

sums insured to bear upon each loss, there is actually a greater amount insured than is required to satisfy the *pro ratâ* condition; so that the policies effected with that condition sustain a smaller proportion of loss than they would have done had the assured either been his own assurer for the excess of sums beyond the amounts insured by them or insured it in another Office. The assured should not, therefore, bear any share of the loss himself, as his obligation to the *pro ratâ* policies has been satisfied by the substitution of the specific policies.

I am, Sir,
Your obedient Servant,

London, 18th April, 1859.

THOMAS MILLER.

REPLY TO PROFESSOR DE MORGAN'S REMARKS AS TO THE
AUTHORSHIP OF GRAUNT'S OBSERVATIONS.

To the Editor of the Assurance Magazine.

DEAR SIR,—I have read, with all the attention due to the authority of so eminent a writer, Professor De Morgan's letter, published in your last Number,¹ criticising the assertion quoted by me, that the *Observations on the Bills of Mortality*, published in the name of Capt. John Graunt, were actually written by Sir William Petty.

The question of the authorship of the earliest work upon vital statistics can hardly fail to be considered an important one by persons interested in the science, among whom will, no doubt, be found the great majority of your readers; and I trust I may, therefore, be permitted to intrude upon your space for the purpose of stating why the facts and arguments adduced by the learned Professor appear to me less conclusive than he evidently considers them.

Before going into the discussion, however, I must point out that, whatever may be thought of the assertion in question, your correspondent is certainly in error when he attributes the revival of it to me—thereby implying that it had been previously an obsolete and exploded opinion.

So far is this from being the case, that I have generally found a similar view to my own entertained by the few persons I have met with who have paid attention to the subject; and this view has been promulgated in more than one popular work of recent date.

Of all the books published during the present century, relating to the time when Graunt and Petty lived, the one most extensively read has been, undoubtedly, Macaulay's *History of England*; and, next to that, may probably be placed the *Diary of Samuel Pepys*. Now, both the author of the first and the editor of the second of these concur in ascribing the work under consideration to Petty.²

Let us examine the facts brought forward in support of a contrary opinion. It is stated, 1stly, that the work was published in the name of Graunt; 2ndly, that he derived great reputation from it, and was, in consequence, elected a Fellow of the Royal Society; 3rdly, that Sir William

¹ *Assurance Mag.* viii. 166.

² See Macaulay, 1st edition; Lond., 1849, i. 282, Note. Pepys, 4th edition, Lond., 1858, i. 266, Note.

Petty, in his published writings, although he occasionally mentions the book as his own, much more frequently speaks of it as Graunt's; 4thly, that he published an edition of the work, in which Graunt was described as the author.

Of these facts the third and fourth only require consideration, because the first and second are in entire accordance with the assertion controverted, which is, that, although the work was written by Petty, it was, with his concurrence, published in the name of Graunt.

It is not necessary for us to determine what could have been Petty's object in making such an arrangement—whether it was for some personal convenience or advantage to himself, or merely to gain a reputation for Graunt; but assuming that he did make it, his subsequently speaking of the book as Graunt's appears to me not only natural but inevitable.

Sir Walter Scott would have been guilty of a very curious piece of inconsistency, at the time he wished his connection with the *Waverley Novels* to remain a secret, if he had referred to them as his own, in a work published with his name; but Petty had much more reason for adhering to the original fiction, if fiction it was, because, in denouncing it, he would have exposed his friend to ridicule and contempt, by stripping him of the borrowed plumes, which, according to the hypothesis, Petty had himself induced, or allowed, him to wear. In addition to this, there is the very simple reason, that a writer desirous of referring his readers to a book, is necessarily compelled to make use of the title by which it is known to them.

The publication by Petty of the fifth edition in Graunt's name, appears, certainly, to be a strong fact, but it took place soon after the death of Graunt; and, at that moment, the consideration already referred to would probably have more influence upon Petty's mind than even during the lifetime of his friend.

Professor De Morgan insists strongly upon the difference, both in the style of the *Observations* and in the opinions expressed in them, from the style of Sir William Petty's known writings and the opinions therein to be found. As regards the latter, I do not find any greater discrepancy of opinion than might have occurred in the course of time. On the contrary, some opinions in the *Observations*, regarding pauperism, appear to coincide with the views upon the same subject expressed in Petty's will; and the argument as to style cuts both ways, for the style of the later chapters, particularly of "The Conclusion," appears to me to differ much less from that of Petty's acknowledged works than it does from the style of the earlier parts of the book itself.

I speak with some diffidence of the error in astronomy upon which the learned Professor lays so much stress. I cannot but think, however, that he overrates the knowledge of the science possessed by Petty, who certainly tells us in his will that, at fifteen years of age, he had acquired "the practical geometry and astronomy conducing to navigation, dialling, &c.," but does not appear to have pursued the subject further. Professor De Morgan will, I think, be ready to admit that, at a period preceding by nearly half a century the first publication of Newton's discoveries, a lad of fifteen was not likely to acquire very sound ideas as to theoretical astronomy. Besides, the paragraph objected to stands unaltered in the fifth edition edited by Petty, and the question naturally arises, How came he to publish, as an

editor, that which, it is asserted, he must have known to be so grossly absurd that it is impossible he could have published it as a writer?

