E R R A T A.

p. 19, 16th line, 2nd column. For "simple", read "single".

" 28, 56th line, 1st column. For "accepted", read "accidental".

" 33, 30th line, 1st column. For "described", read "ascribed".

" 37, 8th and 9th lines, 1st column. The parenthesis should extend to, and include, the word "triforium".

" 37, 41st line, and column. For "clerestory", read "triforium".

" 40, 20th line, 1st column. For "17 ft.8 inches", read "14 ft."

" 43, 59th line, 1st column. For "mximum", read "maximum".

ARCHITECTURAL REFINEMENTS.

A REPLY TO MR. BILSON.

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THE purpose of this Paper is to make some reply to an article by Mr. John Bilson which appeared in the June number of this Journal for 1906.

The exact title of Mr. Bilson's article might not appear to have serious importance in the controversy which it has opened up, but it so clearly indicates the bearing of his argument, as expressly rehearsed by himself, that I shall not hesitate to call attention to its wording, and especially to its quotation marks. The title runs as follows: "Amiens Cathedral and Mr. Goodyear's 'Refinements': A Criticism."

When the quotation marks of this title are related to the opening and conclusion of Mr. Bilson's Paper, it will be found that they imply not only that there are really no refinements at all in the Amiens Cathedral, as supposedly observed by me, but also that there are really none at all in any of the other cathedrals and churches which were represented in the Edinburgh Exhibition of Architectural Refinements.*

The reception accorded this exhibition is stated by Mr. Bilson to have inspired his Paper. He is careful to point out that the list of patrons does not in itself furnish any indication of the opinions of such patrons about the asymmetries which were represented by the exhibition, which must certainly be admitted. He is careful to raise the question (but careful not to decide it) whether the success of the exhibition may not have been, on the whole, a *succès d'estime*; but he nevertheless confesses that "some of our Scottish colleagues have received its teaching with something like enthusiasm," and he consequently feels called upon to ask "whether we are justified in accepting such teaching." His conclusion is an emphatic negative, but just what the teaching is which we are not to accept Mr. Bilson does not state accurately or carefully, excepting that it is represented by this Brooklyn Museum loan of exhibits at Edinburgh which Mr. Bilson does not appear to have seen, or if he has seen them he is careful not to mention it. Anything, however, which Mr. Goodyear has published or done in architectural research may be safely discarded, in Mr. Bilson's opinion, as unworthy of notice, and is consequently fit to be labelled by that most deadly and exquisitely contemptuous anathema which is conveyed by the use of the quotation marks in the title of the article.

Mr. Bilson is also careful to mention "the large number of churches" covered by my investigations as making it "difficult to test" my "theories," "or to arrive at a definite and final summing-up of the whole case in a short general Paper." Mr. Bilson has solved this difficulty by the use of quotation marks in his title. Otherwise, and aside from the contemptuous opinion which is conveyed by the quotation marks, he explicitly states that on account of "the large number of churches covered by his investigations . . . I do not propose to roam at large over the entire area of review," but none the less he says: "The object of this Paper is to give a 'definite answer' to these questions," viz. "whether we are justified in accepting such teaching [as that of the Edinburgh Exhibition], or am I right in believing that the chief value of Mr. Goodyear's investigations will be found in the fact that they repeatedly afford in themselves evidence in disproof of the theories which he bases upon them?" This "definite answer" will not, however, roam over "the entire area of review." "I shall confine myself entirely to what Mr. Goodyear has written about the churches of Northern France and almost entirely to a single typical cathedral."

Mr. Bilson has thus made things easy for himself, but he is not willing to make them correspondingly easy for me. He is disposed to limit his field of inquiry, but he is not disposed to limit the sweeping character of his conclusions and condemnation.

He has so strictly kept to his plan of limiting his own "area under review" that seventeen pages are devoted to Amiens Cathedral, while three pages are devoted to a debate about deflected plans and to brief notes on a very few French churches outside of Amiens. One page is devoted to his preface, from which I have quoted.

He has so closely kept to his plan of limiting his own "area of review" that not one Italian building is even mentioned, and that not one of my publications on Italian churches is quoted, either in footnote or otherwise. As far as Mr. Bilson's Paper is concerned no one would know

* Opened by the Edinburgh Architectural Association in the Scottish National Portrait Gallery, 6th September 1905, closed 11th November.

that I had ever surveyed or photographed or published an Italian building.

This is the method of "definite answer" to the teaching of an exhibition which included only seven French churches, as against forty-five Italian churches, three churches in Constantinople, one in Germany, and six ancient temples. This is the method of "definite answer" to the teaching of an exhibition which allotted 41 photographs to France, and which allotted 226 photographs and surveys to Italy, which allotted 3 photographs to Amiens, and which allotted 42 photographs and surveys to the Pisa Cathedral.

The omission of *seventeen* of my most important publications on architectural refinements from Mr. Bilson's own perusal, mention in text, or footnote bibliography, is a curious and almost incredible illustration of his temper and attitude. Those omitted are the ones referring to Italian and classic buildings. On this head the bibliography of the Edinburgh Catalogue may be compared with Mr. Bilson's alleged bibliography, which is given in one of his footnotes (p. 400). He does not expressly say that he has quoted all my publications on refinements, but he very expressly leaves it to be inferred and understood that he has quoted all which need be examined before giving a "definite answer" to the teaching of the Edinburgh Exhibition.

