Leibniz in Paris 1672–1676, *his growth to mathematical maturity*, by Joseph E. Hofmann, Cambridge University Press, 1974, 1 vol., xi+372 pp., $23.50 (translated by A. Prag and D. T. Whiteside from *Die Entwicklungsgeschichte der Leibnizschen Mathematik während des Aufenthaltes in Paris* (1672–1676), Leibniz Verlag, München, 1949, 1 vol., i+252 pp.)

As a postscript to the foreword tells us, "On 9 November 1972 Professor Hofmann was knocked down by a motor-car while on his early morning walk; some six months later he died, two months after his 73rd birthday" Such was the sad end of this pedestrian, one of the few thoroughly reliable and scholarly writers on XVIIth century mathematics among contemporary historians of science. Thus a review of his last work must turn into a kind of obituary.

This is not the place for a full-scale biography. It will be enough to say that he was born and bred in Munich, prepared himself for a teacher's career, but took a Ph.D. in mathematics in 1927. Soon after that, while teaching from 1928 to 1939 in Bavarian secondary schools, he turned his attention to the history of mathematics. It was his good fortune to be called upon to collaborate with H. Wieleitner, the best living historian of mathematics in those days, from whom he obviously got his training. In 1931, their joint paper *Die Differenzenrechnung bei Leibniz* was published (*Sitz. -ber. Pr. Ak. d. W.* 1931, pp. 562–590); in that same year the collaboration was cut short by Wieleitner's illness and death. From 1939 onwards, Hofmann was able to devote himself wholly to his favorite subject, being for many years editor-in-chief of the monumental Academy edition of *Leibniz' Complete Works*, and lecturing at various German Universities. Thus no one was better qualified to write on Leibniz, and his preeminence in that field became widely acknowledged.

As the author tells us, this book was written in the weeks following the end of hostilities, and completed in 1946; the German edition appeared in 1949. When the English version was undertaken, "it soon became clear", says Hofmann, "that the original would require thorough revision." Actually, footnotes have been added or expanded; if one may rely upon unsystematic spotchecks, the text itself has hardly undergone any change; mysteriously, a few passages have been lost in transit (e.g. the concluding sentences of Chapter XX). In all other respects, the translation, due to two well-known Newtonian scholars, seems excellent; it preserves faithfully the lively while unpretentious style of the original, a good pedestrian style in the true sense of the word.[1] The pace is that of a

[1] Must we remind the reader that "pedestrian" connotes *prose*, and need not be derogatory unless it is applied to poetry?

brisk long walk through a mostly attractive countryside, interrupted only by occasional thickets. After more than twenty years, the old gentleman is revisiting his favorite haunts; he knows every path, nay, every single tree, and invites some of his new friends to follow him. Would it not be boorish to decline the invitation, and ungracious to hint that a different itinerary might have been more to our personal taste?

His aim, as described in his preface, sounds modest enough at first: "The present [book] may serve to supplement the first volume, now in the press,[2] of Leibniz' *Mathematisch-naturwissenschaftlich-technischer Briefwechsel* (in the Berlin Academy edition); this contains all texts relating to Leibniz' formative stay in Paris during 1672–76 . . . . The present monograph seeks to make known to a wider circle of readers what seems to me the essence of this textual volume . . . ." In the conclusion of the book, however, he proclaims a higher purpose: "Leibniz, enthusiastically and unreservedly gathering and absorbing all the knowledge in any way accessible to him and then forming it in a grand new synthesis into a unified whole, is the first true historian of science. From him has been borrowed the method used in the present study . . . ." It may therefore not be wholly unfair to measure his book against the lofty standards he has set himself, while at the same time describing his not inconsiderable achievements.

