and powers were conferred on Tiberius in the lifetime of Augustus, so that the question arises whether his regnal years are reckoned from some act of association with Augustus or from his final proclamation after the death of Augustus. There is nothing *a priori* impossible in either view. The question is to be determined by evidence. We must realize, to begin with, that in the eastern provinces of the Roman Empire the current date was regularly expressed by the regnal year of the emperor. St Luke would not have to convert into regnal years a date expressed in some other way, nor would the authority on whom he relies be likely to have done such a thing. We should expect the fifteenth year of Tiberius to have as definite a meaning as our year 1930, or, I might say, as our year 1730, to allow for the possibility of different styles with different New Year's Days. And, if it had such a meaning, it would be as independent of what, in the writer's opinion might, could, would, or should have been the way to reckon imperial years as the meaning of 1930 is independent of our opinions on the date of the birth of Christ. Dates in leases and contracts would depend on the adopted method of reckoning such years, and every business house would need to have a list, shewing the number of years that had in practice been assigned to each emperor. Have we, then, sufficient evidence to shew whether there was a uniform method of reckoning the years of Tiberius, and, if so, do we know what that method was?

Both these questions must be answered in the affirmative, or perhaps rather with such an approximation to an affirmative as the co-existence of different calendars permits. The earliest discussion which marshals the evidence at all completely is a doctoral dissertation by Kästner, published at Leipzig in 1890 under the title 'De Aeris quae ab imperio Caesaris Octaviani constituto initium duxerint'. It makes no reference to St Luke's gospel, and has been overlooked by most scholars, both classical and theological, though it was known to Schürer,<sup>1</sup> who recognized that it settled the question. To follow the evidence it is

<sup>1</sup> *Geschichte des jüdischen Volkes im Z.A. Jesu Christi*, 3 Aufl. i (1901), 414.

necessary to remember two dates. Augustus died in the year A.D. 14, on August 19. Tiberius did not accept the full powers which Augustus had enjoyed until after the apotheosis of Augustus, which took place on September 17 of that year.<sup>1</sup>

Now, as Kästner (pp. 10, 11) and many others point out, we have an Antiochene coin which bears the double dating of the first year of Tiberius and forty-fifth of the Actian era, and a Seleucian coin which bears the double dating of the third year of Tiberius and forty-seventh of the Actian era. These shew first of all that the first year of Tiberius, as reckoned at Antioch, lasted long enough after his proclamation for coins to be struck during it; they also give us the equation between years of Tiberius and years of the Actian era. Now Mr Tod has shewn in his paper on 'The Macedonian Era', in the *Annual of the British School at Athens*, vol. xxiii (1918-19), p. 212, that the Actian era was generally reckoned in each city from the beginning of the year that was current at the time of the battle of Actium, 31 B.C., September 2, though at Philadelphia it was reckoned from the birthday of Augustus immediately following the victory, i.e. from 31 B.C., September 23, which was also the local New Year's day. Perhaps it was selected as the date of the autumn equinox. From this it follows that the Actian era might begin anywhere from 32 B.C. September to 31 B.C. September according to the position of the New Year in the local calendar. We have, however, definite evidence to shew when it began at Antioch. For this purpose Kästner, pp. 7, 8, uses a succession of Antiochene coins, which give not merely the year of the Actian era, but the number of times Augustus had been created consul. In this way he is able to shew how these years compare with the consular years, and demonstrates conclusively that the Actian era at Antioch began in 31 B.C. on September 1 or October 1.<sup>2</sup> He also produces evidence to prove that September, not October, was the first month of the Antiochene year. The evidence is late and not conclusive. It would seem to establish that the indictions began on September 1, and the years of the Seleucid era on October 1, so that two New Year's days were current simultaneously after the introduction of reckoning by indictions. Now, if the years of the Actian era began on October 1, the numbering of those years at Antioch must have been an exception to the rule discovered by Mr Tod. But, if they were reckoned from September 1, the first year of that era would actually include 31 B.C., September 2. It seems probable, therefore, that the September New Year was not first introduced when reckoning by indic-

<sup>1</sup> *Fasti Amiterni*, *CIL* i<sup>2</sup> p. 244.

<sup>2</sup> von Domaszewski's argument, *Abhandlungen zur römischen Religion* (1909), 205-209, that the Antiochene year began on November 18 is too far-fetched to be considered seriously.

tions came into vogue, but goes back to the original adoption of the Julian calendar at Antioch under Augustus. And, as it is probable that the regnal years and the years of the Actian era would in each city be reckoned from the same New Year's day, we have the first year of Tiberius at Antioch equated with a year which ran from A.D. 14 September 1 to A.D. 15 August 31, and the third year with one which ran from A.D. 16 September 1 to A.D. 17 August 31. In Tacitus' *Annals* iv 1 the year A.D. 23 is described as the ninth of Tiberius. This would imply that A.D. 15 was the first, but the regnal number is not introduced as the name of the year, and cannot be pressed as evidence either of an official or of a popular enumeration. There is also an inscription from the neighbourhood of Marseilles, *CIL* xii 406, which is known to belong to A.D. 19, and which is dated in the fifth year of Tiberius. This would imply that the corresponding date in A.D. 15 fell in the first year of Tiberius, but does not tell us on what exact day the first year was supposed to begin. Then there is an inscription belonging to Cyprus, cited by Kästner,<sup>1</sup> which is clearly dated with the year of tribunician power as well as the month and day of the Cypriot calendar, as belonging to Tiberius's birthday, A.D. 29, November 16, and is dated in the sixteenth year of Tiberius. This would seem to imply that the years of Tiberius are here numbered from the Cypriot New Year A.D. 14, September 23.

