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It is about six years since my first volume of *Studies in the Book of Daniel* was published; and now that the second volume is about to appear, it seems like an opportune time to reply to some of the criticisms of the reviewers of the first. The most elaborate of the criticisms was one by Professor Kemper Fullerton of the Oberlin Graduate School of Theology which appeared in the Bulletin of my alma mater, the Western Theological Seminary, at Allegheny, Pa., for October 1918. Had Professor Fullerton's criticism been confined to myself, I would have forever held my peace; but inasmuch as I am merely the occasion for a violent onslaught on the veracity of the book of Daniel, I shall be excused for referring at length to his strictures. It is high praise, though intended as blame, to be spoken of by him as a "bringer-up-to-date" of the work of such men as Hengstenberg, Keil, Pusey and Green, with whom I am classed as a belated example of the same anti-critical animus and unscientific method.

And, first, let me say, that I do not deem it a reproach to have produced "a typical example of the harmonistic method," which is one of the most serious charges brought against me by Professor Fullerton; but that I fail to see that I have used this method as distinct from the "historical" method, whatever that means. Words break no bones, it may be; but since Professor Fullerton evidently aims to put me in the wrong at the very start and, by necessary implication, all through my studies, by affirming that in my method I am harmonistic as distinct from historical, it is well to pause just here in order to come to an understanding

as to what he means by the insinuation, or implication of this statement.

First, then, what is the "harmonistic" method? As applied to history in general, I understand it to be a method that assumes a certain theory of the development of man in historic times to be true and then attempts to interpret all the evidence found in documents of whatever description in harmony with the theory. The theory may be true or false; but the method is the same. Now, there are two great and diametrically opposite theories of the history of man. The first is the Christian theory, commonly called the philosophy of the plan of salvation, or the history of the kingdom of God on earth. It is based on the belief that in the beginning God created the heavens and the earth, and made man in His image, and that, when man fell through disobedience to His divine will, God revealed a plan of redemption which he gradually unfolded and which culminated in the Cross of Calvary, and will be consummated when the kingdoms of this world shall become the kingdom of our Lord and of His Christ. In accordance with this theory, Paul says that it was in the fulness of time that God sent forth His Son and that of Him and through Him and unto Him are all things; and John, that all things were made by Him and without Him was not anything made that was made. In accordance with this theory we believe that God hath fore-ordained whatsoever comes to pass and that all things work together for good to them that love Him. This theory as applied to the kingdoms of the world was announced by Daniel and the Apocalypse, pursued by Eusebius and Augustine, and followed in principle by Orosius, Mosheim, Neander and Hegel. We Christians who hold this theory are bound to attempt to harmonize it with the facts of human history.

Since the rise of the modern evolutionary theory with its struggle for existence and survival of the fittest, it has become extremely popular to attempt to write and explain the history of humanity without regard to spiritual forces

and, to the exclusion of revelation or any divine intervention, simply as a process of growth inherent in and arising from the nature of humanity. Some, like Buckle, would divide the forces that have influenced and caused the growth of humanity into the physical, the moral, and the intellectual, making the last of these the dominant force of all. Others, following Marx, find "the causes of the process of growth" in the "economic conditions of existence." Some, like H. G. Wells, would find the causes of any upward growth of the human race in education and natural religion, "a growth to a State which will be the true God of all men, displacing the Old Man of the primitive savage and the national gods which have followed him into limbo." In this world state "men's thoughts and notions will be turned by education, example and the circle of ideas about them, from the obsession of self to the cheerful service of human knowledge, human power and humanity." It will be seen that all holding this theory ignore absolutely any divine purpose or intervention in human history. Sin is never mentioned by any of these writers; but the faults and frailties that characterize the life of man are to be remedied by education or better economic conditions. Physical life is to be prolonged and made more endurable by means of better food and hygiene,—to be followed by inevitable death without any comforting assurance of a life beyond. Without God and without hope in this world, the blind adherents of this theory would lead their deluded followers to disbelieve in Him who is the light of the world, and who brought life and immortality to light in His gospel.

As it is the duty of Christians to harmonize the facts of human history with the revelation of God's word; so it is incumbent on the holders of the evolutionary hypothesis to harmonize the same facts with their godless, hopeless scheme of things. Simply as a philosophy of the universe, I prefer the Christian theory, for it has in its God a cause adequate to account for all things that exist, in the glorifying of that God a purpose and end great enough to justify

all things that have occurred, and in the life and death of Jesus, the Son of God, the means sufficient to accomplish the purpose and attain the end designed; whereas, the evolutionists have no adequate cause, no demonstrable purpose or end, and no means capable of a rational interpretation. But, however this may be, let me reiterate, that whatever theory of the universe and of the history of man one holds, he must endeavor to harmonize with it all the known facts concerning the universe and man; and he is in honor bound to defend his theory against all attacks based on alleged evidence. So far, therefore, from its being a reproach that I have used the "harmonistic" method, it is the only natural and honorable method open to me.

Professor Fullerton's charge that I have not used the "historical" method is a more serious one, and one to which I most emphatically plead "not guilty." In every statement I have made, in every scrap of evidence I have employed, I have striven strictly to conform to the laws of evidence concerning documents as used in our courts of law. The question argued in the first volume is this: Are the historical statements of the Book of Daniel trustworthy? I claim that they are for aught we *know* to the contrary. The critics claim that they are not and bring forward a large number of objections in support of their contentions. These objections being fully stated in the very words of the objectors, I proceed to answer the objections *seriatim* by bringing in evidence to show that they do not hold. This evidence was procured according to the strictest rules of historical research. I resorted in every instance to the best grammars and dictionaries, in many cases to carefully prepared concordances, and in almost every case investigated carefully the sources in the best editions and in the original languages. All the contemporary sources were diligently searched in their most accredited editions for every detail of direct or indirect evidence bearing on the objection; and in addition the vast literatures and dictionaries of the Sum-

erians, Assyrio-Babylonians, Hebrews, Syrians and other Arameans; Egyptians, Greeks,¹ Romans and others.

Besides all this, I have never in the first volume found it necessary to bolster up my evidence by changing so much as a single letter of the text of any document. I have taken Daniel as it stands in the Massoretic Text; I have taken the extra-biblical sources as they appear in the best editions; and then I have attempted to interpret and test the evidence of these authorities in harmony with the usages and *usus loquendi* of the times in which they were written. Finally, I have abstained from making statements or expressing opinions or conjectures without giving in every instance the evidence on which they are based and citing this evidence by document, chapter and verse.

Now I call this method "historical." It is in accord with the laws of documentary evidence as set forth in Stephen's *Law of Evidence*, in Sir G. C. Lewis' work *On the Credibility of the Early Roman History*,² and with the principles of Historical Research as propounded by Prof. J. T. Shotwell³ and by Mr. George in his work, *Historical Evidence*. I prefer to call it the "evidential" method, because it proceeds on the ground of evidence, rather than opinion; or the "scientific" method, because it is based on the *known* rather than on the fanciful and conjectural.

This long, explanatory exordium was rendered necessary because by casting suspicion upon my method, Professor Fullerton has sought, by implication, to cast suspicion upon my evidence and conclusions. "The method" says he "is everywhere the same. I have therefore chosen his discussion of the problem of Darius the Mede, possibly the most fundamental historical problem of the work, in order to illustrate the method. If it is found convincing here, it

¹ Professor Fullerton is careful to note two items which I have not mentioned; the order of the kings in Daniel and the succession of kings as given in the Canon of Ptolemy. I shall refer to these later on.

² Vol. I, p. 16.

³ Article "History," in *Encyc. Brit.* 11th ed.

will be found convincing elsewhere; if it fails here, the case for the traditional view of Daniel is lost."

This is a most specious statement, one in which the prosecution attempts to shift the burden of proof from his own shoulders on to those of the defence. For what is the question at issue? It is whether the Book of Daniel is a veracious document, whether the events recorded in it actually occurred. No one can doubt that, on the face of it, it purports to give a true account of certain events in the life of Daniel and his three companions. The first chapter introduces us to the four young men who are the chief *dramatis personæ* by telling their origin, their names, their education and their station; the next five chapters recount certain remarkable events in their careers; and the last six contain the record of certain visions that appeared to Daniel and which he alone could relate. By the mention of dates and localities the whole background of the book is connected with the reigns of certain kings of whom two are well known kings of Babylon and Persia—Nebuchadnezzar and Cyrus the Great. When this book first emerges into general view in the second century B.C., it is already accepted by those best able to judge of its trustworthiness as a genuine and authentic document. It was so accepted by Christ and the Apostles, and has been so acknowledged ever since by the great body of Jewish and Christian scholars and believers.