We begin with a remark on style. The author has a good lively prose style, as we have said; this is true above all of most of the narrative passages and of his usually successful delineation of the characters (major and minor) in his story. Not only Leibniz, Huygens, Tschirnhaus, but also Oldenburg, Pell, Collins come out alive. Only the ever enigmatic Newton remains like a huge stone sphinx in the distance, but so he must also have appeared to Leibniz, who never met him. On the other hand, since this book consists largely of an account of what those people wrote in their letters and private papers, one is surprised at first glance by the total absence of quotation marks. This is deliberate, for we read in the German preface: "My main task was to indicate briefly the views of all concerned; this I have sought to do through the systematic use of indirect discourse". Indirect discourse is a dangerous device, to be used sparingly, as every apprentice writer soon learns. Especially when Leibniz has told a story, why not let him use his own words? In 1673, he had a memorable conversation with Huygens, to which he refers repeatedly in later life. Hofmann describes it thus (p. 47):

Huygens . . . gave him a copy of the *Horologium*, talked to him about

[2] That volume, said to be "in the press" in May 1972, had not yet reached Princeton in April 1975.

> this latest work of his . . . and how eventually everything went back to Archimedes' methods for centers of gravity. Leibniz listened intently; at the close he felt he had to say something, but what he brought up was clumsy to a degree; surely a straight line drawn through the centroid of a plane (convex) area will always bisect the area, will it not? This was nearly too much; if this had been one of his mathematical rivals like Gregory or Newton, then Huygens would probably never have condoned such a remark, but what this innocent young German had to say one could not really take amiss; good-humouredly, Huygens corrected his error and advised him to seek out further details from Pascal, Fabri, Gregory . . . Leibniz procured the books named by Huygens . . . and went really deeply into mathematics.

This is not badly told, though some of it is mere fancy. Now listen to Leibniz, even in a weak rendering of his Latin letter of 1680 to Tschirnhaus:

> At that time, I had hardly given a few months to the study of geometry. Huygens, having published his book on the pendulum, gave me a copy. At that time I was quite ignorant of Cartesian algebra and of the method of indivisibles [i.e. infinitesimals]; I did not even know the true definition of a center of gravity. In fact, happening once to be talking with Huygens, I thought that a straight line, drawn through the center of gravity, must always divide an area into two equal parts, and I said so; as this is obviously true of squares, circles, ellipses and some other figures, I imagined that it must always be so. Hearing this, Huygens started laughing; nothing, he told me, could be further from the truth. Goaded, so to say, by this incident, I started studying higher geometry, having not even read the *Elements* [i.e. Euclid] . . . Huygens, taking me to be a better geometer than I was, gave me Pascal to read . . .

On another occasion, Hofmann summarizes (pp. 188–194) the decisive private notes of 1675, where $\int$ and d occur for the first time in history. He duly observes that $\int$ replaces the earlier notation *omn* while the paper is in progress and that the inverse operation is at first written l/d and later changed to dl. But there is no mention of Leibniz' fascinating dialogue with himself throughout these notes: "It will be useful to write $\int$ for *omn* . . . Thus the law of homogeneity always holds, which is useful in order to avoid mistakes in the calculations . . . This is new and remarkable enough, since it indicates a new kind of calculus [i.e. a new algorithm]; but let us go back a bit. Given is l, x is the independent variable, one asks for $\int l$. This is done by considering the inverse operation; that is, if

$\int l = y$, we put $y = l/d$ . . ." And further on: "Last year I set myself the question... But we have done nothing yet; we should therefore see . . ." And, after a calculation involving perhaps the very first appearance of the symbol Log for the logarithmic function: "This is a quite memorable method . . ." Then again: "In order to acquire more facility in dealing with the very hard questions of that type, it will be useful to take up the following one . . . But now this is not hard to solve . . ." Leibniz' greatest brainchild is being born, and we are witnessing the delivery. Hofmann does say at one point that "Leibniz has to gain familiarity with his newly introduced notation", but is that quite the same as to hear Leibniz say so to himself? Even if Hofmann had merely planned to give a supplement to the original texts, could he not have preserved a little more of their flavor by a judicious use of some direct quotations?