Kästner's material is given independently with additions in Dieckmann's article, *Die effektive Mitregentschaft des Tiberius*.<sup>2</sup> Like Kästner, Dieckmann, in this article, makes no reference to St Luke. Dieckmann goes more into detail than Kästner in the matter of Palestinian and Egyptian evidence.

The Palestinian coins are conveniently summarized by P. Thomsen, *Kompendium der palästinischen Altertumskunde* (1913), 97, but some of the data there given require important modifications in the light of Sir George Hill's studies, published in *Catalogue of Greek Coins, Palestine* (1914), ci. Down to the close of the procuratorship of M. Ambibuchus, A.D. 12, the coins of the Roman procurators of Judaea bear the title *Καίσαρος*. The years are numbered from the accession of Augustus, which coincides with the Actian era. No coins of the procurator Annius Rufus appear to exist. At all events, there are none between the forty-first year of Augustus and the second of Tiberius. A coin which was formerly attributed to the first year of Julia has been recognized by Sir George Hill as a restruck coin of Alexander Jannaeus. Valerius Gratus and Pontius Pilate struck coins bearing the name of Julia or of

<sup>1</sup> Now published by W. Dittenberger, *Orientalis Graeci Inscriptiones Selectae* ii (1905), 583. See Dittenberger's notes.

<sup>2</sup> *Klio*, xv (1918), pp. 339-375.

Tiberius or the two names together. This coinage extends from the second to the sixteenth year of Tiberius, and Julia's name is found in the second and sixteenth, among other years. Julia was the mother of Tiberius, who received the name of Julia Augusta by Augustus's will, so that any coins bearing her name must have been struck after the death of Augustus. In the absence of coins of the first year of Tiberius, these coins do not by themselves prove that the years of Tiberius were reckoned from his final succession. They would be consistent with a reckoning beginning one year earlier. I shall return to these coins presently.

Dieckmann shews both from coins and from papyri that the first year of Tiberius, as reckoned in Egypt, must have been the year ending in A.D. 15 on August 29. In fact, as Gardthausen has pointed out in *Berliner Philologische Wochenschrift*, 40 (1920), p. 618, we know that this first year contained both a Choiak 19 (December 15) and an Epiphi 6 (June 30). Kästner had wrongly supposed that it ran only from A.D. 14 August 19 to August 28 of the same year. We can, if we choose, fix the twenty-third and last year of Tiberius in Egypt by papyri illustrating the passage to the first year of Gaius, and by so doing we get the same identification for the years of Tiberius. All our evidence points to one conclusion, that the regnal years of Tiberius throughout the whole empire were reckoned from his succession to full imperial authority, not from his co-regency with Augustus. But the persistency with which a different view is held by certain scholars makes it necessary to examine their arguments in detail.

I shall begin with Sir William Ramsay,<sup>1</sup> who probably carries most weight in this country. His argument is that St Luke wrote in the reign of the emperor Titus, that the regnal years of Titus were reckoned from his association with his father, Vespasian, and that St Luke used the method of reckoning years with which he was familiar in preference to that which obtained at the time to which he referred. Now let us see what is implied in this argument. It seems to imply that St Luke had before him dates expressed by some other system than regnal years, perhaps by names of consuls, that in order to reduce these dates to regnal years he had to count the interval from an emperor's accession, and that he had before him a chronological table which exhibited, among other events, some act which he regarded, or which the author of the table regarded, as a definite association of Tiberius with Augustus, and that St Luke chose this as the epoch of the reign of Tiberius. Now it is extremely unlikely that St Luke had any such table before him. All his indications of time are related to local tetrarchs, the local governor, the high priests, and the years of Tiberius, i.e. to the era in

<sup>1</sup> *St Paul the Traveller and the Roman Citizen*, 3rd edition (1897), pp. 386, 387.

common use in the country. If he was converting the date from consular years, he would doubtless have named the consuls. And, if he wished to indicate the exact year, as he must have done, he certainly would not knowingly have used a phrase which, in the ordinary chronological lists, had a different meaning. I think we may safely reject Sir William Ramsay's explanation of the supposed co-regency era. Turner held that, if Sir William Ramsay was not right, St Luke must have given a wrong date in error.<sup>1</sup> That is a suggestion which it is outside my purpose to discuss.