But, in the last two hundred years there has arisen a school of critics which accepts no document as genuine and authentic unless it shall have passed successfully the tests supplied by the common rules of literary criticism. These rules may be stated in brief as follows: The language of the document must harmonize with the alleged date and provenance of the author; the literary forms must suit the time of the supposed composition of the document; and the historical data and background must conform to what is known from sources outside the document itself as to the times to which the subject-matter of the document

refers. We believe heartily in this kind of criticism as long as it is conducted fairly and thoroughly. Nothing more brilliant and convincing in the line of the criticism of documentary evidence can be imagined than Bentley's *Dissertation on the Epistles of Phalaris*, nothing more stimulating and instructive than Wolf's *Prolegomena to Homer* and Niebuhr's *Roman History*. Sir George Cornwall Lewis' *Inquiry into the Credibility of the Early Roman History* has been for years my constant mentor and with the general principles of his method I am completely in accord. And it is in harmony with these principles that I welcome all sincere attempts to test the credibility of the books of Holy Scripture and especially that of the Book of Daniel. If the Scriptures are false, the sooner we know it the better. Let us, therefore, test all things and hold fast to that which is good.

Now, of all the books of the Scriptures, the Book of Daniel is the one that affords the most facilities for investigation, for in matter of language, literature, ideas, customs, chronology, geography, and political history, we now possess a vast mass of evidence with which to test its age and provenance, its genuineness and veracity. During the last hundred years or more (Eichorn's *Introduction* was published in 1823), the critics have been assembling all the objections that they have imagined they could discover; and many have been convinced that the book is not historical, but that it was written in the second century B.C. In an article on "The Aramaic of Daniel," published in 1912,⁴ I endeavored to show that there is nothing in the Aramaic language of the book militating against its having been composed at Babylon in the sixth century B.C.; and in a third volume of *Studies*, I shall try to show further that this is equally true of the Hebrew portion. In the second volume the attempt has been made to show that the literary form and history of the Book of Daniel, together with the

⁴ *Biblical and Theological Studies* by the Members of the Faculty of Princeton Theological Seminary, pp. 261 ff.

ideas and customs that characterize it, are not incompatible with its having been composed at Babylon in the sixth century B.C. In the first volume I dealt especially with questions of geography, chronology, and of the personal history of Daniel, Nebuchadnezzar, Belshazzar and Darius the Mede, endeavoring to make it clear by indisputable evidence that, for all we know to the contrary, the persons mentioned in the Book of Daniel may have lived the lives, enacted the events, and had the experiences recorded of them. To-day, there is no evidence that will enable us to go further than this. Perhaps such evidence will never be discovered. But, one may be sure of this one thing, that those who assail the veracity of the Book of Daniel have not made out their case. They have not been able in a single instance to prove by evidence that any statement recorded in this book is wrong; and in view of this fact, it seems incredible that it can have been written four hundred years after the death of Nebuchadnezzar. The abundance of evidence that I have gathered, when taken altogether, points to Babylon in the sixth century B.C. as the most likely time and place of the composition of Daniel; whereas, it is impossible to see how a historical romance, or a fictitious history, could have been constructed in the second century B.C., composed partly in Hebrew, partly in Aramaic, yet both of a type clearly indicative of the time of Cyrus, and with its geographical and chronological statements so correct, its allusions to customs and ideas so detailed and accurate, and its references to persons and events so timely, appropriate and indisputable. It is more reasonable, more in accordance with all the known facts and evidence, to believe in the early date and the Babylonian provenance than in the Palestinian provenance in the second century B.C.

To this general conclusion, Professor Fullerton has only one objection left to make—Darius the Mede. How, he says, do you explain and account for Darius the Mede? To this question, I might content myself by responding,

How do you account for him?⁵ I have shown in Chapters ix-xiii of my first volume of *Studies*, that he cannot be a confusion with or a reflection of Darius Hystaspis, or of any other king or kings of Persia. How then can he be accounted for? As a creation of the imagination? Why, then, did the author mar the verisimilitude of his work by injecting this fictitious character into the midst of his tales centering about the well known kings of Babylon—Nebuchadnezzar, Belshazzar and Cyrus? Well known, I say, for whatever quibbles one may raise as to the special designation of Belshazzar as *king*, no one can deny that he was the first born son of Nabunaid, that he was commander of the Babylonian army in Akkad, and that he is represented in the inscriptions as possessed of royal prerogatives.

In Daniel v. 31 (vi. 1) Darius is said to have received the kingdom of Belshazzar the Chaldean, and in ix. 1 to have been made king over the realm of the Chaldeans. Visions are seen in his reign (ix. 1, xi. 1) just as in the reign of Belshazzar (vii. 1, viii. 1) and Cyrus (x. 1). In fact, on the face of it, the author treats him as a real king (Aramaic, *malka*) exactly in the same manner as he treats Nebuchadnezzar, Belshazzar, and Cyrus, as being real kings; but with this noteworthy exception, that Darius the

⁵ All that we know about Darius the Mede is contained in the Book of Daniel, v. 31-vi. 28, ix. 1, xi. 1. From these passages we learn that he was the son of Xerxes of the seed of the Medes and that when about 62 years of age he was made king of the Chaldeans, having received the kingdom of Belshazzar the Chaldean. He was king of the Chaldeans for at least one year (or part of two). He appointed under him 120 satraps over whom were three presidents of whom Daniel was chief. Like a sultan of Bassorah under the Califs of Bagdad, ruling according to the law of Islam, so he ruled according to the law of the Medes and Persians. After the episode of Daniel and the lion's den, he issued a decree to all the nations, peoples and tongues in all the land (or earth) acknowledging the God of Daniel as the living God. Daniel is said, moreover, to have prospered in the reign of Darius and in the reign of Cyrus the Persian. He is never called the king of the Medes, nor the king of the Persians, nor the king of Babylon; but only king of the Chaldeans. As successor of Belshazzar, whose first year as king of Babylon is mentioned, he may have been for a time and in some sense, king of Babylon also.

Mede alone is said to have *received* the kingdom and to have been *made king*.

On these two phrases "*received the kingdom* of Belshazzar the Chaldean" and "*was made king* over the realm of the Chaldeans," shiver in pieces all the attacks that have been made upon the trustworthiness of all the statements with regard to Darius the Mede that can be found in the Book of Daniel. Nebuchadnezzar inherited his kingship from his father; Cyrus acquired his; Darius received his. He was made a sub-king, just as Pharaoh-Necho made Eliakim a subject-king over Jerusalem and Nebuchadnezzar afterward made Mattaniah king instead of Jehoiachin.⁶ Pharaoh-Necho changed Eliakim's name to Jehoiakim, Nebuchadnezzar changed Mattaniah's name to Zedekiah. In like manner we have only to suppose that Cyrus made Darius king, his name being changed from some other name to Darius, and there will be complete harmony between the account in Daniel and that in the Nabunaid-Cyrus chronicle. A large number of the Persian kings had pre-regnal names which were changed when they became king. Thus, Cyrus himself is said to have been called Agradates; Artaxerxes I, Cyrus; Darius II, Nothus, and Artaxerxes III, Ochus; and the last Darius, Codomanus. As late as the fourth year of Cambyses, Gobyras was still the *pihatu* or governor of Babylon.⁷

Of course, I have always admitted that Gobryas is not called king in the *Babylonian* language, where *sharru* would be the word. My claim has been that in the pure Aramaic, where neither *sharru* nor *pihatu* is found, the word *malka* would be the best equivalent native title, just as *khshatrapava* "satrap" would have been in native Persian. It is admitted that the author of Daniel might have denoted the governor of Babylon by one of the foreign titles, (*sagan*, *paht* or *satrap*), all of which he uses elsewhere in his work; but this is not saying that these words are Aramaic, or that he could

⁶ 2 Kings xxiii. 34, xxiv. 17.

⁷ See Pinches in *Expository Times* for April 1915.

not have used the best Hebrew or Aramaic equivalents, *mèlèk* or *malka*, to denote the same official. Especially is this true when we consider that Gobryas was not an ordinary *pihatu*, but that he had been *pihatu* of Gutium before he conquered Babylon for Cyrus and that as the *pihatu* of Cyrus he appointed other *pihatu*s in Babylon,⁸ thus exercising the prerogatives of a king. Besides, in that time of transition, when Babylon had just passed from the rule of the Chaldeans to that of the Persians, it is probable that the natives would designate the successor of Belshazzar by the same title that Belshazzar himself had enjoyed. It must be remembered, also, that the question is not whether the author of Daniel may have called a sub-king of Babylon a *pihatu*, but whether he may have called a *pihatu* of Babylon a *mèlèk* or *malka*.