To paraphrase Leibniz' beautiful prose can only disfigure it. On the other hand, opening the book at random, we come across paragraphs beginning with such phrases as: "In Oldenburg's letter, he tells briefly (p. 31)... Collins further gives a survey (p. 40)... Collins next relates that (p. 137)... Collins begins with the observation that (p. 202)... In his introduction Tschirnhaus says that (p. 250)... Collins now gives a lengthy survey (p. 255)..." Of this last exchange, we are told (p. 257) that "as an episode in the general history of mathematics the discussion... appears as largely fruitless... and leads nowhere; for the purposes of the Leibniz scholar, it is, however, of the greatest importance... because it provides evidence that Leibniz learnt through Tschirnhaus nothing of any significance regarding the results reached by the English mathematicians". But the reader, by definition, is not a Leibniz scholar; Leibniz scholars do not read paraphrases of readily available texts. Of an earlier letter (of 8 pages) from Collins, we were told (p. 139) that it was "long, poorly arranged"; but we were given, in 7 pages, a paraphrase, faithfully preserving the ill arrangement of the original. Of one of the main statements in it (about Gregory's work), we read (p. 132) that "from the isolated result that was sent to him Leibniz could not draw anything at all"; nor can the reader, unless he knows a good deal more about Gregory than he can learn from this book.

More is involved here than a clumsy stylistic device. Later exchanges between Leibniz, Tschirnhaus, Huygens, the Bernoullis were conducted for the sake of the advancement of science and of communicating mathematical ideas. Not so the correspondence between the English mathematicians, on the one hand, and Leibniz and Tschirnhaus on the other; even when they did not descend to the level of Newton's anagrams, those letters were written in order to establish priorities and conceal methods. Leibniz himself, a far more open man by nature than his English correspondents, once erased from a first draft[3] two passages which might have given away his method for deriving the power series for e^x and $\cos x$ from their differential equations. When Newton makes a show of opening up, he is really offering a rehash of old methods which he privately regards as largely superseded by his more recent work.

The truth is that those celebrated letters are of little value to the historian more concerned (as he should be) with ideas than with individual results. Scrutinizing them too closely has always been a waste of time; paraphrasing them does not make them more interesting than they were before. As had long been known to all unprejudiced scholars, developments on the continent and in England after 1670 (we leave aside Scotland, since Gregory died in 1675) were essentially independent of one another. Newton learned nothing from Leibniz[4] and did not care. Leibniz ardently wished to be instructed, but the one thing of value he learned was that Gregory and Newton had been far ahead of him in the theory of power series and that a great deal of work would still be required of him in order to catch up with them in that field. As to "the calculus", he was never given the opportunity of learning anything from Newton. In the early stages he could have learned a good deal from Barrow's *Lectiones geometricae*; but, by the time he read them, he found little there that he could not do better. At any rate he says so (while at the same time giving high praise to Barrow). In the absence of any serious evidence to the contrary, who but the surliest of British die-hards would choose to disbelieve him?

Perhaps the reader of this volume would have been spared a great deal of dull material if the author, at the outset, had made up his mind whether to write the "grand synthesis" he seemed to promise us or to appear as the lawyer for the defense in the absurd prosecution for plagiarism launched against Leibniz in the early years of the XVIIIth century by Sir Isaac's sycophants and eventually by Sir Isaac himself. Even if there could ever have been a case against Leibniz, C. I. Gerhardt's excellent publications seemed to have closed it long ago. But we find Hofmann constantly on the defensive: in his introduction (p. 10–11), throughout

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[3] This is the draft of the letter of 27 August 1676 to Oldenburg, discussed on pp. 155–156 of the German edition; regrettably, the reference to the second deleted passage has disappeared from the English edition. Some further details can be found in I. Newton, *Correspondence* (ed. H. W. Turnbull), vol. II, p. 74, note 19.

[4] Here we may note one of the rare lapses from our author's usually faultless scholarship. In 1676, Newton writes: "Leibniz' method of series expansion is quite elegant . . . It pleased me all the more, since I knew three such methods and should scarcely have expected another one". The paraphrase, p. 261, reads: "He knows three ways of expanding in series and scarcely expects there to be any more". A polite though grudging compliment is turned into a churlish rebuke.

the book (pp. 77–78, 98, 140–142, 172, 186, 210, 225, 231–233, 251, 257, 275–276, 279–280), even in his final conclusion (p. 306). Whole chapters have no other purpose. How differently Leibniz himself had proceeded when attacked! He composed his *Historia et origo calculi differentialis*, where in a few masterly pages he gives the whole history of his major discoveries in a nutshell, beginning with the famous words:

> It is most useful that the true origins of memorable inventions be known, especially of those which were conceived not by accident but by an effort of meditation. The use of this is not merely that history may give everyone his due and others be spurred by the expectation of similar praise, but also that the art of discovery may be promoted and its method become known through brilliant examples. One of the noblest inventions of our time has been a new kind of mathematical analysis, known as the differential calculus; but, while its substance has been adequately explained, its source and original motivation have not been made public. It is almost forty years now that its author invented it . . . .