But there exists a group of scholars who hold that, in addition to the more common reckoning of years from Tiberius's actual accession, there was also in use a reckoning from his co-regency, of which they believe they have documentary evidence. The evidence consists firstly of two Antiochene coins bearing the head of Tiberius with the legend  $\Sigma\epsilon\beta\alpha\sigma\tau\acute{o}\varsigma$   $\Sigma\epsilon\beta\alpha\sigma\tau\omicron\upsilon$  and the double dating in the one case of 1 and 43 and in the other of 1 and 44. Similar coins, to which I have already referred, are found with the dates 1 and 45 and 3 and 47. These coins were published by Havercamp<sup>2</sup> in 1734, and the copies were taken by him from Morel's collection. If all these coins are genuine, the years 43, 44, and 45 of the Actian era are all found coinciding with the year 1 of Tiberius, and Tiberius is found in the lifetime of Augustus with the title Augustus, not otherwise given him on coins before his succession. Eckhel<sup>3</sup> satisfied himself by enquiry that the first two of these coins did not exist and had never existed, but that Morel had made a mistake. They have, however, been defended by Zahn in his Commentary on St Luke (1913), p. 188, and by Hartl in a monograph on the hypothesis of a one-year ministry of Jesus, *Neutestamentliche Abhandlungen*, vii (1917), 67 ff. Gerhardt, in an article on the date of the Crucifixion,<sup>4</sup> goes the length of stating that Hartl had verified the existence of the first named of these coins from two well-preserved specimens in the coin-cabinet at St Florian. This is a mistake. What Hartl professes to have verified is the existence of the third named, which belonged to the first year of Tiberius's sole reign. Hartl tries to explain the connexion of the year *one* of Tiberius with three successive years of the Actian era by supposing that the years of Tiberius's co-regency were reckoned from A.D. 13 January 1, so that the first year would coincide partly with the Actian year 43 and partly with the Actian year 44. Then we have the first year of the actual reign beginning about the same time as the Actian year 45. This is the best explanation, if the coins which he refers to

<sup>1</sup> *Encyclopaedia Britannica*, 11th edition, iii (1910), 888.

<sup>2</sup> *Thesaurus Morellianus*, Tom. II, Fam. Junia, Tab. I, Lit. I.

<sup>3</sup> *Doctrina Numorum Veterum*, iii (1794), pp. 276-278.

<sup>4</sup> *Astronomische Nachrichten*, Band 240 (1930), 137-162.

the period of co-regency are real. But I note that Gerhardt places the first year of the co-regency one year earlier than Hartl does, making it equivalent to our year 12, and he thus identifies the fifteenth year of Tiberius with A.D. 26. I do not believe that the reality of these coins can be seriously defended, and, even if they were real, they provide no evidence that the era of the co-regency was continued after Tiberius's succession to sole rule. In fact, as coins of the same type issued after the death of Augustus shew the era of sole rule, the inference would be that the co-regency era, if it ever existed, was completely superseded when Tiberius was proclaimed sole emperor.

All our literary sources, early or late, reckon the years of Tiberius from his final accession. Gerhardt has attempted to find two exceptions. The one is a passage in the *Stromata* of Clement of Alexandria I, xxi 144, ed Stählin (1906), p. 89, where two schemes of Roman chronology which Clement had found in different authorities are given. In the first Tiberius is assigned 22 years, but in the second he is given 26 years 6 months 19 days. As he died in A.D. 37, March 19, this would make his reign begin on the Egyptian New Year's Day, August 29, in A.D. 10, if the months are Egyptian months of 30 days. This is exactly four years earlier than the date from which the reign is reckoned on Egyptian coins and papyri. There are several errors in this list, and it is easier to suppose that in one of the many imperial lists preserved to us a scribe has written 26 in error for 22 than that an otherwise unknown reckoning is preserved in this list in Clement. The second exception is still less satisfactory. Hippolytus in his *Commentary on Daniel iv* 23, ed. Bonwetsch (1897), p. 242, gives the date of the Crucifixion as follows: 'And he suffered in his 33rd year on the 8th day before the Kalends of April on a Friday, in the 18th year of Tiberius Caesar, in the consulship of Rufus and Rubellion.' As the consulship of Fufius and Rubellius, to which Hippolytus wishes to refer, was our A.D. 29, it would follow that if this was the eighteenth year of Tiberius, his first year must have been A.D. 12. But there can be no doubt that Hippolytus has merely added 3 to the date given by St Luke for the beginning of John's ministry, just as he has added 3 to the age attributed by St Luke to Our Lord at the beginning of his ministry. He has permitted himself to combine the eighteenth year of Tiberius obtained in this way with the consular date given by tradition. There is no reason to suppose that he had examined the consistency or otherwise of these two dates.<sup>1</sup>

I take it, then, that we may safely regard the theory of an era of the co-regency of Augustus and Tiberius as exploded.

<sup>1</sup> Turner, who adopted the date 29, rejected the evidence of Hippolytus for the 18th year, explaining it as I do here.