This assumption is preposterous. If a critic were desirous of testing the general value and character of my investigation by confining his attention to a single typical building, fairness and common sense would suggest the choice of the building which I have most carefully surveyed and described, and which had the largest number of exhibits at Edinburgh. This is the Pisa Cathedral, which was represented by forty-two exhibits, as just mentioned. No other mediæval building has ever been so carefully surveyed, and Mr. Bilson's failure either to mention any of the Pisan exhibits at Edinburgh or to mention any of the publications in which they were reproduced and carefully described is an inexplicable omission in view of his sweeping conclusions.

Another reason suggesting this choice of the Pisa Cathedral is that my investigation of mediæval architectural refinements was originally suggested by this cathedral and originally began there (in 1870). Not only was the Pisan Romanesque the actual point of departure for my own studies in this special investigation, but it must always remain the logical point of departure for every systematic examination of mediæval asymmetry, because the material is far richer and the phenomena are far more numerous than elsewhere.

Moreover, the argument for the historic derivation of certain mediæval refinements from those of antiquity can only be presented through the Italian buildings which show marked Græco-Byzantine influence, and these are notoriously, and above all others, the Pisa Cathedral and St. Mark's at Venice.*

Any critic is privileged, of course, to select any church as a subject of debate, and no one could complain that Mr. Bilson's criticism has selected the Cathedral of Amiens as a matter for discussion; but it is a very sad indication of his prejudice and bias, and of his want of discretion, that he should be willing to base a wholesale condemnation of the given architectural investigation on the assertion that I have made a mistaken observation in one cathedral. This is, however, his avowed position, for Mr. Bilson assures us, in his closing and final sentence, that "when the other French churches investigated by Mr. Goodyear have been the subject of monographs as exhaustive as that of M. Durand at Amiens, there will be no difficulty in producing equally conclusive proof that their 'refinements' exist only in Mr. Goodyear's imagination." Certainly there will be no difficulty, as far as Mr. Bilson is concerned, even in advance of the happy day when every church has its own monograph in two enormous volumes. There will be no difficulty, because there has been no difficulty. Mr. Bilson's word is as good as his bond. He knows very well that these monographs will never be written, and by assuring us of what would happen if they were written he evidently makes it unnecessary, for his special purpose, that they ever should be written. Thus does a single good monograph serve the cause of truth and of Mr. Bilson for a large number of churches. These include the Edinburgh exhibits for forty-five churches in Italy and the seventeen publications about them which Mr. Bilson did not think it worth while to mention, because he has heroically resolved to confine himself to the French churches and "not to roam at large over the entire area."

If the distribution of the various classes of phenomena so far observed were asserted to be locally uniform and regular throughout mediæval work; if the mediæval churches were asserted to exhibit generally an equally large variety of phenomena when one cathedral is compared with another; and if the churches of France had been as widely and as thoroughly examined as those of Italy, Mr. Bilson's summary method of limiting his area of inquiry without limiting his area of conclusions might have some show of reason. As matters actually stand,

* The accidental destruction of most of the blocks belonging to the *Architectural Record Magazine* (in which the Pisa Cathedral surveys were originally published) made it impossible to illustrate these surveys in the pages of the *Edinburgh Catalogue*. The Catalogue illustrations offered a partial memento of the exhibition to those who had seen it, but they were actually a make-shift choice in consequence of the wholesale destruction just mentioned, and very poorly balanced in consequence, relatively overnumerous for some particulars and wholly deficient for many others. It need hardly be pointed out that a catalogue cannot be expected to take the place of the exhibition to which it is a guide.

his method has no show of reason. Certain classes of phenomena appear to be strictly limited, even within the area of Italy, to the Pisan Romanesque. Outside of the Pisan Romanesque the variety of phenomena is much greater in the Romanesque of Italy than it is in the Italian Gothic. These matters have been carefully explained in the publications which Mr. Bilson has refrained from mentioning, and which he has possibly refrained from reading. On the other hand, in the publications for the Gothic of Northern France which Mr. Bilson has formally quoted, it is expressly explained that the French Romanesque of the south and centre has not been examined. It has also been expressly explained that the time occupied in Northern France was necessarily limited to eight weeks in 1903 (one week in 1905), during which short time 330 photographs were taken and other classes of surveys were correspondingly limited.* It has also been expressly explained that in the Gothic cathedrals of Northern France only one class of phenomena has, generally speaking, been noticed, and that the exceptions observed were almost wholly limited to one church—viz. Notre-Dame at Paris—in which a much greater variety of phenomena were found.