When Leibniz, in the heat of controversy, attempts to assess Newton's achievements, we may not always find him as fair as one could wish, in spite of his evident efforts to be so; but, as long as he speaks of his own work, he is not only strikingly sincere but (what is more) remarkably objective. As to his veracity, apart from some insignificant lapses of memory, it has been fully vindicated by Gerhardt's publications.

Hofmann does mention (p. 294) "rich new material" on which he claims to have based his exposition and thanks to which he hopes to have "spared the reader all the detours" necessary to correct the errors of earlier writers. Truly his knowledge of the Leibniz archives is impressive; there is not a scrap of paper he has not read and excerpted. Even so, a fairly careful scrutiny has failed to reveal more than two documents of substantial value, used in this book, which could not already be found in Gerhardt. One is the draft, already mentioned, of a letter to Oldenburg; the other and the more important one (said to contain "a fully rigorous proof" of some of Leibniz' results on integration) is a manuscript of 1676, summarized by D. Mahnke in his Appendix to the 1931 paper of Wieleitner and Hofmann quoted above; for more details, we shall have to await the Academy publication. Anyway, while those documents seem to provide interesting additional touches to the picture, there is nothing in this book to suggest that they change it materially.

But now, not without some reluctance, we must mention what appears to us the cardinal defect of this book. It is true that the *dramatis personae* come out alive; but, alas, the principal character, mathematics, never does;

this is *Hamlet* without the Prince of Denmark. What was Leibniz chiefly proud of as a mathematician? The invention of the differential calculus. In his own career, the prehistory of the subject begins some time before he came to Paris, with amateurish disquisitions into what we call the calculus of finite differences. It continues, after the first contacts with Huygens, with the "arithmetical quadrature", of which more anon; this was enough to impress Huygens (a good judge, and not easy to please), but neither the result nor the method went beyond the general framework of contemporary mathematics. Calculus was born in the notes of 1675 quoted above; it was developed over a long period, extending well into Leibniz' middle age and beyond it; in 1697, for instance, he found the rule for differentiating a definite integral with respect to a parameter, surely a notable discovery. But let us quote Hofmann (p. 294):

> What has induced us to confine the story of the development of Leibniz' mathematics to his time in Paris can be stated in a few words. In those few years he conceived his decisive ideas in mathematics, and brought them to a degree of completeness which allows us to view all his later researches as their offshoots . . . The multiplicity of his new tasks [in Hannover after 1676] makes it quite impossible for him to continue his mathematical researches with the same depth or intensity as before; mathematics . . . becomes a means of mental diversion and recreation, etc.

There are other occasions, too, when we find Hofmann indulging in rhetorical flourishes of debatable value. On p. 249 we are told that Leibniz was never to succeed "in obtaining from Huygens more than his polite agreement" on the merits of the calculus. Is that a fair way of describing an extensive correspondence on this and a host of other topics, which eventually brought from Huygens, on 17 September 1693, this handsome tribute: "your wonderful calculus (*votre merveilleux calcul*) which I can now handle with moderate ease"? On p. 293 a picture is drawn of Leibniz turning into a "recluse" in Hannover ("the graveyard of his hopes and aspirations") "in consequence of the lack of concern, understanding and interest surrounding him". Even the most casual reader of Leibniz' huge correspondence would fail to recognize him there. Actually, even though his irksome official position in that most provincial of capitals was in no way commensurate with his acknowledged talents, he was widely acclaimed as one of the best minds in Europe, traveling to Italy, Berlin, Vienna, actively engaged in a multitude of projects, setting up learned journals, founding academies. A recluse indeed!

Perhaps such extravaganzas are mostly harmless; knowledgeable readers will hardly take them seriously, and one assumes that Hofmann knew better. But when one of them is made to set the tune for the whole book, this is too much.