But, although it is established that the years of Tiberius were reckoned from the beginning of his sole reign, there remains a question as to the initial date of each regnal year. In the eastern provinces of the Roman empire the year in which a reign began was regularly regarded as the first year of that reign, and the year beginning at the next New Year's Day was regarded as the second. We have abundant evidence to prove this in the case of Egypt, and such evidence as we have elsewhere confirms it. But we have seen that alike in Syria, in Cyprus, and in Egypt, the whole year, lasting from the autumn of A.D. 14 to the autumn of A.D. 15, was reckoned as the first year of Tiberius. Now, if his reign was deemed to have begun on the death of Augustus in A.D. 14 August 19, a second year might have been expected to begin at the New Year's Day in the autumn of A.D. 14, and Cichorius has, in fact, in the *Zeitschrift für die neutestamentliche Wissenschaft* (1923), pp. 16-20, argued that the second year of Tiberius was reckoned from the Antiochene New Year's Day A.D. 14, October 1. He has clearly not studied the evidence. This would not only imply that the Antiochene reckoning of the years of Tiberius was consistently one year in advance of the reckoning in Cyprus and Egypt, but it would also imply that all coins which give the Actian era as well as the years of Tiberius, whether in his first or in his third year, were struck in the month of September, quick work in the case of his first year, since Augustus did not die till August 19.

It is now the accepted view that the regnal years of Tiberius were made to run from the date of Augustus's death, August 19, and from its anniversary in each year, irrespective of local New Year's Days, except in Egypt, where the evidence in favour of a year beginning on Thoth 1, or August 29, is unassailable. But such a departure from the usual practice is not required. We have seen that Tiberius did not accept the full powers exercised by Augustus till September 17 or later. By the time the new régime was proclaimed in the east, the principal New Year's Days would be past, August 29 in Egypt, September 1 in Syria, September 23 in Cyprus, and the first year of Tiberius would last till the New Year's Days of A.D. 15.

I have mentioned Egypt, Syria, and Cyprus, but not Palestine. Here comes in the significance of the Palestinian coins of the sixteenth year with the names of Tiberius Caesar and *Ιουλία Καίσαρος*, Julia the daughter of Caesar, the chronological value of which has not received sufficient attention. The sixteenth year of Tiberius at Antioch must have begun in A.D. 29, September 1. But the death of the Empress, Julia Augusta, is the first event noticed by Tacitus under the consulship of Fufius and Rubellius, our A.D. 29. It seems improbable that coins bearing her name continued to be issued after her death, and, in fact, a

new type is found on Palestinian coins after this date. On the other hand it might seem strange that the first event recorded by Tacitus for a year beginning in January should have fallen in August or later, so as to be still unknown when coins were struck at Caesarea in September or later. This argument, however, cannot be pressed, since there is nothing to connect the other events recorded by Tacitus under this year with the earlier part of it. Any difficulty that there may be is lessened, if we suppose that the procurators of Judaea had adopted a different New Year's Day from the legates of Syria, probably Nisan 1, so that an event happening early in the Roman year might also be in the new Palestinian year. The Books of Maccabees use both the Nisan and the Tishri new years.<sup>1</sup> In both books the months are numbered from Nisan, but, while in the Second Book of Maccabees the years of the Seleucid era begin in the autumn, the First Book appears to use years of that era beginning in Nisan when following a Palestinian authority and years of that era beginning in autumn when following a Syrian authority. The Mishnaic tract Rosh-hashana, stating the purposes for which the Nisan and Tishri New Years respectively were used, states that regnal years were reckoned from the beginning of Nisan,<sup>2</sup> but it is not likely that this is intended to refer specially to the practice of the Roman procurators of Judaea, who, of course, had other subjects besides Jews. I know of no other evidence bearing on the beginning of the officially reckoned regnal years in Palestine, but the evidence seems to support the view that in the Palestinian practice the number of the year changed in Nisan rather than in the autumn, so that the fifteenth year of Tiberius would begin in Nisan of A.D. 28, while at Antioch it did not begin till September 1 of that year.<sup>3</sup> In any case the fifteenth year of Tiberius must have been our A.D. 28-29. In the Elephantiné papyri of the fifth century B.C. and in the papyri of Ptolemaic<sup>4</sup> and imperial Egypt, where two

<sup>1</sup> See Bickermann, s.v. 'Makkabäerbücher' in Pauly-Wissowa, *Real-Encyclopädie*, xiv (1928), 781-784.

<sup>2</sup> *Der Babylonische Talmud*, tr. Goldschmidt, iii (1930), 531.

<sup>3</sup> Reichardt, who owned coins which appeared to be dated in the 8th and 9th years of Agrippa I, but of which there is no example in the public collections, explained these dates on the supposition that Agrippa's 2nd year began on Nisan 1 of A.D. 37, immediately after his accession, and that his 9th year began on Nisan 1 of A.D. 44, shortly before his death. The coins were discussed at some length by Madden, *Coins of the Jews*, *Numismata Orientalia*, ii (1881), 132. Turner, *Hastings's Dictionary of the Bible*, i (1898), 416, viewed this argument with favour. Most scholars reject or ignore the evidence of these coins. The argument is in any case precarious, and it would be more precarious to draw from it a deduction as to the practice of the Roman procurators.