The author of the life of Graunt inserted in the *Biographia Britannica*, urges the great improbability of Petty's proposing, or of Graunt's acceding to, such a course as they are said to have adopted. I am quite willing to admit the apparent improbability. The only answer I can give is, that much greater improbabilities have been believed upon much more slender evidence than can be adduced in the present case; and it must be borne in mind that this improbability, as well as all the other considerations cited in support of it, are not more obvious to us than they must have been to the writers who recorded, notwithstanding, that the *Observations* were written by Petty.

If I were disposed to argue the question upon probabilities, I might ask what other evidence Graunt gave of his capacity for writing such a work. The author, whoever he may have been, was undoubtedly a man of genius, who struck out an entirely new path of scientific inquiry, and followed it with such skill as to draw from so competent a judge as Milne the opinion, that, "although his work was the first, it was one of the best" that had been published upon the subject.¹ It is certainly strange, if Graunt were the man, that he should have stopped short after having made such a remarkable step. Of Petty's abilities for dealing with the subject it is unnecessary to speak.

Arguments of the kind we have been considering are, however, of very little value, in my opinion, when compared with the direct testimony of competent witnesses. Burnet has stated positively that the *Observations* were written by Petty,² and in this he is partly confirmed by Anthony à-Wood.³ I do not see why Burnet's evidence should be objected to, as although we may admit that he was credulous and prejudiced, very few persons, I think, would doubt that he honestly believed whatever he reported; and, as an active member of the Royal Society, well acquainted with Petty, he had ample opportunities for ascertaining the truth: still, if his statement were supported by no other writer than Anthony à-Wood, I might hesitate in adopting it without reserve. But Professor De Morgan has overlooked a much more important witness, and one whose evidence I venture to believe would alone be considered decisive. John Evelyn has recorded in his *Diary*, under the date of the 20th January, 1674-5, that he supped at Sir William Petty's "with the Bishop of Salisbury and divers honourable persons." He appears, like most of his contemporaries, to have entertained the highest admiration for Petty, and took the opportunity of making the entry to give an account of him and his works. Among other things, Evelyn says, "He is the author of the ingenious deductions from the bills of mortality which go under the name of Mr. Graunt."⁴

Next to Graunt and Petty themselves, no person in the world was so likely to have known the truth upon this subject as Evelyn. Abundant proof is to be found, both in his *Diary* and elsewhere, that he was intimately acquainted with Petty during the whole of his career; and no person was better informed as to everything that took place in the

¹ *Ency. Brit.*, 7th edition; art. "Mortality."

² *Hist. of his own Times*; Lond., 1838, p. 204.

³ *Ath. Ox.*; Lond., 1813; art. "Sir W. Petty."

⁴ *Diary*; Lond., 1859, ii. 104.

Royal Society. As he died before Burnet's *History* was published, their testimonies are quite independent. Equally independent is the testimony of Anthony à-Wood; and if such evidence is to be rejected, I think there are few facts in history that can be considered as established.

Having stated the case as fairly as I am able, I beg leave, with the greatest respect both to my learned opponent and yourself, to subscribe myself,

Dear Sir,

Your faithful servant,

Camden Hill, W.,
13th June, 1859.

W. B. HODGE.

PROFESSOR SYLVESTER'S LECTURES.

To the Editor of the Assurance Magazine.

SIR,—In some former communications, I have urged upon the consideration of your readers that the doctrine of life contingencies, in place of being based, as for years past, upon specific, and, in many instances, dogmatic assumptions, should rather recognise the results with which it has to deal as essentially variable, and, as such, better predicated for by a system of variation than one of assumptive fixity.

Your readers are aware, that one great step has already been taken in this direction, by the now recognised admission, that, even in the calculations of compound interest, it is not advisable to always assume that the interest upon interest will necessarily be at the same rate as the interest or dividend upon the original capital. The modern actuary is thus not thrust, as the old school were, upon such a generalization as that in $(1+i)$ compounded for n years, the value of i must be necessarily constant.

Indeed, you have already inserted tables by Mr. Peter Hardy, involving a mixed rate for annuities certain; and Mr. Willich has illustrated the same principle in life annuities in the new edition, just published, of his excellent *Popular Tables*. Thus, in new Table IV., the value of a life annuity (Carlisle mortality) is, at age 30, 16·37 years' purchase, if the simple interest be 4 per cent., but varies in compounding to 3. It will thus be seen that, even in tables deservedly called "Popular," the change is commencing.

The more particular object, however, of the present communication is to direct the attention of your readers to some discoveries, for so they may be rightly called, of Professor Sylvester, which, I think, will eventually be found to also aid the progress of actuarial calculation—and not the less so because the learned Professor has heretofore been a member of the profession.

The purpose of the discoveries is, I think, sufficiently defined in the following paragraph, which appeared in the *Times* of Saturday, June 10th, 1859, and which I accordingly beg to offer for your insertion, and venture to recommend to the notice of members of the Institute who are still perfecting their mathematical education.

I shall only premise, that, if we pass from a system of special values to one of limits, the partition of numbers becomes, immediately, of the highest importance—the vagueness of an average that does not indicate