Who would have believed that a book on Leibniz' mathematics would barely include more than half a chapter on the calculus? Such is the result of Hofmann's misguidedly confining himself to Leibniz' Paris period. Apart from a few pages of little import (pp. 244–247) on some unpublished fragments of 1676, we get no more on that subject than a rather lifeless summary (pp. 188–194) of the notes of 1675. These notes[5] do contain the germs of the calculus, but the germs only. There is nothing in them about the higher differentials and their notation, to which Leibniz attached so much importance; the celebrated formula for $d^n(xy)$ came much later. Above all, throughout these notes, no distinction is made between the notations $\int y$ and $\int y\,dx$ for the integral of y when x is the independent variable. Both are Leibniz' substitute for what had previously been expressed in words as "*summa omnium y, ad x applicatarum*" or more briefly "*summa omnium y ad x*", or again (the independent variable being understood when there is no ambiguity) "*summa omnium y*", and in symbols *omn y ad x* or simply *omn y*. At first $\int$ is introduced merely as a shorter substitute for *omn*, and Leibniz writes $\int x = x^2/2$. On the next page, he sees that there is an inverse operator to $\int$; putting $l = \int y$, he writes at first $y = l/d$, then $y = dl$, seeks rules for the operator d, and finds some. Gradually and after no little fumbling, notations like $\int y\,dy$ creep in when y is not the independent variable. But there is no hint yet of Leibniz' decisive discovery of the invariance of the differential form $y\,dx$ with respect to *all* changes of variable. To us this is an all-important fact, more so perhaps than the so-called "fundamental theorem of the calculus", about which so much ink has been spilled in vain. It comes so effortlessly out of the Leibnizian notation, and that notation has so deeply penetrated our way of thinking, that usually historians fail to take any notice of it. That Leibniz' notation was finally chosen with this fact in mind, and that this happened "not by accident but by an effort of meditation" is shown by his comments in the *Acta eruditorum* of 1686. As Leibniz came to realize, and as he pointed out in his letter of 1680 to Tschirnhaus and elsewhere, Barrow had already had an equivalent result in geometric garb; in our language, it amounts to no more and no less than the general theorem on changes of variable under the integral sign. We may never know just when this basic discovery first occurred to Leibniz; there seems to be no allusion to it before 1680. Certainly there is no trace of it in the notes of 1675, and no hint of

[5] They bear the title "*Analysis tetragonistica*", i.e. *integral* calculus; only later did Leibniz come to refer to his method as *differential* calculus. Surely this is a notable change of emphasis which ought to have been pointed out somewhere.

it anywhere in Hofmann. Had the world possessed no more of Leibniz' work than is described in the present volume, it would have had to wait for another Leibniz to create the calculus.

A good deal is said of Leibniz' and Tschirnhaus' algebraic investigations, which all came to nothing, at least during those years; perhaps in an indirect way they stood Leibniz in good stead when he sought systematically to integrate rational functions, but this was almost thirty years later. There are a number of "flashbacks", intended to illuminate the scene into which Leibniz was stepping as a beginner, and this is as it should be; a long chapter (pp. 63–78) is devoted to Gregory, to Sluse's *Mesolabum*, to the writings of Huygens; but Pascal's all-important *Lettres de A. Dettonville*, which opened up a whole new world for Leibniz, as we know, are disposed of in a few lines (p. 48). One chapter (pp. 101–117) treats in considerable detail the "quarrel over rectification" after telling us that "at first glance it seems only loosely connected with Leibniz' affairs"; so it seems indeed, even after a second glance, since the dispute was between Huygens and the British. What the author means by saying that "it is of the greatest significance" for his story is that it did queer the pitch for Leibniz in his dealings with the English mathematicians, who, by that time, had rightly come to regard him as Huygens' *protégé* and close friend. Such a digression might have been a fortunate one if it had provided us with a good essay on the topic of "rectification", i.e. of arc lengths, in the XVIIth century; this could have shown how the work of many of the best mathematicians of that period gradually clarified the problem, at first by a study of special cases, then in full generality with Heuraet in 1659, until the whole subject was brought to its completion by the Leibnizian $s=\int ds$, $ds=\sqrt{dx^2+dy^2}$. Mathematicians could have found there some food for thought. But, alas, this chapter tells us more about petty quarrels than about rectification. We do get some proofs, or sketches of proofs, by Huygens, Neil, Wren, Fermat; as they are out of context, the temptation is almost irresistible to skip them. Heuraet is lengthily mentioned as a character in the quarrel, but the mathematical content of his paper (perhaps the most decisive contribution to the whole subject) is dismissed in less than two lines. As to the formula $ds=\sqrt{dx^2+dy^2}$, Leibniz was (rightly) proud of it, as appears from his words in *Historia et origo*: "Thus formulas could now express everything which previously required a reference to figures; for $\sqrt{dx^2+dy^2}$ was the element of arc, $y\,dx$ the element of area . . .". It is true that this formula, just as the notation d^2x, $d^3x, \ldots$ for the higher differentials, does not seem to occur in any document within the time-span covered by Hofmann; but one may also doubt whether he truly appreciated its import, since he uses it casually (or rather the equivalent