<sup>4</sup> For the calendar under the Ptolemies, see Frank in *Archiv für Papyrusforschung*, xi (1933).

calendars with different New Year's Days are used simultaneously, a new regnal year is made to begin on the New Year's Day of the calendar in which the date is expressed. This means that the regnal year may at times differ by one according to the calendar used. In such a case, if the date is expressed in both calendars, the proper regnal year is used with each. There would, therefore, be nothing contrary to Roman practice in having differences which might amount to several months in the beginning of the regnal year in different provinces and even in different cities in the same province, for the Palestinian cities did not all observe the same calendar. We may compare the difference of styles which was common in Europe in the middle ages, and which survived in England till 1752. I believe the Annunciation style, by which the year begins on March 25, is still used in papal appointments to bishoprics.

If the fifteenth year of Tiberius was a correct date, then we can deduce from it a *terminus a quo* for the date of the Crucifixion. If that year began in the spring of 28, the Crucifixion can hardly have been earlier than the passover of 29, and most scholars would probably feel that the passover of 31 is the earliest consistent with that. But, as has been seen, Turner was prepared to abandon the fifteenth year of Tiberius sooner than the year 29, which was, according to the oldest tradition preserved to us, the year of the Crucifixion.

The question of the exact year of the Crucifixion opens up an entirely different kind of chronological problem. All the gospels are agreed that the Crucifixion was on a Friday. The three synoptic gospels imply that it was on the first day of unleavened bread, the fifteenth of the Jewish Nisan, while St John's gospel asserts in the most definite way that it was on the preceding day, the 14th of Nisan. Ecclesiastical tradition favours St John's date. The problem, then, is to discover in what years, if any, the 14th of Nisan fell either on a Thursday or on a Friday. The Jewish calendar has always been lunar, each month beginning in the neighbourhood of the new moon. Twelve such months would average 354 days 8 hours, approximately 11 days less than a solar year. But each month was kept approximately to a fixed place in the solar year by the intercalation of a thirteenth month when necessary. The problem, then, divides itself into two parts. We have to consider which astronomical month in a given year was the Jewish Nisan, and which day in that month was regarded as the 14th. Throughout the whole ancient world civil governments, which might or might not be also priestly, intercalated somewhat irregularly, using their own discretion from year to year. Astronomers preferred intercalation by cycle. In the modern Jewish calendar, which is commonly supposed to date from the fourth century A.D., intercalation is by cycle, but the whole of

our external evidence, which is mainly rabbinic, goes to shew that intercalation was regulated annually by the judgement of the Sanhedrin, not by a fixed cycle, in the time of Our Lord. This does not mean that the intercalation was absolutely arbitrary. There are four passages in Philo which connect either the month of Nisan or the feast of Passover with the spring equinox (*De Mose* ii 169; *De Decalogo* ii 206; *De Septenario* ii 293; and *Eae quae sunt in Exodo quaestiones solutionesque, in Philonis Judaei Paralipomena Armena*, ed. Aucher, Venice 1826, pp. 443-446). Josephus, *Antiquities* iii 10. 5, mentions the command to sacrifice the passover in the month Xanthicus or Nisan on the fourteenth day according to the moon, the sun being in Aries. Aristobulus is cited by Anatolius<sup>1</sup> for the rule that both sun and moon must at the feast of the passover be passing through the equinoctial  $\tau\mu\eta\mu\alpha$  or section (of the zodiac). This means that the sun must be near the spring equinox and the moon near the place of the autumnal equinox. If we suppose that the present Jewish calendar dates from about A.D. 370, we shall find that it was originally arranged so that the earliest possible date for Nisan 14 should be March 18,<sup>2</sup> two days before the spring equinox, which the authors of the calendar may have calculated for our March 18. In the Samaritan calendar, which is probably very ancient and is independent of Jewish developments, the earliest possible date for Nisan 14 is March 25, which in Caesar's calendar was the supposed date of the spring equinox. There can, I think, be no doubt that the theoretically correct date for Nisan 14 has been within the twenty-nine or thirty days following the spring equinox from a remote antiquity with perhaps a latitude of a few days on the earlier side of the equinox. Aries, mentioned by Josephus, was reckoned as beginning sometimes about 8° before the equinox and sometimes at the equinox itself, while the equinoctial  $\tau\mu\eta\mu\alpha$  of Aristobulus must mean the equinoctial sign, as there was necessarily a variation of about 30° in the solar longitude at the time of passover. The term is, therefore, subject to the same ambiguity as the name of the sign given by Josephus. I reject Eduard Schwartz's argument in favour of a later date,<sup>3</sup> based on supposed synchronisms with the Tyrian calendar and also Schoch's,<sup>4</sup> based not on direct evidence but on inferences from modern information about the ripening of barley. It should be observed that the offering of the firstfruits of barley, which took place on Nisan 16, did not need to contain more than one sheaf, which could be obtained from

<sup>1</sup> Eusebius *H. E.* vii 32 17.

<sup>2</sup> This is the date in A.D. 379, the earliest day in the particular 19 years' cycle.

<sup>3</sup> 'Christliche und jüdische Ostertafeln', *Abhandlungen d. königl. Gesellsch. der Wissenschaften, Göttingen, phil.-hist. Kl.*, N. F. viii, no. 6 (1905), 138-150.