This word *pihatu* (פּחַתּוּ) is one which can afford small comfort to the radical critics of the Old Testament. In every instance, except possibly the case of its doubtful occurrence in the Aramaic inscription of Panammu and one place in Isaiah (xxxvi. 9), it is found (exclusive of those written in Assyrio-Babylonian) only in documents from the sixth, or fifth century B.C. In the Bible it is used in Hebrew passages 32 times,⁹ and in Aramaic passages 9 times.¹⁰ In the Sachau papyri it is found twice (i. 1 and ii. 29). In all of these cases, except 1 Kings x. 15 it refers directly to the governors of the Assyrians, Babylonians and Persians. It was used of these foreign potentates, just as we might use *Kaiser* of the ex-emperor of Germany or Austria, or *Stadholder* of a viceroy in a province of the Netherlands. If this word had become truly Hebrew, or Aramaic, how does it come that it is never used in that mass of Hebrew literature which the critics assign to the Persian and the

⁸ See Nabunaid-Cyrus Chronicle, Rev. 1. 20.

⁹ 1 Kings x. 15, xx. 24, 2 Kings xviii. 24, Is. xxxvi. 9, Jer. li. 23, 28, 57, Ezek. xxiii. 6, 12, 23, Hag. i. 1, 14, ii. 2, 21, Mal. i. 18, Ezra ii. 6, viii. 4, 36, x. 30, Neh. ii. 7, 9, iii. 7, 11, v. 14, 15, 18, vii. 11, x. 15, xii. 16, Es. iii. 12, viii. 9, ix. 3.

¹⁰ Ezra v. 3, 14, vi. 6, 7 bis, 13; Dan. iii. 2, 3, 27, vi. 8.

Greek periods? The word is never used in the Hebrew and Aramaic of the Talmud, in the Aramaic Targums, in Syriac, in Mandaean, in Palmyrene, or in Nabatean; so that there is absolutely no evidence, that as Professor Fullerton implies, it had ever been adopted as a *bona fide* Hebrew or Aramaic term.

The same may be said of *sagan* (סגן) "deputy." Outside of Is. xli. 25 (where it refers to the officials of Syria) it is used in the Hebrew of the Bible only 16 times,¹¹ and in the Aramaic, only 5 times¹² In Jeremiah and Ezekiel, it is always used of the Babylonian deputies; and Ezra and Nehemiah are Persian officials from Babylon who got this nomenclature from their overlords. In the Talmud it is used of the assistant High Priest, but never of state officials. It is never found in the Aramaic Targums or in Syriac, or in any other Aramaic document, except a number of times in the papyri of the fifth century B.C. Why does this word not occur in the literature assigned by the critics to the period from 500 to 100 B.C.?

With the Persian word *satrap* the case is even worse for the critics. It occurs in the Hebrew of the Bible only 4 times,¹³ and in the Aramaic portion of Daniel 9 times.¹⁴ It is spelled אַחַשְׁרָפָן in almost exact correspondence with the old Persian of the Behistun inscription of Darius Hystaspis which has *khshatrapava* (§38). It never occurs besides in Hebrew, or Aramaic, except in the Syriac *satrapa* or *satrapis*, a form derived through the Greek σατράπης. Why is this word found only in Esther and Daniel, where as a Persian official term we might expect to find it, but never in any of those many parts of the Old Testament which the critics arbitrarily assign to the Persian and the subsequent Greek period?

Some interesting parallels to the changes of title are found

¹¹ Jer. li. 23, 28, 57; Ezek. xxiii. 6, 12, 23, Ezra ix. 2; and nine times in Nehemiah.

¹² Dan. ii. 48, iii. 2, 3, 27, vi. 8.

¹³ Esth. iii. 12, viii. 9, ix. 3; Ezra viii. 36.

¹⁴ iii. 2, 3, 27, vi. 2, 3, 4, 5, 7, 8.

in the different recensions of the Behistun inscription. Thus the Persian *khshayatiya* "king," is always rendered in Susian by *sunkuk*; in Babylonian by *sharru*, and in Aramaic by *malka*. *Kshatrapava* (satrap) is rendered in Susian by *shakshabamaname*; in Babylonian by *pihatu*. Unfortunately, the Aramaic document for §§ 38 and 45, where the word for satrap occurs, is rubbed off, so that it is by conjecture only that we suppose that the manuscript had *pihatu*. Noteworthy is the Susian rendition of the Persian *mathishtam* in § 38, where it is translated by *sunkuk* (king). Ordinarily the Susian equivalent of *mathishta* is *irshairru*, which the Babylonian renders by the phrases *ina muhhi* as in § 47 or *ina kakkadu* as in § 50. In § 38 the Aramaic renders the word by [ם] בראשיהו "at their head," an exact equivalent of the Babylonian. It is to be noted that the Susian renders the phrase by *sunkuk* (king). Surely Professor Fullerton will not claim that Gobryas was not at the head of the Persian army that conquered Babylon, or that he was not 'at the head' of the Persian government established there by Cyrus.¹⁵

Professor Fullerton is a past master in all the arts of the radical Higher Critic. He is always on the lookout to find mistakes and inconsistencies in all the documents that he

¹⁵Some further evidence on the claim that a governor of the Persians might be called "king" or a king of a country "a governor" may be gathered from Aeschylus in comparison with Herodotus and Xenophon. Thus Syennesis is called by Herodotus (v. 118) the king of the Cilicians, and Xenophon (vii. iv. 2) says that the Cilicians always had native rulers (*οἱ ἐπιχώριοι βασιλεύοντες*); whereas Aeschylus in the *Persae* (i. 326-7) calls him the governor of the Cilicians (*ἄπαρχος*). Aeschylus speaks, also (i. 24) of Amistres and other leaders of the Persian army at Salamis as "kings, subalterns of the great king" (*βασιλῆς βασιλέως ὑποχοι μεγάλου*); and in i. 969 calls Sevalkes, one of the friends or companions of Xerxes, a king (*ἄναξ*).

Further, Onesicritus (325 B.C.) is quoted by Strabo (xv. 3) as saying that on the great tower at Pasargada, where Cyrus was buried, was the inscription: "Here lie I, Cyrus king of kings." This certainly implies that Cyrus had kings under him; and the other passage cited above shows that the generals and officials under the king of Persia might be designated as kings. See further in *Studies in the Book of Daniel*, vol. I, 92-95.

reads, like a sharp lawyer with an unwary witness. When I was defending Daniel against Dr. Driver's charge that the author must have meant *the whole empire of Persia* by the phrase "in all the earth," I collected a mass of evidence to show that the most usual and explicit Hebrew phrases for denoting a world-wide empire are "all lands," or "all the kingdoms of the lands," and that by "all the land" (כל-הארץ) in Dan. vi. 26, it is possible that the more limited sphere of the sway of Darius as sub-king was denoted. Professor Fullerton challenges me to produce a single instance where the phrase is "applied to such a diversified territory as Sargon's empire." This I gladly do. He will find it in Dan. iv. 1 (iii. 31 in MT). Dan. vi. 25 is identical with iv. 1, except that one is the greeting of Darius the Mede and the other of Nebuchadnezzar. The kingdom of Nebuchadnezzar was about the same size as that of Sargon. Professor Fullerton is here respectfully invited to show upon what ground he supposes that the realm of Darius the Mede (Gobryas) may not have been as large as that of Nebuchadnezzar. We know that Gobryas was *pihatu* of Gutium before he became *pihatu* of Babylon. These make two of the *satrapies* mentioned by Darius Hystaspis in the Behistun, and in the Persepolis E., inscriptions. May he not have ruled the satrapy of Arabia also, and must not one of these three satrapies have included Syria and Palestine which are not mentioned by Darius Hystaspis? May he not have been *pihatu* of Media also? or did the Gutium of the Nabunaid-Cyrus Chronicle include Media? Since Professor Fullerton is attacking Daniel, and not I, will he pardon me further for asking him to give some instances to show that the phrase used by Daniel was characteristic of the second century B.C.? Candidly, I prefer Daniel's *prima facie* evidence to Professor Fullerton's opinion.