In face of these repeated and time-worn explanations, it is not an encouraging indication of Mr. Bilson's acumen or grasp of the subject-matter which he is attempting to handle, to find him insisting that the Amiens Cathedral, as a "typical" cathedral, ought to show all the refinements, and that if it does not, this is an argument against the general results of the investigation. Mr. Bilson can hardly be aware that the temple of Phigalia and the Parthenon were built by the same architects, Ictinus and Callicrates, and that the temple of Phigalia is destitute of the Greek refinements, as is also the Erechtheum on the Athenian Acropolis. If such a remarkable contrast of practice existed within the limits of Greek art and within the individual practice of its most famous architects, why should it not occur also in mediæval work? Mr. Bilson's equipment as a debater may thus be argued from his wholly inadmissible position that if all the refinements are not found in all the mediæval buildings, or in all the important ones, they cannot really exist in any. On the contrary, if the Pisa Cathedral exhibits phenomena which cannot be found in Notre-Dame, and if Notre-Dame exhibits phenomena which cannot be found at Amiens, this is good corroborative evidence for other proofs that their phenomena are not imaginary or accidental. Our reviewer, however, ridicules the idea that Notre-Dame should have refinements which cannot be found at Amiens. My own suggestion (aside from the one which he quotes, and which I do not retract), would be that Notre-Dame stands in much closer relations to the Romanesque, to which several of the refinements observed in Notre-Dame appear to be confined, when the Italian buildings are included in the comparison of phenomena.

We are thus led to point out that Mr. Bilson's self-set limitations in his effort to give a "definite answer" and a black eye generally to the teaching of an investigation which he appears not to have thoroughly examined, and in many particulars not to have examined at all, have resulted in an absurdly incorrect and deficient summary of the phenomena which he has attempted to classify.

Thus Mr. Bilson's preface continues, from the sentence last quoted about limiting himself to "the churches of Northern France and almost entirely to a simple typical cathedral," as follows:—

"The irregularities which Mr. Goodyear claims as intentional 'refinements' may be roughly divided into two classes:

"1. Obliquity of alignment in plan, including want of parallelism in walls and piers, deflections of axis, curves of alignment, . . . and asymmetric plans generally.

"2. Walls, piers, and columns out of plumb, sometimes straight, but more generally described as 'vertical curves.'

"I do not suggest that this classification includes all the irregularities which Mr. Goodyear believes to be refinements, *but it covers the great majority of the examples noted by him in his publications*, and those which it does not include are not material to the examination which is the immediate object in view." *

Now the "examination which is the immediate object in view" has been explicitly announced by Mr. Bilson as intended to give a "definite answer" to the question "whether we are justified in accepting" the teaching of the Edinburgh Exhibition. This being so, let us inquire, for example, of Mr. Bilson how far his classification covers the forty-two Edinburgh exhibits for the Pisa Cathedral, as also carefully described in the various publications of the *Architectural Record Magazine* which Mr. Bilson has failed to mention.

The *Building News and Engineering Journal* of 22nd September 1905 says of these Pisan exhibits, which the editor had personally examined in Edinburgh: "In Pisa, particularly, the idea of creating false perspective effects seems to have taken full possession of the architect. The divergences, particularly those of elevation, have evidently been carefully devised with a definite object—that of making a small building appear to be imposing—and they have been successful." Let us now inquire what other churches, as represented in the Edinburgh Exhibition, may fall outside of Mr. Bilson's classification, as also representing perspective illusions. In the index of the *Edinburgh Catalogue* I find that eighteen churches, out

* The above matter was written before the recent survey work of this year was undertaken in Northern France.

* The italics are mine.—W. H. G.

of forty-five Italian churches—that is, just about one-third of all the buildings in the entire exhibition—were selected to represent perspective illusions. (See "Perspective Illusions" under "Index of Exhibits according to Subject-matter," p. 150.) The index of the *Edinburgh Catalogue* does not therefore appear to be in harmony with Mr. Bilson's assurance that his classification "covers the great majority of the examples" noted by my publications. One of the most important of my publications which he therefore appears to have overlooked or to have forgotten is "Perspective Illusions in Mediæval Italian Churches," in the *Architectural Record*, vol. vi., No. 1 (1896). This publication is quoted in the Edinburgh bibliography which Mr. Bilson must be presumed to have read. My original announcements on this subject were made in *Scribner's Magazine* for August 1874, under the title "A Lost Art." Favourable critiques of these observations were published by G. A. T. Middleton in the *Nineteenth Century Magazine* for March 1897, "Deliberate Deception in Ancient Building"; and by C. J. MacCarthy (City Architect of Dublin) in a Paper read before the Royal Institute of the Architects of Ireland, and published, with numerous illustrations, in the *Irish Builder* of 1st February 1899, under the title of "Intentional Irregularities in Mediæval Italian Architecture."

The existence of perspective illusions in mediæval architecture has also been formally accepted and affirmed by the high authority of Choisy and Enlart, as well as by the *Dictionary of Architecture* published by the Architectural Society.* Inasmuch as one-third of the buildings in the Edinburgh Exhibition were devoted to perspective illusions, it would certainly appear that Mr. Bilson has not given me what Mr. Roosevelt would call "a square deal." My publications on this subject long precede those of the French and English authorities quoted, my earliest one dating back to 1874, as just mentioned. My publication of 1896 on this subject also precedes the mentions of Choisy and Enlart. It was the formal opening of my general report on the Brooklyn Museum surveys of 1895, and immediately followed the article giving an introductory summary account of the results of that expedition. The Italian examples described and surveyed by me are far more numerous than those quoted by the French authorities mentioned. Their instances, however, are found in France (with one exception), and yet Mr. Bilson's title does not shun the designation and quotation marks of "Mr. Goodyear's Refinements," with which he should hardly venture to publicly insult his French colleagues.*

Mr. Bilson is undoubtedly thoroughly familiar with the quoted publications of MM. Choisy and Enlart, especially as his own work at Durham has been so much praised by the latter authority. He also pretends to be conversant with my own writings. Would it not, therefore, have been a fairer and wiser procedure if Mr. Bilson had included in his classification of "Mr. Goodyear's Refinements" those which had been independently announced by such high authority? But the tale of Mr. Bilson's delinquencies as a summariser and classifier is not half told as yet.