<sup>4</sup> *Biblica* ix (1928) 54 note.

the part of the country where the barley ripened earliest, perhaps from the Jordan valley, and might be ready long before a whole field was ripe. Although the command in Leviticus xxiii 14 does not go further than to prohibit the eating of fresh ears or of bread or corn made from them till after the offering of the firstfruits, Josephus, as Mr Herbert Owen has pointed out to me, states in the passage cited above, that the Hebrews were not permitted to reap for themselves till after the offering of the firstfruits on Nisan 16. This would be a strong incentive to celebrate the passover at the earliest date at which the sheaf of barley could be obtained.

But we are not entitled to suppose that Nisan was always officially fixed by the strict astronomical rule just mentioned. The actual practice is probably well represented by the Talmudic tract *Synhedrin*, 10<sup>b</sup>-13<sup>b</sup>, according to which the Sanhedrin, when considering whether to intercalate or not, might have regard to the state of the roads, the bridges, and the passover-ovens, to the possibilities of pilgrims who had already started arriving in time for the passover, to the growth of the kids, lambs, and pigeons, of the corn and of the fruit, and to the number of days that had to elapse before the equinox. According to some rabbis intercalation was to be avoided in a year of famine or in a sabbatical year, and a court might be influenced by the fact that the next year would be, or the last had been, a sabbatical year. We must, therefore, allow some margin of uncertainty in selecting the month which we are to regard as having been Nisan in a particular year.

Although intercalation was influenced by other than astronomical considerations, our evidence seems to shew that the beginning of the month was fixed rigorously by astronomical observation. The rule was the same as that which obtained at Babylon, and is still used to govern the date of religious festivals among the Mohammedans. If the lunar crescent could be seen at the close of the twenty-ninth day of the month, a new month was made to begin that evening. If the crescent was not seen that evening, the month was given a thirtieth day, and a new month was made to begin the following evening.<sup>1</sup> It is true that in the third century A.D. certain rabbis held that the Adar followed by Nisan or even both Adars in an intercalary year should be limited to twenty-nine days, but this rule is unknown to the Mishna and was disputed in the third and fourth centuries A.D. The inconvenience that it tried to obviate, uncertainty among the diaspora as to the date of the passover, could also be obviated by doubling the festival, and the practice of doing so appears to be at least as old as the doctrine that Adar ought to contain

<sup>1</sup> See the Mishnaic tract *Rosh-hashana*, and the *Πέτρον κήρυγμα* cited by Clement of Alexandria, *Stromata* VI v 41.

twenty-nine days only.<sup>1</sup> There is really nothing in favour of Turner's view that such a rule existed in the time of Christ except that it is necessary to assume it, if the Crucifixion is to be dated in A.D. 29.

So we come to the question whether we can compute the first visibility of the moon astronomically and, if so, how. Literary and historical students do not always realize that the conditions of visibility of heavenly bodies to the naked eye which play an important part in ancient astronomy have no interest for modern astronomy, and that modern astronomers can give little information concerning them. No attempt was made to investigate scientifically the conditions of visibility of the moon before my paper on the subject in the *Monthly Notices of the Royal Astronomical Society*,<sup>2</sup> published in 1910. In that paper I found a simple formula dependent on the altitude of the moon at sunset and its difference in azimuth from the sun at the same moment, which satisfied a series of seventy observations of the appearance or non-appearance of the young moon, made by Julius Schmidt at Athens. Since then Schoch has applied the same method to about 400 Babylonian and some modern observations. He found a formula differing from mine only in the numerical values to be used, the moon being more easily visible at Babylon than at Athens if the relative positions of sun, moon, and horizon were the same at both places. I applied my formula to the determination of the date of the Crucifixion in a paper published in the *JOURNAL* in 1910. The difference between my formula and Schoch's is unimportant. Where they differ, Schoch's formula is to be preferred. But the only conceivable year for which it gives a different result from mine is A.D. 27, if so early a date be conceivable.

I assume as a preliminary hypothesis that the 14th of Nisan is the first 14th day of a lunar month to fall after the spring equinox. Then in the year 27 the 14th of Nisan would fall on Thursday, April 10, according to Schoch's formula, or on Friday, April 11, according to mine. So this year would suit the synoptic gospels in that it would place the Crucifixion on Nisan 15. In A.D. 28 Nisan 14 would fall on Tuesday March 30. If we suppose that Nisan fell a month late that year and that the appearance of the moon was delayed one day by cloudy weather, Nisan 14 in A.D. 28 would be Thursday April 29, which would suit the synoptic gospels, always supposing that 28 is on other grounds a possible year. I may add that it is not very likely that the moon would be obscured by clouds on April 14, equivalent to April 12 Gregorian, but such a possibility must not be dismissed.

The year favoured by Turner, A.D. 29, cannot be forced into agree-

<sup>1</sup> See the Babylonian Talmud, tract Rosh-hashana, and my discussion in *J.T.S.* xii (1910) 124-126.