This may be a good place to say, also, that if anyone prefers to translate ארץ by "earth" instead of "land," it would be equally permissible and in accordance with the Aramaic usages of the word ארעא. There is no reason why both

Nebuchadnezzar and Darius the Mede should not have made known their faith in the God of Heaven to the whole earth, outside as well as inside their respective realms. Mohammed summoned all the earth to accept Islam, and the Korean and other peoples are appealing for help to all the earth. May not and would not Nebuchadnezzar and Darius the Mede have magnified the good deeds of God beyond their own boundaries to earth's remotest "peoples, nations and tongues"?

Professor Fullerton says (1) that the rare use of the word *khshatrapava* (satrap) in the old Persian inscriptions makes its employment by Daniel very doubtful, and (2) taunts me with having resorted in support of its possible use by Daniel to the argument from silence, whose use by the critics I oppose so vigorously. He argues further (3) that, because on account of this rarity of occurrence I cannot tell its exact application, it cannot have been a familiar term to Daniel, writing so early; and (4) that because I can give "no instance of the occurrence of the word *satrap* in the Aramaic literature of the early times," it cannot have been used in the Aramaic of the sixth century B.C.

In these accusations there are the four specifications which I have numbered, not one of which is true, either in essence, or by implication. For, (1) it is not fair to say that the use of *khshatrapava* is *rare* in Old Persian in the sense that Professor Fullerton implies. It is the *only* word used *anywhere* to denote the idea of governor, or deputy. My readers must remember that there are only four hundred words all told in Old Persian. In all of the documents known, the word for governor occurs but twice—once in § 38 of the Behistun inscription and once in § 45. In § 38 the Babylonian recension is abraded; but in § 45 it renders the Persian word by *pihatu*. The Persian word for "satrap" does not occur again for more than 700 years when it appears in the Avesta in an abbreviated form *shoitrapan* without the initial *kh*. This *kh* is found, however, in the word as used in Daniel, so that the form in Daniel must

have been derived from the Persian of the Achaemenid period. Since the Behistun inscription was written about 515 B.C., we see that the use in the avowedly contemporary document of Daniel in the same form corroborates the genuineness of Daniel.

(2). What I have just said under number (1) will prove sufficiently that I have not resorted to the argument from silence to support my statement about "satrap."

(3) Since *khshatrāpava* is the only term known to have been used by the Medes and Persians of Daniel's time to denote "governor," it is hard to see how in an empire with many governors the term may not have been familiar to Daniel. The first thing Cyrus did after he conquered Babylon was to appoint a governor. So it was invariably in the case of the Assyrian kings. They no sooner subdued a city than they appointed a governor to rule it. In spite, however, of the scores of governors installed by the Assyrian kings, Klauber in his great work on the officials of the Assyrian Court¹⁶ declares that the functions of these officials cannot be exactly described. We know only that a *pihatu* was practically equivalent to a *shaknu*, and that each of them ruled over a larger or smaller region of the empire, exercising the authority of the king.¹⁷ Since § 45 of the Behistun inscription shows that the Persian *khshatrāpava* was the equivalent of the Babylonian *pihatu*, I fail to see how the inability to define the former militates against its existence any more than in the latter case. That is, if we cannot define the exact functions of the *pihatu* with scores of instances of its use before us, how can we be expected to define the functions of its equivalent Persian word with only two instances in Persian of its use?

(4). The fourth charge of Professor Fullerton is especially unfortunate for his argument. He ought to know that there is no early literature in Aramaic in which "satrap" could reasonably be expected to be found. From the 9th

¹⁶ *Assyrisches Beamtentum*, pp. 99-104.

¹⁷ *Ibid.*, p. 100.

century B.C. we have the Zakir inscription,¹⁸ and from the 8th century the Sendshirli inscriptions. No one would expect to find a Persian word in these.¹⁹ From the 6th century the only document which could possibly have had the word is the Aramaic recension of the Behistun inscription. We have seen that the Persian recension has *khshatrava* twice. Unfortunately, the Aramaic papyri are rubbed off where the translation of these words would be. In the Aramaic of the 5th century B.C. the word does not occur, but in the Hebrew of Esther and Ezra it is found four times, and in the Aramaic of Daniel, it occurs nine times, as we have seen. The only direct evidence I can produce in favor of the early use of the word is that the letters with which the Aramaic word is spelled in Daniel transliterate the Persian word more exactly than we have it in any later literature.

This all looks bad for Daniel.

But it is not as bad as it looks at first sight. For, the word does not occur in any Aramaic document from 500 B.C. to 200 A.D. It is not found in any Aramaic document from the 5th century, or the 4th or the 3rd, or the 2nd (when the critics say that Daniel was written), or from the 1st B.C. or A.D., or from the 2nd A.D. Nor is it found in the Talmud, nor in the Aramaic Targums, nor in the Egyptian Aramaic, the Nabatean, Palmyrene, Samaritan, or Mandaean. This looks like a *reductio ad absurdum*; for if this line of reasoning be valid, it is perfectly sure that Daniel never can have been written at all; at least with this word in it. But, it is of this word we are talking now. And there it is in Daniel—*nine times*; and in the Hebrew of Esther, treating of the times of Xerxes, three times, and once in Ezra. Three witnesses, even though only biblical, can scarcely be impugned by the mere opinion of a modern scholar—no, nor by the opinion of ten thousand of them.

¹⁸ Pogonon, *Inscriptions sémitiques de la Syrie*, II, pp. 156-178.

¹⁹ The Assyrian *pihatu* may be meant by the 𐎱𐎠 of the Panammu inscription, l. 10.

In Syriac we have the forms *saṭrapa* and *saṭrapis*, both derived from the Greek *σατράπης*. The earliest occurrence of the word is in the translation of 1 Esdras iii. 2 (not found in Hebrew), where it is translated by the Greek *σατράπαις*. This is probably not earlier than the year 200 A.D. It seems to be found nowhere else in either the canonical or apocryphal books of the Syrian version of the Old Testament. It is not found at all in those two great Syrian Classics *Addai the Apostle* and *Joshua the Stylite*, both of which uniformly employ Syriac, Greek and Roman words to denote governors, pro-consuls, etc.²⁰

Again I can understand how the author of Daniel and Esther can have employed a Persian word for governor in the 6th or 5th centuries B.C.; but I cannot see why the translators of these books into Syriac and the Aramaic dialects should not have simply transliterated the word *khshatrapava*, if it had been current in any form in their day. Yet we find the Peshitto (and the Targum of Esther iii. 12) rendering it by רבי הילא "army chiefs,"²¹

Moreover the Greek translation can afford no help to the critics.²² Apparently the Greek translators who lived

²⁰ Joshua once employs the Persian word *astabid* and once *marzeban* as designations of the generals of the Persians (see §§ 21 and 59). The earliest occurrences of the word in Syriac after 1 Esdras iii. 2, are in the Syriac translation of *Julian the Apostate*, in Epiphanius and in the works of Ephraem Syrus, none of them earlier than the 4th century A.D. It will thus be seen that the word occurs but once (i.e. in Ephraem Syrus) in Syriac original writings before the end of the 4th century A.D. How an author writing in the Aramaic dialect of Daniel or in the Hebrew of Esther could have derived a word from Syriac documents composed hundreds of years after his death, we leave to the critics to determine.

²¹ Except in Ezra viii. 35 where the Peshitto has רורכני "chiefs" and in Esther viii. 9, ix. 1, where the Targum has אסטרטילוכי, a word derived from the Greek *στρατηλάτης*. Unfortunately there is no Targum to David and Ezra.

²² The LXX transliterates the term in Dan. iii. 2, vi. 1, 2, 4, 7, but renders it by ἵπατοι in Dan. iii. 3, 27, by στρατηγοί in Esther iii. 12, by οἰκονόμοι in viii. 9, by τύραννοι in ix. 8, and by διοικηταί in Ezra viii. 35; and Theodotion transliterates in Dan. iii. 27, vi. 1, 2, 4, 7, and renders by τόπαρχοι in iii. 3 and by ἵπατοι in iii. 2.

shortly after the time when the critics say that Daniel was written, had no definite conception of the meaning of the term, since they used seven different words to render it.