It has been pointed out by many students of my work that Mr. Ruskin is my predecessor in a very important class of observations, and I have always been ready to acknowledge this precedence, and have always been anxious to call attention to it.† This class of observations is also ignored by Mr. Bilson, who, notwithstanding, persists in speaking as though he knew my publications, and were qualified to summarise them for the benefit of the British public. Once more it will be noticed that the omission relates to a class of phenomena which have also been observed and announced by an authority of distinction. That the relation asserted to exist between Mr. Ruskin's work and one phase of my own is not imaginary may be best shown by quotations from other writers. For instance, Professor Charles Eliot Norton, Emeritus Professor of the History of Art in Harvard University, was a personal friend of Mr. Ruskin, and has published his correspondence.‡ Professor Norton said of my quoted publication of 1874 in *Scribner's Magazine* that it was "the most important contribution to the topic since Ruskin wrote *The Seven Lamps of Architecture*." § Macmillan's *Dictionary of Architecture and Building*, edited by Dr. Russell Sturgis, opens its article on "Refinements in Design" as follows: "With regard to mediæval buildings, the existence of apparently deliberate irregularities in measurement was pointed out by Ruskin in the *Seven Lamps of Architecture* and in the *Stones of Venice*, and Viollet-le-Duc in the *Dictionnaire de l'Architecture Française*, s. v. Trait (vol. ix.). There has been, however, no such comprehensive investigation as that undertaken by Professor W. H. Goodyear, of which the results were

* Choisy, *Histoire de l'Architecture*, vol. ii. pp. 184, 410 (1899); Enlart, *Manuel d'Archéologie Française*, p. 58 (1904); *The Dictionary of Architecture*, published by the Architectural Society, London, vol. v. p. 18, under "Optical Refinements" (no date, about 1882). Choisy quotes Arles, Civray, Payerne, Poitiers, Montréal, Orbais, Rheims, S. Maria Novella, Florence. Enlart quotes Nesle, Boulogne, Civray, Poitiers, Montréal, Orbais, S. Maria Novella. *The Dictionary of Architecture* quotes Saint-Gilles, Erbach, S. Maria Novella.

* Mr. Bilson is also careful to distinguish between what he calls "real refinements," viz. those observed at Amiens by Viollet-le-Duc, and those observed by me.

† *Edinburgh Catalogue*, p. 20.

‡ Ruskin, John: *Letters to Charles Eliot Norton*, 1904, 2 vols. Houghton, Mifflin, & Co.

§ *Church Building in the Middle Ages*, by Professor Charles Eliot Norton, p. 321 (1880).

published in part in the *Architectural Record* (vols. iv.–vi., vii., ix. New York)." *

These two references may be sufficient to indicate that Mr. Ruskin first observed a class of phenomena to which I have made wide additions; and as Mr. Bilson's classification palpably omits these phenomena, I am obliged to indicate them myself—first, by quoting from Mr. Ruskin; secondly, by mentioning the number of churches and exhibits shown at Edinburgh for these phenomena; and thirdly, by mentioning the special publications in the *Architectural Record* which were devoted to them.

In the *Lamp of Life* Mr. Ruskin speaks of "accidental carelessness of measurement being mingled indistinguishably with the purposed departures from symmetrical regularity. . . . How great, how frequent, they are, and how brightly the severity of architectural law is relieved by their grace and suddenness, has not, I think, been enough observed." After a series of observations he continues: "I imagine I have given instances enough, though I could multiply them indefinitely, to prove that these variations † are not mere blunders nor carelessness, but the result of a fixed scorn, if not dislike, of accuracy in measurements; and in most cases, I believe, of a determined resolution to work out an effective symmetry by variations as subtle as those of nature."

In my article on "Constructive Asymmetry in Mediæval Italian Churches," which is ignored by Mr. Bilson,‡ I have published and analysed a series of designed irregularities in the measurements of arcades in Italian churches, similar to those observed by Mr. Ruskin, as follows: Palaja (near Pontedera), interior arcades of the nave; S. Michele ai Scalzi, Pisa, arcades of the façade; S. Paolo, Ripa d' Arno, Pisa, the north wall; Cathedral of Pisa, the north and south walls; Cathedral of Prato, the south wall; Cathedral of Troja, the south wall; interior arcades in the Cathedrals of Siena, Cremona, and Fiesole; interior arcades in the Churches of S. Maria and S. Pietro, Toscanella, of S. Nicola at Bari, of S. Maria della Pieve at Arezzo, &c.