<sup>2</sup> Vol. lxx, pp. 527-531.

ment with astronomy. Nisan 14 in that year ought to have been Monday April 18. If we throw it back a month, it would fall on Saturday March 19. To bring it back to Turner's date, Friday March 18, we have to assume not only what is not difficult, that the passover was in that year allowed to fall before the equinox, but also that the Sanhedrin was deceived by a witness who falsely believed, or at least falsely alleged, that he had seen the crescent on an evening when it was really invisible. I cannot say that this was absolutely impossible. But it is a rather desperate hypothesis. In A.D. 30, the 14th of Nisan should have fallen on Friday, April 7. And if 30 be not ruled out on other grounds, this is one of two dates which will satisfy St John's gospel. Gerhardt has attempted to shew that the moon may have been seen one day earlier than Schoch and I suppose, so that this year would satisfy the requirement of the synoptic gospels that the Crucifixion was on Friday Nisan 15. In a computation made for Gerhardt, Professor Neugebauer, of the Astronomisches Recheninstitut, Berlin, found for the true altitude of the moon at true sunset at Jerusalem in A.D. 30, March 23,  $9.37^\circ$ , and for the difference in azimuth of the two bodies,  $5.46^\circ$ , thus confirming computations made by Schoch and myself. According to Schoch's latest formula, for this difference of azimuth an altitude of  $9.9^\circ$  would be necessary in order to render the moon visible. The values which he obtained were regarded by him as minimum values. In view of the large number of instances on which they are based, any further reduction seems inadmissible. Professor Neugebauer advised Gerhardt that in favourable circumstances the moon might be seen half a degree lower than Schoch's revised minimum. This is true, but Professor Neugebauer has said nothing to shew how extremely rare such favourable circumstances are. In all the naked-eye observations that have come to my notice or Schoch's there was only one evening when the moon was seen nearer to the sun than Schoch's formula permits, in fact a good deal nearer than Professor Gerhardt requires. That was in 1916, May 2, when two maids looking for Zeppelins at Scarborough, and two ladies looking for Zeppelins at Heighington, succeeded in observing the lunar crescent.<sup>1</sup> It is well known to astronomers that there are rare occasions of abnormal atmospheric transparency. Apart from these Schoch's formula will not admit of so large an exception.

In the year 31 Nisan 14 should have fallen on Tuesday March 27. We can shift it to a Thursday by supposing that Nisan fell a month late and that the appearance of the moon was delayed one day by cloudy weather. Schoch regards such a delay as impossible at that season of

<sup>1</sup> A selection of observations which had been regarded as remarkable was discussed by me in *The Observatory*, xliv (1921), 308-311.

the year. Eclipse observers know that you can never count on an absence of clouds. If any one wants to find a year that will suit the synoptists' date, I should certainly advise him to place Nisan one month late and the appearance of its crescent one day late in A.D. 31 rather than with Gerhardt to place the appearance of the crescent one day early in 30.

In 32 Nisan 14 should have fallen on Sunday April 13, or Monday April 14. It is absolutely impossible to shift this to a Thursday or Friday.

In the year 33 Nisan 14 ought to have fallen on Friday April 3. It cannot be shifted to a Thursday.

In the year 34 Nisan 14 ought to have fallen on Tuesday March 23, or on Wednesday March 24. If we suppose that Nisan fell a month after its normal time, we should have Thursday April 22, and Nisan 15 would be a Friday, as the synoptic gospels imply.

From this analysis it follows that from A.D. 27 to A.D. 34, 27 is the only year which will satisfy the implication of the synoptic gospels that in the year of the Crucifixion Nisan 14 fell on a Thursday, unless we assume that the first appearance of the crescent took place abnormally early or abnormally late or that Nisan itself fell a month after its normal season. Few scholars would regard 27 as a possible date or even 28 or 34. For 30 we have to suppose an extremely early visibility of the crescent; for 31 both a late Nisan and a late visibility. 29, 32, and 33 fall out altogether on this hypothesis.

If, on the other hand, we hold with St John that the Crucifixion was on Friday Nisan 14, we have a choice between A.D. 30 and A.D. 33, without assuming anything abnormal about the arrangement of the calendar or the visibility of the moon. This study tends, therefore, to support the Johannine date. If the fifteenth year of Tiberius is correct for the beginning of St John the Baptist's ministry, A.D. 30 seems rather earlier than we should have expected, A.D. 33 rather later.

There is one more astronomical phenomenon to examine. Matthew, Mark, and Luke record a darkness over the whole land from the sixth hour to the ninth hour, and the best authorities for the text of St Luke add the explanation *τοῦ ἡλίου ἐκλείποντος*, 'the sun being eclipsed'. *ἐκλείπειν* is a technical term, and when used of the sun or moon always means 'to be eclipsed'. Now the darkness was certainly not caused by an eclipse of the sun, for on the fourteenth or fifteenth day of the lunar month the moon is at or near the full, and solar eclipses can occur only at new moon. It is open to us to suppose that this was a ridiculous unscientific explanation, but it is also open to us to suppose that it is the result of a confusion. At two of the passover seasons discussed in this paper there was a partial eclipse of the moon. The one was on