My readers will please take note that here I am not making an argument from silence, but that I have the direct evidence of Daniel himself, of Esther and of the Behistun inscription, in favor of my claim that the word "satrap" (אחשדרפן) indicates the early date for Daniel. Also, I have all the indirect evidence on my side, to wit: all the Aramaic dialects, and all the ancient versions. Would it be shocking the proprieties to ask that the critics be good enough to produce some evidence in favor of their side of the case? For the days are come when the mere word or opinion of a critic, however high his position, and however scholarly his attainments, cannot and will not be accepted as evidence against the express statements of the Scriptures. The critics have raised the cry: Let us judge the scriptures as other documents are judged. So be it. Let us treat all documents alike as far as the laws of evidence are concerned.

And so with regard to the *argument from silence* with respect to the use of the word "satrap" in Daniel. Professor Fullerton charges that I have made use of the argument in order to show that the title "satrap" may have been applied to the governor of a more or less restricted territory. Now, I maintain that I might fairly make use of the argument from silence in reference to the connotation of this term "satrap" and that we might infer that the absence of all evidence to the contrary justifies us in holding that the Book of Daniel is right in its use of the term. The word itself presents evidence of its possible use which cannot be overthrown by the silence of other documents, or by the mere opinion of persons now living. Nevertheless, my argument has not been one from silence. For I have shown, first, that the Persian word means simply a "protector of a district of country" and that while a satrapy may have

been a large province,²³ it may also have contained no more than thirty men and women.²⁴ Secondly, I have shown that the Babylonian equivalent of satrap is *pihatu*;²⁵ and thirdly that Gubaru as *pihatu* (satrap) of Babylon, appointed other *pihatu*s (satraps) under him.²⁶ Fourthly, I have shown that the *pihatu*s are the same as the *shaknu*s²⁷ and that they and their equivalent the satraps, exercised all of the prerogatives of kings²⁸; and fifthly, that, as a matter of fact, many of the subordinate governors were really called kings by the Assyrians and Persians.²⁹ Sixthly, I have shown that there was no native Aramaic or Hebrew term to denote exactly the Persian satrap or the Assyrio-Babylonian *shaknu* or *pihatu*, and that consequently they sometimes take over the foreign words and sometimes use the best native equivalent *mèlèk* or *malka* to denote the idea involved in satrap.³⁰ The title "king of *shaknu*s or *pihatu*s" is never found in the Assyrian, Babylonian or Persian records; but always we read "king of kings" or "lord of kings."³¹ Ibn Hisham in his *Life of Muhammed*³² speaks of the letters that the prophet sent to the kings of Arabia and of the strangers. He refers to these kings by name as the *maliks* of Rome, Persia, Abyssinia, Alexandria (Egypt), Oman, Yemama, Bahrein, the boundaries of Syria, and Yemen. Wüstenfeld in his German translation calls the *maliks* of Rome and Persia *Könige* and those of the other countries *Fürste*; but the Arabic uses the same word for all. Yet we know that at the time when the letters of Muhammed were written, Alexandria at least was under

²³ *Studies*, p. 175.

²⁴ *id.*, p. 176.

²⁵ Behistun Inscription, § 45.

²⁶ Nabunaid-Cyrus Chronicle, Rev. Col. I. 20.

²⁷ See Klauber, l.c.

²⁸ *Studies*, pp. 203-208.

²⁹ *id.* pp. 203-206.

³⁰ *id.* pp. 83-95, 181.

³¹ See my article in the *Princeton Theological Review* for 1916, and in the *Sachau Denkschrift*, Berlin, 1915.

³² Wüstenfeld's Edition II. 971.

the sovereignty of Rome (Constantinople) and Bahrein, Oman and Yemen under that of Persia. Besides, Oman and Yemama are both said to have had "kings." All this confirms our contention that in the Semitic languages *mèlek*, *malka*, *malik*, etc. denote "chief," without defining the extent of the dominion ruled over or whether the chief was simply a king or a king of kings. The Arabic dictionary defines the term *malik* as denoting "one who attains the sultanate or rule in being set over a people, tribe or country." In Ethiopic, also, the word *negus* means not merely *rex*, but *princeps*, *dux*, *praefectus*, *tyrannus*, *satrapa*, and translates βασιλεὺς, ἡγούμενος, ἄρχων, τετράρχης, στρατηγός, τύραννος, τακτικός, χιλίαρχος ναύαρχος, and other like words.³³

To sum up, usage shows that the Hebrew *melek* and the Aramaic *malka* correspond at times to the Assyrio-Babylonian *sharru*, *malku*, *shakmu*, and *pihatu*, and to the Arabic *malik* and *sultan*; to the Persian *khshatriya* and *khshatrapava* and that all may be represented in Ethiopic by *negus*.

Perhaps, if Professor Fullerton had read and digested the chapter of my book on the use of the words for "king," he would not have suffered the pangs of "amazement" and "stupefaction" with which he was affected when he read that "if it were a valid argument against the *de facto* rule of Darius the Mede (over Babylon) to say that no records dated from his reign existed, so also would it be against the rule of Gobryas"; nor would he have said that the finding of no records is "of course, an argument against the rule of Gobryas" because "chapter vi describes a king and not a governor." The evidence does not prove any one of these statements. First, the Book of Daniel does not tell anything about the words and deeds of Darius the Mede that may not just as well have characterized a sub-king as a king of kings. Besides, he was an extraordinary *pihatu* for he had appointed *pihatu*s under him. As long as he paid the tribute of his government to the imperial power and kept the peace and ruled according to the laws of the Medes

³³ See Dillman's *Lexicon*, under *negus*.

and Persians, he could do, for aught we know, whatever he pleased inside his own dominions. Such has invariably been the custom in the East. It was so among the ancient Egyptians, Assyrians, Babylonians, Persians, Parthians, Sassanians, and Abbasids, and has been so among the Turks and the Hindus. It is a necessary accompaniment of the satrapial form of government.³⁴

In the second place, it seems perfectly clear to me that since the documents declare Gobryas to have been *pihatu* (i.e. in Aramaic, *malka*) of Babylon and the Bible says that Darius the Mede was *malka* of Babylon, that the finding of no records of one is exactly equivalent to the finding of no records of the other. In fact, we have not a single document dated after the name and year of any governor, *pihatu*, or sub-king (even when called *sharru*) of Babylon, whether under Babylonian, or Persian rule. During the last two years of his reign Cyrus associated his son Cambyses with him on the throne and Cambyses governed Babylon and probably all the surrounding regions while Cyrus was engaged in his distant expeditions against the Getae, and other tribes in Turkestan. The tablet, Strassmaier-Cyrus No. 16, is dated only from the reign of Cyrus. The phrase "Cambyses, king of Babylon" appears simply on the margin without date, or any further description. No tablet with the name of Gubaru, Zopyrus, Tritaechmes or Megapanus³⁵ has been found. So, if Darius the Mede received the governorship of Babylon from Cyrus and was made king under him, we need not expect to find tablets dated from his reign. That these governors might be called "king" (i.e. *malka* or *melek*) in Aramaic and Hebrew, we have shown above. It seems most probable, also, that they might be called "king" (that is *sharru*) even in Babylonian. For, how else can we account for the fact that *Bel-shum-ishkun*, the father of *Nergal-shar-uşur* is called "king of Babylon" in the *Trinity College Cylinder*?³⁶ Since *Nergal-shar-uşur*

³⁴ See my discussion of this kind of government in *Studies*.

³⁵ See Herodotus I. 192, III. 160, VII. 62.

³⁶ Col. I. 14.

reigned from 559 to 556 B.C., his father must have been in some sense "king" of Babylon either in the reign of Nabopolassar (626-606 B.C.) or in that of Nebuchadnezzar (606-561 B.C.), or in that of Evil-Merodach (561-559 B.C.), or, even under his own son Nergal-shar-usur. At any rate, it shows that Professor Fullerton, and all of us, must not be too cock-sure that we know all about the use of the words for "king" in the 6th century B.C.

Professor Fullerton wastes a great deal of effort in showing that the classical historians and the monuments know nothing about a Median empire (over Babylon) intervening between the Chaldean and the Persian. Since on this point the book of Daniel agrees entirely with both the classics and the monuments, we shall only say, that if Professor Fullerton likes the exercise of putting up a man of straw and hitting him, he is welcome to it. Daniel says that Darius the Mede, the son of Xerxes, of the seed of the Medes, received the kingdom which Belshazzar the Chaldean had ruled. It also calls Darius "king of the Chaldeans." Never once does it call him king of the Medes or king of Media. Harpagus a Mede was made by Cyrus, governor of Lydia; Datis a Mede led the armies of Darius at Marathon; Tachmaspada a Mede led the army of Darius against the rebels in Sagartia; Darius the Mede was made *malka* of Babylon by Cyrus. We are told that Croesus, king of Lydia, was made governor of Barine or Barce in Media,³⁷ that Astyages, king of Media, was made governor of Hyrcania,³⁸ and that Nabonidus, king of Babylon, was made governor of Carmania;—all by Cyrus. Xenophon speaks of a Cyaxares II, son of Astyages, as a coadjutor-king with Cyrus. It looks as if the great "kings of kings," the Persian monarchs, were in the habit of making use of the great men of the subject races for the better government of the subject provinces.³⁹

³⁷ Justinus I. 7.