These were all represented by surveys in the Edinburgh Exhibition, except Palaja and the façade of S. Michele ai Scalzi, and two additional churches were included there for designed irregularity of exterior arcading, viz. the churches of the Pieve Vecchia and Pieve Nuova at S. Maria de' Giudici, near Lucca. Where the above summary quotes buildings which also exhibit perspective illusions, or oblique plans, it will be understood that none have been mentioned which have not been expressly published as also exhibiting arcadings which are demonstrably irregular by intention, aside from the perspective illusive arrangements or aside from obliquity. For example, the arcades at Fiesole descend toward the choir three feet, and the spacings narrow toward the choir eight feet; and in that sense the irregularity produces perspective illusion, but the church is quoted in the above list because "both arches of the second bay are higher than the first, both arches of the third bay are lower than the second, both arches of the fourth bay are higher than the third, both arches of the fifth bay are lower than the fourth, both arches of the sixth bay are higher than the fifth, both arches of the seventh bay are lower than the sixth." * S. Michele ai Scalzi has an interior perspective illusion † but it also has the façade measurements to be presently described.

My general conclusion drawn from the comparison of arcadings was as follows, and corresponds with Mr. Ruskin's in result, if not in method: "From the facts so far brought out in this part of the argument it appears that a purpose can be proven in many irregular arrangements of the Italian Romanesque, first by showing that correspondences of irregularity can be used as proof of intention; secondly, by showing that there are means, in many cases, of fixing a limit of error due to accident; thirdly, by urging the point that we cannot admit the purpose of definite schemes in one part of a church and assert at the same time that the builders did not know what they were doing in another part. We have, for instance, such proofs of the use of the same definite schemes in both gallery levels of the Pisa Cathedral in some cases as to indicate that a different scheme was purposely employed in each gallery in other cases." ‡

For the nature of the evidence otherwise submitted I will only quote the churches at Palaja and of S. Michele ai Scalzi at Pisa. Of Palaja I wrote in 1897: "The perspective deceptions so far quoted in the preceding issue bear, on the face of things, that evidence of design which is furnished by a scheme, *i.e.* by an arrangement of measurements which the law of chances would lead us to suppose could not be accidental in one case, and which certainly could not be accidental when found in a series of repetitions. But evasions of regularity were also practised from a definite artistic feeling and purpose, and generally without the design of obtaining an effect of dimension by palpable trickery. Here is an illustration from the basilica at Palaja. The measures taken to centres for the interior pier spacings, in metres and centimetres, on one side of this church, beginning at the entrance and moving toward the choir, are as follows: 4·92, 4·87, 5·11, 4·86, 4·92. These measures

* None of these volumes, containing eleven articles, are quoted by Mr. Bilson, who refers only to the later articles on French cathedrals in vols. xvi. xvii.

† Viz. those which he has carefully described.

‡ *Architectural Record*, vol. vi. No. 3 (1897).

* *Architectural Record*, vol. vi. No. 2 (1896), one of the publications ignored by Mr. Bilson.

† Exhibit 119, *Edinburgh Catalogue*, and *Architectural Record*, vol. vi. No. 3, fig. 2.

‡ *Architectural Record*, vol. vi. No. 3. Publication ignored by Mr. Bilson.

were taken hastily, and yet the first bay and the last bay tally exactly; the measurements next adjacent tally within a centimetre. The middle bay is largest by 24 centimetres, or, say, 10 inches. We will not debate the purpose at present. The proof is the proof of intention. Where is the objector who will say that the larger arch is due to careless building when the measures tally within a centimetre on either side of it?" Of S. Michele ai Scalzi, which has a typical Pisan-Romanesque façade, I wrote as follows: "The measures for the arcades of the exterior façade are here given in metres and centimetres: 2·27, 2·36, 3·33, 2·36, 2·30. The centre measure represents the arcade of the doorway. On either side of it the arcades diminish in span in corresponding gradation." These measurements prove that an allowance of 3 centimetres, or a little over one inch, will represent the amount of error here due to carelessness. They also indicate a purposed grading of dimensions in corresponding pairs away from the central arcade, showing the same kind of feeling which Mr. Ruskin long since instanced in his measures for the Pisa Cathedral façade.

Without mentioning particulars in other instances I will simply say that there were exhibits for not less than fourteen Italian churches in the Edinburgh Exhibition which represented this particular class of intentional asymmetries which is omitted from Mr. Bilson's classification. This number of churches is double that of all the French buildings, and one-third that of all the Italian buildings, which were represented in the exhibition. However, we have already found Mr. Bilson assuring his readers that *his* classification "covers the great majority of the examples noted" in my publications, "and those which it does not include are not material to the examination which is the immediate object in view." The "immediate object in view" is avowedly and obviously to discredit the Edinburgh Exhibition. Whether fair means have been employed to this end may well be left to the judgment of the impartial reader.

It is to be remembered that the series of *Architectural Record* articles from which I have been quoting constituted a preliminary report on the Brooklyn Museum surveys of 1905, covering a period of six months' work in Italy, during most of which time I had the assistance of a thoroughly qualified architectural surveyor and expert in engineering construction. Consequently the order of arrangement of subject-matter in which these articles appeared may be considered significant for the order in which I desired to lay these observations before the expert public. It appeared desirable to me, at that time, to publish first the phenomena which were the least open to debate, either on account of inherent probability, or on account of weight of demonstration, or on account of the support to be obtained from the independent similar announcements of other authorities; for what two or more observers have independently remarked has more weight than what only one observer has announced. Hence it is significant, for my own view of these matters, that the original *Architectural Record* articles on the surveys of 1895 were prepared and arranged in the order to which I now call attention. The articles were, namely, so arranged as to place in the front of the argument the evidence for perspective illusions, because it is largely derived from palpably schematic arrangements, and the evidence for intentional departures from exact symmetry without perspective illusions, because it is largely also derived from palpably schematic arrangements, either in corresponding parallel pairs of measurements (as at Fiesole) or in a balance of measurements moving from a common centre, as found at Palaja or in S. Michele ai Scalzi.