the evening of Wednesday April 25 in the year 31, two days before the Crucifixion, if we assume, as we must to make that year possible, that Nisan fell a month and a day late, and that the Crucifixion was on Nisan 15. This seems a highly improbable hypothesis. The other eclipse of the moon was on the evening of Friday April 3 in the year 33, i.e. on the very evening of the Crucifixion if we accept that year which has been found to agree with the Johannine date. This eclipse was first computed and brought into the question by Sethus Calvisius, but it is only within very recent years that the relative accelerations of the sun and moon have been determined with sufficient accuracy to enable us to state definitely whether the eclipse was visible at Jerusalem or not. In these investigations Schoch and I have had the largest share. Our results are practically identical. Using Schoch's tables I find that the moon rose that evening at 6<sup>h</sup> 18<sup>m</sup> Jerusalem mean time, and that the eclipse ended at 6<sup>h</sup> 37<sup>m</sup>, so that a small eclipse would be visible the last nineteen minutes. No one who saw the eclipse of the moon can have mistaken it for an eclipse of the sun. But we must remember that in addition to St Mark's gospel or to a source common to the three synoptic gospels, St Luke had a good source for the Crucifixion, from which he gets his very Christlike sayings from the Cröss. It is not unlikely that St Luke's separate source told him of the eclipse, but if his mind was running on the darkness earlier in the afternoon, recorded by St Matthew and St Mark, it was not unnatural that he should have misunderstood his informant and taken the eclipse for a solar eclipse, which would explain the darkness. I make the suggestion for what it may be worth.

On the whole I consider that the date A.D. 33 April 3 offers fewer difficulties than any of the others, but my ambition has been rather to explain the character and tendencies of the different lines of evidence than to arrive at a conclusion, and I believe, as I certainly hope, that my opinion has in no part of the discussion been biased by the desire to support any particular conclusion.

For convenience of reference I reprint Schoch's table, shewing under column *h* the minimum altitude of the moon at sunset corresponding to different values of the difference in azimuth between sun and moon at sunset shewn in column  $\Delta$  for the lunar crescent to be visible in the evening. In using this table it is necessary to disregard parallax and refraction, both in finding the time of sunset and in finding the position of the moon in relation to the horizon. An explanation of the reason why the conditions of visibility of the moon can be represented by such a table is given by me in *The Venus Tablets of Ammizaduga* (1928), 45-48. The figures appearing here are revised figures, which were circulated by Schoch in a leaflet and reprinted after his death in *Astrono-*

*mische Abhandlungen, Ergänzungshefte zu den astronomischen Nachrichten*, Band 8, Nr. 2 (1930), B 17. The tables for computing the appearance of the moon, printed in *The Venus Tablets of Anmizaduga*, were, as a fact, computed with these revised figures, not with the figures given in the table on p. 95 of that work.

$\Delta$	$h$	$\Delta$	$h$	$\Delta$	$h$
0°	10.4°	8°	9.5°	15°	8.0°
1	10.4	9	9.4	16	7.7
2	10.3	10	9.3	17	7.4
3	10.2	11	9.1	18	7.0
4	10.1	12	8.9	19	6.6
5	10.0	13	8.6	20	6.2
6	9.8	14	8.3	21	5.7
7	9.7				

I also extract from my article 'Astronomical Evidence for the Date of the Crucifixion', in this JOURNAL, (1910, xii 122), the moon's altitude at sunset, and the difference of azimuth between sun and moon at sunset on the last evening when the moon could not be seen, and on the first evening when it could be seen at the beginning of Nisan of each year from A.D. 28 to A.D. 34 according to this table, with the resultant date of Nisan 14.

For A.D. 27 the figures are given only for the evening of first visibility. Alternative dates are given for A.D. 29. In this computation, as in Schoch's table, parallax and refraction have been disregarded.

A.D.	Day.	Moon's Altitude at Sunset.	Difference of Azimuth at Sunset.	Day.	Moon's Altitude at Sunset.	Difference of Azimuth at Sunset.	Date of Nisan 14.
27				Mar. 27	10.7°	6.6°	F., Apr. 10.
28	Mar. 15	6.3°	5.9°	Mar. 16	17.6	8.0	Tu., Mar. 30
29	Mar. 4	5.6	6.0	Mar. 5	15.9	8.4	Sa., Mar. 19
29	Apr. 3	9.4	5.9	Apr. 4	20.4	7.3	M., Apr. 18
30	Mar. 23	9.3	5.4	Mar. 24	20.3	6.5	F., Apr. 7
31	Mar. 12	8.0	4.9	Mar. 13	19.9	6.0	Tu., Mar. 27
32	Mar. 30	10.2	3.0	Mar. 31	25.6	3.1	{ Su., Apr. 13 or M., Apr. 14
33	Mar. 19	2.8	0.9	Mar. 20	16.8	1.5	{ F., Apr. 3 Tu., Mar. 23
34	Mar. 9	10.1	4.5	Mar. 10	21.3	1.2	{ or W., Mar. 24

The date of the spring equinox, reckoning the day from Jerusalem mean midnight, was March 23 in 27, 30, and 31, and March 22 in the other years.

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