³⁸ Justinus I. 6.

³⁹ In fact my theory of sub-kings is broad enough to leave room for a sub-king Darius the Mede with Gubaru as *pihatu* of Babylon under

Professor Fullerton lays great stress upon "the silence of the classical authors about Darius the Mede, but he says not a word about their silence concerning Belshazzar. Consistency, thou art a jewel! I see that Belshazzar the son of the king is mentioned in nine of the tablets lately published in Vol. III. of the *Yale Babylonian Texts*. Two new instances of oaths taken in his name are found in numbers 225 and 232. It will be remembered that oaths were never taken in the name of any man who was not a king.⁴⁰

Darius the Mede, the son of Xerxes, of the seed of the Medes (whether he be the same as Gobryas, or Cyaxares II, or some other person), was simply one of the numerous

him. There is much to be said in favor of the supposition that Darius the Mede was of the royal line of Cyaxares the conqueror of Nineveh. The two Medes who rebelled against Darius Hystaspis called themselves respectively *Khshatriya* a descendant (*zeru*) of Cyaxares, and *Kithvatakhma* a descendant of Cyaxares (Behistun Inscription, §§ 24, 33). So Darius the Mede is said in xi. 1 to be the son of *Ahasuerus of the seed of the Medes*. Now in some texts of Tobit, Cyaxares is rendered by Asoueros and in others by Achiacharos. It is possible that the אַחַשְׁוֵרֶשׁ of Daniel xi. 1 was meant by the original writer for Cyaxares. In this case Daniel would make Darius the Mede to be a descendant of Cyaxares, the conqueror of Nineveh. There is much to be said, also, in favor of identifying Darius the Mede with the Cyaxares the Second who is so often mentioned by Xenophon in his *Cyropaedia*, and is called the father-in-law of Cyrus. (See Moses Stuart: *Commentary on Daniel*, pps. 148 f.) My theory of sub-kings will harmonize Xenophon, the monuments and the Bible. That sub-kings (called kings) were common in the time of Cyrus is evident from Xenophon's *Cyropaedia*, when he speaks of the following sub-kings: of Atredates king of Susa (vi. 34), of Aradus king of Arabia whom the king of Assyria had made his vassal (I. v. 2, I. i. 5), of Cambyses the father of Cyrus as sub-king of Persia under Astyages (I. v. 4); of Artacamas king of Greater Phrygia, Aribaeus king of Cappadocia, as sub-kings of Croesus (II. i. 5), of the king of Armenia who had revolted against the king of Assyria (II. iv. 12, iii. 1), of the kings of Cilicia and Cyprus who paid tribute (III. iv. 2), of Gedates a prince subject to the Assyrians (V. iv. 34), of Gobryas the sub-king of the Assyrians (IV. vi); of the king of Hyrcania (V. ii. 22-23); of the king of Lesser Phrygia (VII. iv. 10); and especially of Cyaxares II, king of Media, said to have been the uncle of Cyrus and treated by him as his equal in rank and almost in power (I. iv. v.), *et al mult.*

⁴⁰ See *Studies* p. 125.

governors or sub-kings (Aramaic *malka*) who were appointed by the Persian king of kings to rule as his subordinates over the subject provinces. Hystaspes himself, the father of Darius Hystaspis, was such a sub-king. Darius calls his father a king,⁴¹ and yet according to the Behistun Inscription (§§35, 26), he was apparently a governor of Parthia and certainly commanded the army sent to subdue the rebellious provinces of Parthia and Hyrcania. According to Herodotus I. 209, Cyrus implies that Hystaspis was his governor in the land of Persia; yet, Darius calls his father king. Such facts should make us hesitate before charging that the author of Daniel is wrong in calling Darius the Mede king of Babylon.

The trend of modern scholarship with regard to Belshazzar is clearly shown in the statement made by Walter Miller in his translation of the *Cyropaedia* that Xenophon probably meant Belshazzar by the "young king" whom he mentions.⁴²

But, says Professor Fullerton, the order in which the kings are mentioned in Daniel shows that the author can not have meant that any one of them was reigning synchronously with another. In his own words: "We have the following consequence: Belshazzar, the last king of Babylon v. 30, Darius v. 31 and vi; Cyrus vi. 28. *In agreement with this is the sequence in chaps. vii-x* (the italics are Professor Fullerton's); first year of Belshazzar vii. 1; third year of the same viii. 1; first year of Darius ix. 1; third year of Cyrus x. 1. Does Prof. Wilson claim that these dates in chaps. vii-x do not indicate sequence? Unfortunately we cannot tell *for he does not refer to them*" (italics Professor Fullerton's).

Judging from the italics, Professor Fullerton evidently thinks that he has in this passage a formidable and unanswerable indictment either against my knowledge or my

⁴¹ Behistun Inscription 3, 4. Smaller Inscription 3, 4.

⁴² Loeb edition, p. 457, note.

fairness. Is he then ignorant of the fact that the first year of Darius the Mede is referred to in the eleventh chapter, verse one, of Daniel? Is it fair for him to give the dates of vii. 1, ix. 1, and x. 1 and then omit xi. 1, because it overthrows his whole conception of sequence? The sequence for which I stand is the sequence of the book of Daniel; for the historical part, Nebuchadnezzar (i-iv), Belshazzar (v), Darius the Mede (vi); for the visions, Belshazzar (vii. 1, viii. 1), for Darius (ix. 1), for Cyrus (x. 1), and for Darius (xi. 1). A fair interpretation of vi. 28, and one justified by the dual datings in use in all times, shows that Daniel meant us to know that Darius and Cyrus were reigning at the same time. The visions of these synchronous reigns are arranged as he thinks best, i.e. in the order Darius, Cyrus, Darius. This order is perfectly proper, if they were reigning at the same time. If they were not reigning at the same time, how are we to explain the sandwiching of a vision from the reign of Cyrus in between two from the reign of Darius the Mede? Will Professor Fullerton please explain this and also why he omits a reference to xi. 1 from his lists of sequences?

Professor Fullerton asserts that it is a serious defect of my work that I (1) "take so little account of the statements of the classical historians which bear upon the problems of Daniel. This prevents him from (2) observing, or at least permitting others to observe, the *trend* of the evidence when the statements of the monuments are compared with the previous data."⁴³

This is a false and misleading charge. It is not the case that I have taken little account of the classical historians; for the volume in review shows, and all my works show, that I have striven to make use of everything that any of the classical authors can afford, that will throw light upon the book of Daniel. I have made diligent use of Herodotus, Aeschylus, Thucydides, Xenophon, Polybius, Diodorus Siculus, Strabo, Pliny, Arrian, Plutarch, Q. C. Rufus and

⁴³ The numberings are mine.

Eusebius, and of the Greek inscriptions, epistles, tragedies, and of the fragments of Ctesias, Berosus, Manetho, Alexander Polyhistor, Dios, Nicolaus of Damascus, Abydenus, Julius Africanus, Eratosthenes and Syncellus. Perhaps, I should have made larger use of these works; but I was chiefly concerned to employ them to answer the charges of the critics; and besides, the historical method which I employ compels me to put the principal emphasis upon the primary and direct evidence of the monuments and not upon the secondary and indirect evidence of the classics.⁴⁴

⁴⁴ If I do take little account of the statements of the classical historians, I am certainly following in the steps of Winckler (whom Professor Fullerton cites as an authority); for, in his *History of Babylon and Assyria*, he cites Herodotus but three times, once on p. 276 as saying that the son of Gyges, king of India, was Ardys (Herodotus I. 15); again on p. 319 as saying that Nebuchadnezzar assumed the role of peacemaker between Cyaxares and Alyattes (Herodotus I. 74, where, however, Herodotus calls the king "Labynetus, the Babylonian"); and once on p. 326 as having made a mistake as to the purpose of the draining of the Diyala by Cyrus (Herodotus I. 189, where Herodotus calls the stream the Gyndes). Berosus is cited twice by Winckler; once, as mentioning the murderer of Sennacherib, and once as stating that Evil-Merodach was dethroned 'because he ruled unjustly and tyrannically.' He cites Ptolemy merely in the phrase "Ukinzir the 'Chinzer' of Ptolemy."