I am therefore pointing out that Mr. Bilson's classification of the Edinburgh Exhibition is not only absurdly deficient and inaccurate as to matters of fact, but it is also unjustly deficient and inaccurate in omitting the classes of facts which are most easily demonstrated, and which are substantiated by the independent announcements of other high authorities. I am referring now not only to Mr. Ruskin, but also to another high authority, for in a very large series of observations I have taken ground which was not only anticipated by Mr. Ruskin, but which has independently been taken by M. Auguste Choisy. Moreover, in the *Edinburgh Catalogue*, I expressly quoted the stand taken by M. Choisy as corroborating my own in the given field of observations—of course without making him in the least degree responsible for my own particular illustrations.*

I shall now quote M. Choisy still more fully on this head in view of the challenge conveyed by Mr. Bilson's quotation marks and peculiar title, and the assertion therein conveyed that I stand alone in all the positions I have taken.

Following his matter on perspective illusions M. Choisy says: "*Asymmetrics.* Architects who analysed with this refinement the play of perspective must have held cheap the symmetrical combinations which are nullified by perspective, and which are complicated by the unceasing play of light and shade. The law of symmetry as we understand it now, and which consists in reproducing on the left the arrangements on the right, this rather narrow rule, plays a very secondary rôle in the Middle Ages. On this head, as in so many others, the point of view of the Gothic builders was that of the Greeks (vol. i. p. 414). Asymmetry appears acceptable as soon as an evident reason justifies it. If an edifice is placed in an enclosure the plan follows that of the enclosure. Two spires are successively erected, architecture has progressed meantime, and all the improvements are accepted in the new construction although a contrast is involved. *Generally speak-*

* *Edinburgh Catalogue*, p. 21.

*ing, the architects of the Middle Ages avoid formal regularity.** If they admit a symmetrical result in the total effect they know how to avoid monotony by details which are infinitely diversified. Notre-Dame has on the façade three portals erected at one time; from left to right only the effects of the masses are balanced, while each one has a character of its own. These differences give to the composition a charming variety; a sympathetic feeling attaches us to these works in which the designer has disdained the expedient of a set pattern, in which each part has cost a separate study, an individual execution; instead of symmetry we have balance, and the unity of impression does not suffer." †

This philosophy of the subject of mediæval asymmetry is an extremely apt statement of the point of view that both accidental and designed irregularities represent a single virtue, and that acceptance of accidental and local variations in some cases is a phase of the artistic spirit which purposely designed such variations in other cases. This is also the ground taken by Mr. Ruskin, whereas the antagonists of the belief in purposed variations appear to consider that every case of accidental variation constitutes an argument against design in any other instance. At all events prejudice, incompetent grasp of the subject, and slovenly examination of it have drawn Mr. Bilson into a very misleading analysis of the Edinburgh Exhibition. In the preface of the Catalogue, as well as in the body and quotations of its text, I have announced the point of view which is supported by the exhibits I have now described, and by the quotations which I have now repeated and enlarged.

Since Mr. Bilson constantly refers to my "theories" throughout his article as though they were peculiarly abnormal and otherwise wholly unheard of, I have thought it worth while to show that he has had some opportunity, which he has not made use of, to ascertain that this is not entirely the case. He says that "as a result of this exhibition (at Edinburgh) there now seems to be a disposition in some quarters to hail Mr. Goodyear's supposed discoveries as having an extremely important bearing on the history of architecture." May not this disposition, as far as it exists, be due to the fact that these "supposed discoveries" are in some particulars thoroughly in line with the observations of other authorities, and that, in other particulars, although they are an advance beyond these observations, they are an advance in the same general direction? An extraordinary feature of Mr. Bilson's article is his predetermined oversight of all the publications and of all the Edinburgh exhibits in which my "supposed discoveries" have had the ratification of subsequent or previous independent announcement on the part of standard authorities.

For instance, one of the *Architectural Record* articles which was written to report on the results of the Brooklyn Museum surveys of 1895 was entitled "A Discovery of the Entasis in Mediæval Italian Architecture."*

The Edinburgh "Index of Exhibits according to Subject-matter" specifies four buildings for the entasis. The *Architectural Record* quotes other examples. The matter was presented as relating directly to the origin of the vertical curves against which Mr. Bilson's satire and satirical quotation marks are specially directed. As far as I am aware no observations on the mediæval columnar and pilaster entasis had otherwise ever been published up to 1897. But in 1904 M. Camille Enlart tells us that "les fûts galbés sont restés fréquents dans les Écoles Germanique et Lombarde. On trouve des exemples de cette forme dans les régions de la France qu'elles ont influencées."†

Mr. Bilson has received such flattering treatment at M. Enlart's hands (for the Durham observations) that we are disposed to believe that he has read his book. The conundrum which we cannot answer is this. Since the mediæval entasis is directly connected with the question of the vertical curves, which Mr. Bilson has selected for his *pièce de résistance*, and since it has been treated at length by me as a step towards an explanation of the vertical curves, as being derived from an entasis in pilasters,‡ did Mr. Bilson omit the mediæval entasis from his classification and argument because M. Enlart had independently verified its existence, or did he omit it because he did not know that it is one of my "supposed discoveries"? If the latter is the case, how does it happen that he did not know it? The second conclusion would be the more flattering to Mr. Bilson as a man of honour, but it would be less flattering to him as a careful student of the subject which he has undertaken to discuss.