$$s = \int_0^x \sqrt{1 + (dy/dx)^2}\, dx$$

in his exposition, not only (p. 189) of Leibniz' notes of 1675 where it is merely anachronistic, but also (p. 228) of Newton's letter (to Oldenburg, but intended for Leibniz) of 13 June 1676. Had that letter been as he thus describes it, it would have provided far more damaging evidence against Leibniz, in the dispute on plagiarism, than any that Hofmann is at pains to refute in his book. Of course no such formula occurs there; Newton had indeed long been in possession of substantially equivalent results, but carefully refrains from quoting them, since he would have had to express them in terms of his "fluents" and "fluxions". What Newton does is to mention some of the power series he had derived from those results. As to the formula itself, it is no more, but also no less, than a translation into a definitive algebraic notation of Heuraet's geometric theorem, which was a relation between two curves.

The same narrow outlook mars Hofmann's treatment of "transmutation". In the XVIIth century, this word served to denote the applications of the following principle. Let A, B be two areas or other magnitudes, such as volumes, moments, etc.; assume that they can be cut up into "indivisibles", i.e. infinitely small elements a, b (rectangles at first, then also prisms, triangles, etc); if then $a=b$ up to an infinitesimal of higher order, we say that B is derived from A by transmutation, and we have $A=B$. In a somewhat cruder form, one case of this served as the foundation for Cavalieri's epoch-making book of 1635 on "indivisibles" (and, could he have known it, already for Archimedes' heuristic treatise on *The method*): "transmutation" plays an important role in the work of Pascal, Fermat, Barrow, Huygens; this enabled them to do, in a variety of special cases, what we should do, and what Leibniz soon did, by changes of variable and integration by parts. Reading Pascal in 1673, Leibniz conceived a new variant of this method and applied it with brilliant success to several problems, including what he called the "arithmetical quadrature of the circle"; by this, he meant the power-series for arc tan x, and its celebrated special case, the series $1-(\frac{1}{3})+(\frac{1}{5})-\cdots$ for $\pi/4$. This was at the time a notable advance over Mercator's "quadrature of the hyperbola", i.e. the power series for $\log(1+x)$; but it was still quite in the geometric spirit of Leibniz' great predecessors. Always on the look-out for underlying general ideas, he soon conceived a remarkably broad formulation of the transmutation principle, which he described eventually in his answer of 1676 to Newton; regrettably, this is barely hinted at in a few lines (p. 235) in Hofmann's paraphrase of that letter. By that time

Leibniz had ceased to attach any great value to it, whatever he may have pretended in serving it as a kind of warmed up dish to the English mathematicians. He was already in possession of the first elements of the calculus; but Newton was not disclosing his secrets, and Leibniz saw no reason for being more open. He merely staked his claim in the final words of the letter, with a noble conclusion which we will quote in full: "In connection with centers of gravity, I have discovered a singular approach to altogether new considerations which promise to be of great use in geometry as well as mechanics. When (God willing) I shall have brought these to their conclusion, my task will be to devote the rest of my life (or as much of it as it will be permitted to me to spend on philosophical meditations) to the investigation of Nature."