Besides, the amount and importance of the information afforded by the classical writers as to the history of Assyria, Babylonia, Media and Persia, are much overestimated by Professor Fullerton. One need only read the able article on Assyria in Winer's *Biblisches Realwörterbuch* (Leipzig 1847) to see how meagre and unsatisfactory is the evidence for the history of the period from Tiglath-Pileser to Cyrus to be derived from Greek sources. They seem to render necessary the supposition of two Sardanapaluses and a twofold conquest of Nineveh by the Medes. One Sardanapalus is said to have been father of Nebuchadnezzar. Of 38 kings of Assyria mentioned in the lists derived from Africanus through Eusebius only one is recognizable in the light of the monuments and his name is incorrectly spelled. Moreover, Herodotus the earliest of the Greek historians who gives information on this subject mentions as kings of Nineveh Ninus, Semiramis, Sennacherib and Sardanapalus; and the rulers of Babylon as having been a Labynetus I in the time of Croesus (by a mistake for Nebuchadnezzar), a Nitocris to whom he assigns the building of Babylon attributed by the monuments to Nebuchadnezzar, and a Labynetus II who fought against Cyrus. Diodorus Siculus, Strabo Justinus (from Trogius Pompeius) and Valerius Paterculus all follow Herodotus in mentioning Ninus, Semiramis

There are two points, however, in reference to the testimony of the classics to which especial attention has been called by Professor Fullerton. He commends Professor Wilson because "he very correctly makes no use of the historical ghost" (i.e. of Cyaxares II, son of Astyages), who according to Xenophon ruled over Media and was the overlord of Cyrus; "though Dr. Davis of Princeton still suggested in his *Bible Dictionary* the possibility of the identification" which had been maintained so learnedly by Moses Stuart in his *Commentary on Daniel*. In view of the fact that the Behistun Inscription represents two of the rebels against Darius Hystaspis as claiming to be sons of a Cyaxares, we cannot see how anyone can deny the *possibility* of there having been a Cyaxares the son of Astyages, and the father of these two rebels Prauartis (§24) and Shitratakma (§33). It seems passing strange, however, that on the same page Professor Fullerton should condemn me for not using the classics and at the same time commend me for paying no attention to what Xenophon, certainly one of the greatest of the classical writers, has to say.

The second matter for which I am blamed is that I do not refer to "the Canon of Ptolemy, a chronological work of the utmost importance." The point is raised in connection with the discussion of "the succession of kings at the conquest of Babylon," the implication being either that I did not know of the Canon, or that I was not fair enough to introduce it in evidence.

Without going into the matter of my ignorance or fairness, it is a sufficient explanation of my not bringing the Canon of Ptolemy into any of the discussions of Volume One to say that it has no bearing upon the subject there under consideration. Ptolemy was the greatest of ancient chronographers and his chronological list of the kings of Babylon and Sardanapalus as the kings of Assyria. Hellenicus (450 B.C.) speaks of two of the name Sardanapalus, one a king of Persia who is said to have founded Tarsus in Cilicia. No one of the Greek classics mentions Belshazzar and the name and date of Nebuchadnezzar are preserved in the fragments of Berosus and Megasthenes of the time of Alexander, the fragments themselves being found in Josephus and Eusebius.

and Persia is correct enough for general purposes. But it does not propose to give, and it does not give, a complete list of the kings of either Babylon or Persia, and is particularly deficient in the period from 606 to 516 B.C. which covers the time of the activities of Daniel. Thus, of the twenty-two kings that reigned over Babylon from Nabonassar to Nabunaid inclusive, Ptolemy mentions but eighteen;⁴⁵ and of the eighteen kings from Cyrus to Darius Codomannus, the names of eight are omitted.⁴⁶

This deficiency in the Ptolemaic Canon will be the more apparent when we observe that between the death of *Nergal-shar-usur* in 556 B.C. and the accession of Darius II in 424 B.C., *i.e.*, in 132 years, the Canon gives the names and length of reigns of only six kings of Babylon,⁴⁷ whereas the classics and monuments give the names, and in most cases, the approximate lengths of the reigns of nine others.⁴⁸

⁴⁵ The four names omitted are Nabu-shum-ukin (see Winckler's *History of Babylonia and Assyria*, p. 114), Marduk-zakir-shum, Hallasku (V.A.S.D. iv. 1), and Labashi-Marduk (Evet's in *Bab. Texte*, vi. B. 85-90).

⁴⁶ The eight names omitted are Smerdis (K.B. iv. 295-299), Nebuchadnezzar (KB iv. 209-303), Shamash-erba (V.A.S.D. iii. 178, 179, v. 116, vi. 173, 174, 175), Bel-Simanu (V.A.S.D. iii. 180, vi. 131), Akshimakku (V.A.S.D. iii. 177, 178), Artabanus (Eusebius Chron. Appendix p. 104), Xerxes II (Eccles. Canon No. 27), Sogdianus (Eccles. Canon No. 28).

⁴⁷ Nabunaid, Cyrus, Cambyses, Darius (Hystaspis) Xerxes I and Artaxerxes I.

⁴⁸ Ptolemy and those who copied his Canon have been very careful in copying the notation of the number of years. It is different, however, when we look at the proper names. Thus, of the eighteen names of the kings of Babylon from Nabonassar to Nabunaid, only the first and last, and that of Esarhaddon are written with approximate correctness. That their difference may be patent to the eye of our readers, I shall give the names in interlinear transliteration, the first line as given in the Canon, the second as we find the name on the Babylonian monuments:

1 Nabonassarou	2 Nadiou	3 Chinzirou kai Porou
1 Nabunansir	2 Nabu-nadin-zir	3 Ukinzir and Pulu
4 Iougaïou	5 Mardokempadou	6 Arkianou
4 Ululai	5 Marduk-aplu-iddin	6 Shar-ukin
7 Belibou	8 Apronadiou	9 Rigebelou
7 Belibni	8 Ashur-nadin-shum	9 Nergal-ushezib
10 Mesessimordakou	11 Assaradinou	12 Saosdoucheou
10 Mushezib-Marduk	11 Ashur-ahi-iddin	12 Shamash-shum-ukin
13 Xuniladanou	14 Nabokolassarou	15 Nabokolassarou
13 Kandalanu	14 Nabu-aplu-uşur	15 Nabu-kudur-uşur
16 Ilouarodamou	17 Nirikassolassarou	18 Nabonadiou
16 Amel-Marduk	17 Nergal-shar-uşur	18 Nabu-na'id

Perhaps, in view of these facts, Professor Fullerton will decide on second thought that it is just as well for him to ignore the Canon of Ptolemy when discussing the succession of the kings of Babylon. Perhaps, also, he will pardon me for suggesting that in laying so much stress, as he demands, upon the classical writers, I would be departing from the first principles of the "historical" method which he proposes to follow, and which I may also claim, I hope, to be following in my Old Testament studies. To cite the words of Sir George Cornwall Lewis: "Historical evidence, like judicial evidence, is founded on the testimony of credible witnesses. Unless these witnesses had personal and immediate perception of the facts which they report, unless they saw and heard what they undertake to relate as having happened, their evidence is not entitled to credit. As all original witnesses must be contemporary with the events which they attest, it is a necessary condition for the credibility of a witness that he be a contemporary; though a contemporary is not necessarily a credible witness."⁴⁹ Judged by this fundamental principle of historical research, it is not I that have erred in confining my testimony as far as possible to records contemporary with Nebuchadnezzar and Cyrus.

If I might add a second principle that I have pursued, it would be this, that I have sought as far as possible to use the documents of the chief actors in the drama of Babylon in the time of Daniel's supposed life; the records of Nabopolassar, Nebuchadnezzar, Nabunaid, Cyrus, and Darius Hystaspis, and all contemporaneous documents in Babylonian, Susian, Persian and Hebrew. Where I have called in the testimony of the Greeks and Aramaeans, I have laid especial emphasis upon Ezra and the papyri of Egypt and upon the Greek of Herodotus, because they were written nearest to the time of Cyrus. I have also paid careful attention to Berosus; for although he lived in the time of Alexander, and his works are preserved only in fragments found in much later authors, yet he was himself a Babylonian

⁴⁹ *On the Credibility of Early Roman History*, p. 16.

priest from Babylon, and hence would be acquainted with its language and its ancient records.