While I am more than prepared to meet Mr. Bilson on his self-chosen battle-ground of deflected plans and vertical curves, and while I realise deeply the importance of meeting him squarely and fairly on the questions raised for the Amiens Cathedral, I must, in the first instance, protest, by the most emphatic, explicit, and carefully detailed explanation of which I am capable, against his preposterous assertion that his argument on the Cathedral of Amiens covers all the other French churches which I have examined, and that his argument on the French exhibits covers the ground of the Edinburgh Exhibition.

Mr. Bilson's method of procedure reminds me of the emperor Caligula, who wished that all his

* The italics are mine.—W. H. G.

† Translated from *Histoire de l'Architecture*, ii. 411–12.

* Vol. vii. No. 1; quoted in the *Edinburgh Bibliography*; not quoted by Mr. Bilson.

† *Archéologie*, i. 326, with a list of instances; another allusion at p. 23.

‡ *Edinburgh Catalogue*, pp. 9, 10; *Architectural Record*, vol. vii. No. 1 (1897).

enemies had but one neck in order that he might behead them with a single blow. To quote the exact words of Mr. Bilson: "I now turn to the Cathedral of Amiens, which I have selected for a detailed examination of Mr. Goodyear's observations and theories."

A very considerable ignorance of these observations and theories looms from that sentence, and inevitably pervades the article, which turns on the axis of such a preamble, and this ignorance I have just instanced for three notable classes of phenomena.

I therefore propose to examine quite thoroughly Mr. Bilson's equipment for his undertaking to make a "detailed examination of Mr. Goodyear's observations and theories" by the extraordinary method of testing them in block on a cathedral in which, according to the consent of all parties, only one form of refinement, if any, appears, and I accordingly note the following Edinburgh exhibits and related publications as being wholly overlooked by Mr. Bilson, in addition to his omissions already mentioned.

In 1898 I published, as a portion of my report on the Brooklyn Museum expedition of 1895, elaborate surveys and specially made detail photographs of the asymmetries in elevation of the north and south walls of the Pisa Cathedral, including the great string-course which separates the two stories, and all the masonry courses below and above this string-course, as connected with the construction of the façade.*

This façade, which does not project at the apex beyond the heavy pilaster bases at the angles, leans forward a foot (0·97) in the first story, steps back half a foot (0·56) at the second story, leans forward a foot (0·97) in the first gallery (second story), leans forward an inch (0·10) in the second gallery, and is exactly perpendicular in the two upper galleries. The suggestion of two successive subsidences during construction and before the subsequent erection of the two perpendicular upper galleries was negatived by an elaborate series of surveys and photographs in the Edinburgh Exhibition, which were the originals of those published as described. The facts showing constructive intention, as announced by me (originally in 1874), have been carefully examined and certified by the architect in charge of the Cathedral, Signor Annibale Messerini. Baron H. von Geymüller, who, as the architectural editor of Burckhardt's *Cicerone*, ranks as the foremost authority in Germany on the architectural history of Italy, wrote me as follows regarding this article:—

Baden-Baden, 7th January 1904.

DEAR SIR,—It was only yesterday that I succeeded in reading your interesting study concerning the Leaning Tower of Pisa in the *Architectural Record*, vol. vii. No. 3. Allow me to tell you that you have entirely converted me concerning the cathedral. I had noticed those lines of the aisles outside, but never studied the reasons. I naturally considered it was the bad ground, as I had been taught to believe in the Vasari explanation of the Leaning Tower. The courage of those masons who dared to continue seemed to me quite wonderful. Your study has opened my eyes, and I am very grateful for the benefit received. As for the reasons why they did in the cathedral what we see I am less convinced than as concerning the intentional building so. But you may be right also in what you say on this point.—Believe me to be, dear Sir, yours very truly

BARON H. VON GEYMULLER, Architect.

Does Mr. Bilson's silence on this topic outweigh the utterance of Baron von Geymüller?*

In 1902 I published an article on "A Renaissance Leaning Façade at Genoa." The certificate of the architect in charge has been published and the related exhibits were shown at Edinburgh.†

M. Choisy wrote me regarding this article as follows, under date of 7th December 1904:—

"Au point de vue de l'histoire des recherches d'effet dans l'art occidental, St. Ambroise de Gênes est une trouvaille. Voilà un cas où la déformation systématique est indéniable; les bases de pilastre qui ne se retournent pas d'équerre témoignent leur intention; la façade se penche vers le spectateur comme le tableau d'une galerie."

Does Mr. Bilson's silence about S. Ambrogio at Genoa outweigh the utterance of M. Choisy?