That is the story of the Leibnizian transmutation. In his *Historia et origo*, he explains how he soon desisted from this line of investigation "after discovering that it had been developed, not only by Huygens, Wallis, Wren, Heuraet and Neil, but also by Gregory and Barrow"; "nevertheless", he says, "it did not seem useless to describe it, in order to make clear by what gradual steps access was gained to bigger things". A moment later, when he comes to the "arithmetical quadrature", he begins by explaining it in its original geometric form, but soon, as we might do ourselves, drops into the language and notation of the calculus. At any rate he does not fall into Hofmann's mistake of mixing up inextricably one notation with the other; nor does he try (as Hofmann does) to create the impression that his use of transmutation was essentially different from the use others had made of it before him. As he says, he could well have derived some of his inspiration from Barrow, had he read him at the right moment; there is no point in disputing this fact. He could have; but he says he did not; so he did not, and that is all. As to the arithmetical quadrature, and particularly the series for $\pi/4$, this was new indeed; Huygens congratulated him, saying that this was "une propriété du cercle très remarquable, ce qui sera célèbre à jamais parmi les géomètres". But the bigger things were yet to come.

Another serious disappointment is in store for the reader, especially if he knows of Hofmann's early collaboration with Wieleitner on Leibniz' calculus of finite differences. In the *Historia et origo* and elsewhere, Leibniz never tires of explaining how finite differences were at the source of his calculus. The analogies between the two topics, or rather (in view of his way of looking at them) their substantial identity, appear to have been at all times at the very center of his thoughts on this subject. In one case the differences are finite, while in the other they are infinitely small; more precisely, the differences of each order are so with respect to those of lower order. The Bernoullis and Euler did not view the matter differently. In a different context, those same analogies also played a role in English mathematics; the possibility of some cross-fertilization at a later stage cannot be excluded. In view of Hofmann's previous work, one expected this theme to be displayed prominently in his book; but it is nowhere mentioned. There is only the most casual reference to finite differences in Chapter I, where it leads up to Leibniz' early (and not particularly deep) work on series. There is not a word (if we are not mistaken) to suggest that there is a connection between this and the differential calculus.

Where then is the grand synthesis? The grand synthesis remains Leibniz' own *Historia et origo*. If an ampler one is ever to be written, it will require deeper mathematical insight than is displayed in the present volume, and it will have to deal with a far broader segment of Leibniz' mathematical career; we have given some examples to show how severely our author has been hampered by his self-imposed limitation to the Paris period, and we could easily have adduced some more.

Nevertheless, this book is not only a labor of love, but also a solid contribution, honestly written, generally reliable and thoroughly documented, to a fascinating subject. If it fails to capture (as the author had hoped) the essence of the original texts to be included in the Academy volume, it will provide for the novice a useful companion to that volume. Many readers, only too ready to skip mathematical passages, can find here the characters and atmosphere of an exciting moment in the history of science. If this sounds like faint praise, let the reader reflect how few are the books nowadays that would deserve it.

As to the presentation, the German edition was a well-printed but unassuming small volume of 252 pages brought out in conformity with the austerity standards of the early post-war years in Germany. Somehow one was reminded of the distressing circumstances under which the book had been written; this had an endearing effect of its own. The English translation makes its proud appearance as a fine volume of 372 pages; the enlarged size is due in part to the added footnotes, but chiefly to the more generous style of printing. The presentation and typography are what one has come to expect of the Cambridge University Press; need one say more? But . . . .

But the price is \$23.50. No doubt, inflation must take its toll. Still, at such a price, one cannot avoid the question: to whom should one recommend buying the book? Libraries, of course. Leibniz scholars; but surely most of them already own the German edition and may well find that the added footnotes hardly justify the expense; for the others, the book provides, in a convenient arrangement, many details not easily assembled otherwise, and excellent indices, of great value as long as the Academy volume is not yet out. Those who are looking primarily for a history of major mathematical ideas will find little here to whet their appetite and less to satisfy it. Many others can read it with pleasure and profit, skipping what deserves to be skipped, and (we fear) maybe more. Most of them, perhaps, will choose to wait for the book to reappear as a *Penguin*, with much of the mathematics, some of the ballast and all the indices lifted out. When that happens, one may hope that it will have a wider audience.

A. WEIL