One more criticism of Professor Fullerton's review and I have done. In reference to the statement on the Nabonaid-Cyrus Cylinder, that a battle was fought by Cyrus in the month Tammuz, it is said⁵⁰ that the "reading does not seem to be certain at this point. It should also be noticed that the length of the interval between the original entrance of Gobryas with the troops of Cyrus and the entrance of Cyrus himself is by no means certain. Professor Wilson assumes the correctness of the present text at l. 12⁵¹ of the Nabonidus-Cyrus Chronicle (K. B. II, p. 133)⁵² which implies an interval of four months. But the probability is that Tishri⁵³ should be read for Tammuz⁵⁴ (so Winckler and Eduard Meyer), in which case the interval between the entrance of Gobryas and that of Cyrus is only about two weeks. It is interesting to observe that Prof. Wilson, though he does not even notice this textual emendation of the Nabonidus-Cyrus Chronicle, suggests the possibility of an exactly reverse correction (Tisri to Tammuz) in another inscription (apparently K.B. iv. 265, see Wilson, p. 135)."

This note of Professor Fullerton's illustrates the difficulty that one experiences in attempting to argue with a Higher Critic who is either ignorant of Babylonian, or who has not had access to the original documents. If he had examined my first volume with the care demanded of a reviewer, he would certainly have noted that I always, unless otherwise stated, cite from the text of the original. I have sought invariably to pursue this method ever since in writing my articles on *Royal Titles in Antiquity*, I perceived that so excellent a scholar as Professor Driver of Oxford was led astray by relying upon the *Records of the Past* and other similar versions. Had Professor Fullerton observed

⁵⁰ *Bulletin*, p. 19.

⁵¹ Reverse.

⁵² Should be III. II. 132.

⁵³ i.e. the seventh month.

⁵⁴ i.e. the fourth month.

this, he would have known that when I say that "Strassmaier gives but twelve tablets from the end of the fourth month of the 17th year of Nabunaid until the 11th of the 8th month" and that "all of these are dated with the name of Nabunaid, except one bearing the name of Cyrus," he would have known, I say, that this last tablet was to be found in Strassmaier's *The Inscriptions of Cyrus*.⁵⁵ Since Strassmaier arranges the tablets according to year, month and day, and only one of Cyrus is said by me to be dated before the eighth month and only one dated from the reign of Cyrus is given by Strassmaier before the eighth month, it is obvious that I refer to the first tablet in the *Inscriptions of Cyrus*. It is not *apparent*, as Professor Fullerton says, that I refer to K.B. iv. 265 (where Peiser's translation occurs). It is certain that I refer to Strassmaier's original text, number one. If I had referred to K.B. iv. 265, I would probably have noted that Peiser reads the name of the month as possibly *Shebit* (i.e. the eleventh month) instead of *Tishri* (the seventh, as Strassmaier reads) or *Tammuz*, as I suggested.

Now, Professor Fullerton tells his readers that "it is interesting to observe that Professor Wilson suggests the possibility of a correction of the text in this place," i.e. Strassmaier's *Inscriptions of Cyrus*, I, 14. In fact, I have said, that this tablet is "dated the 7th (or perhaps better the 4th) month of the accession year."⁵⁶ Strassmaier himself suggests the 7th month; Peiser the 11th; I, the 4th (as possible). Why this difference? *Because the sign which denotes the month is abraded in the tablet itself*, so that anyone of these readings is possible. It is like a tombstone on which we can clearly read A.D. 1800 J——. the day of the month being entirely rubbed off and only J being left to denote the month. Does the J stand for January, June or July? The tombstone itself being un-

⁵⁵ *Inschriften von Cyrus*.

⁵⁶ *Studies*, Vol. I, p. 135.

certain, we are driven to conjectures. So it is in the case of the Cyrus tablet No. 1.

Professor Fullerton, however, charges me with inconsistency because I call attention to this uncertain sign while "*assuming the correctness of the present text at l. 12 of the Nabonidus-Cyrus Chronicle.*"⁵⁷ But there is no inconsistency, because the cases are not parallel. The date in the Chronicle is like one on a tombstone reading clearly, JOHN SMITH, JAN. 16, 1800. No one can dispute, or does dispute the reading; but the contestant on one side in a case in a dispute involving the day of death of Mr. Smith claims the stone cutter made a mistake in engraving JAN. *instead of* JUNE. So in the Nab-Cyrus Chronicle, Rev. 1. 12, no one disputes that the scribe wrote *Tammuz* as Professor Pinches gives it in the *Transactions of the Society of Biblical Archaeology* vii. 164. Professor Schrader of Berlin "the father of Assyriology in Germany" so reads it.⁵⁸ Professor A. H. Sayce of Oxford so reads it.⁵⁹ Professor Albert T. Clay so reads it.⁶⁰ Professor Winckler so interprets it in his *History of Babylonia and Assyria*.⁶¹ Eduard Meyer⁶² says that Babylon surrendered to Gobryas on the 16th of Tishri,⁶³ wilfully going against the text of the cylinder both before and after the account of the entrance. For in line 12 it speaks of the month *Tammuz*, at line 15 of

⁵⁷ K.B. III. II, 133.

⁵⁸ K.B. III. II., 132.

⁵⁹ Article "*Babylonia and Assyria*" in *Enc. Brit.* III. 106.

⁶⁰ *Light on the Old Testament*, pps. 374-375.

⁶¹ P. 326.

⁶² Article "Cyrus" in the *Enc. Brit.* VII. 707.

⁶³ Umu sissaesru *amelu* Ug-ba-ru piḫatu mat Gu-ti-um u ṣabini *amelu* Ku-raš ba-la ṣal-tum ana E-ki erubu. See T.S.B.A. vii. 165. The necessity for the scribes being careful in writing the signs for the month will be evident from the fact that in Obv. Col. I. 9 he mentions *Tebit*, in the same l. 14, and Rev. I. 21 *Kisliu*; in Obv. II. 10 and 13, 15, Rev. I. 23 *Nisan*; in Obv. II. 14, 21, *Sivan*; in Obv. II. 24, Rev. I. 10, *Ehul*; in Rev. I. 2, 21, 27, *Adar*; in Obv. I. 12, 16, *Tammuz*; in Obv. I. 18. 22, *Marchesvan*. That is, signs for eight months occur eighteen times, and all are correct except the one that does not suit some German critic's theory!

16th of the month, and at the end of line 16 of the end of the month Tammuz, whereas in line 15 and the beginning of line 16 it says that "Gobryas, governor of Gutium, and the soldiers of Cyrus entered Babylon without fighting." In fine, there is no "probability" as Professor Fullerton claims, that in this case we should read *Tishri* instead of *Tammuz*. The text of the document is clearly in favor of Tammuz. The opinions of the experts are all in favor of the reading, *Tammuz* (i.e. the fourth month).

Tombstones are usually made while the friends of the deceased are still alive. The children know whether their mother died in January or June and you may be sure that the date on the head stone will correctly record the month. So, the royal inscriptions were made in the lifetime of the kings whose deeds they celebrate. The work of the royal scribes was not merely written under fear of the royal displeasure when they erred; but was inspected by other scribes who put upon it the seal of their approval.⁶⁴ In the one case January will not be put for June. In the other case Tammuz would not be put for Tishri. So much certainly we can assume.

In conclusion, let me express the hope that the critics of the Book of Daniel will cease making charges against it, without producing some objective evidence to support them. Also, that it might be well for them to read the Book of Daniel carefully enough to find out what it really says. Otherwise, they may waste their time and ours, as they have so frequently done in the past, in attacking the author of Daniel for making statements that he has never made at all. In the mean time, will they pardon me for continuing to abide in the serene conviction that, judged by the best evidence attainable, they have thus far fought a losing fight, simply because the evidence is against them. Belshazzar, in spite of the silence of the classics even as to his name, and notwithstanding the whilom denial of the critics that such a man ever existed, now stands forth on the pages of the

⁶⁴ See Streck's *Assurbanipal*, vol. II, 355.

monuments as possessor of all the prerogatives of a king. No real scholar can longer deny the possibility of the existence of the king whom Daniel calls Darius the Mede. He may have been Gubaru. He may have been Cyaxares II. He may have been a third man as yet unknown except from the description in Daniel. We know enough to say that he may have existed. We do not know enough to say that he may not have existed. Until we do know enough to affirm that he did not exist, the account in Daniel may be reasonably believed.

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R. D. WILSON.