In 1902 I also published an article ‡ containing a series of measurements taken in the spiral stairway of the Leaning Tower of Pisa, showing that the stairway rises in height towards the leaning side, through the first story, 12 inches; that it declines in height away from the leaning side, through the second story, 8 inches; that it again rises in height towards the leaning side, through the third story, 7 inches; and that it again declines in height away from the leaning side, in the fourth

* The Problem of the Leaning Tower of Pisa. *Architectural Record*, vol. vii. No. 3. Not quoted by Mr. Bilson.

* I have published various instances, to be subsequently mentioned, of apparent relations between some of the Pisanesque refinements and those of antiquity. Vitruvius directs that the pediment of the temple should be leaned forward, and says that it otherwise would appear to lean backward. It may be that we have at Pisa a survival or modification of such a practice or a practice based upon similar considerations. The forward projection of the upper façade, amounting to about 17 inches, when compared with the lower surface of the first story, has been carefully kept within the limit of safety, because it does not project beyond the outer line of the massive pilaster bases at the angles. Considered as a constructive unit, and with reference to these supports, the façade is constructively secure. The inclination is inconspicuous, and has been generally unknown except as announced by my publications. For instance the gentleman who was in charge of the monuments of the district in 1895, and who is an authority on Pisan art, was not aware of it in that year. It was also unknown to the architect in charge of repairs in 1906, who subsequently in that year certified to the facts as examined by him in my company.

† *Architectural Record*, vol. xii. No. 6; also published as *Museum Memoir*, No. 1 (Macmillan). Neither publication is quoted by Mr. Bilson.

‡ *Journal of the Archæological Institute of America*, vol. vi. No. 2, new series. Not quoted by Mr. Bilson.

story, 12 inches.* These measurements support the contention and arguments of Ranieri Grassi (1837), and of other observers, as to the intentional construction of the Leaning Tower, by the argument that a spiral stairway could not alternately and gradually rise and decrease in height without a motive, and that the only assignable motive would be to decrease the weight of masonry on the leaning side and to increase it on the side opposed to the lean, thus indicating an intentional construction. The *Building News and Engineering Journal* of 22nd September 1905 says that I have proved "that the tower was built as it now stands." Does Mr. Bilson's silence on this subject outweigh the utterance of the Editor of the *Building News*?

In 1905 I analysed the levels of the great string-course of the Pisa Cathedral † with fourteen exhibits, showing that the surface level descends from the north-west angle to the south-east angle of the cathedral 3·02 feet, showing that the string-course descends 4·56 feet between the same angles, showing that the string-courses of the north and south sides each fall 2 feet towards the transept, showing that the string-courses of the transepts adjoining these walls both rise 6 inches away from them in opposed direction, and that the string-course of the various sections of the building is from these points built to the slope of the surface as reaching its lowest point at the south-east angle of the choir. The levels of the Pisa string-course are the most important contribution ever made to the study of the Pisan Romanesque asymmetries, and they are absolutely without a parallel in any other mediæval building known to me.

Does Mr. Bilson's silence on this subject outweigh the authority of Signor Annibale Messerini, architect in charge of the Pisa Cathedral repairs, whose certificate on the obliquities of the string-course has been published?

The query thus rises: Are all these various observations to be thrown out of court by Mr. Bilson's argument about deflected plans and vertical curves? Is the standing of Mr. Bilson as an authority on the Pisan Romanesque such as to justify him in tacitly condemning wholesale, by a conspiracy of silence, careful surveys which he has never seen and a multitude of publications which he has never criticised or quoted? Is not the authority of Signor Annibale Messerini, the architect in charge of the Pisa Cathedral, to be treated with the same respect for questions relating to that cathedral as the standing of Mr. Bilson on a question relating to some mediæval church which might be under his care as repairing architect in Hull? The certificate of the architect in charge of repairs in a given building may not be an absolutely final document, but it certainly deserves mention and respect when such remarkable observations are in question.

I now once more recur to Mr. Bilson's prefatory classification and to his assertion that "it covers the great majority of the examples" noted in my publications, and that "those which it does not include are not material to the examination which is the immediate object in view." Mr. Bilson's opinion has also been quoted to the effect that "the chief value of Mr. Goodyear's investigations will be found in the fact that they repeatedly afford in themselves evidence in disproof of the theories which he bases upon them." The object of his Paper "is to give a definite answer" to the question whether they do or do not.

As far as we have proceeded in the subject, Mr. Bilson's "definite answer" has been to say nothing at all. We congratulate Mr. Bilson on the discovery of a great "refinement," not, to be sure, in architecture, but at least in architectural criticism. Considering the number and importance of the phenomena which Mr. Bilson has ignored it can hardly be said that his performance is equal to his promise.

After our sketch, as so far given, of Mr. Bilson's shortcomings in describing the scope of the Edinburgh Exhibition we shall approach his "Class I." with some trepidation as to his mastery of the topic, which he specifies as follows:—"I. Obliquity of alignment in plan, including want of parallelism in walls and piers, deflections of axis, curves of alignment (stated as uncommon in mediæval churches) and asymmetric plans generally."

Our critic confines his discussion of "asymmetric plans generally," and of all the phenomena above mentioned, to a single class, viz. the deflections of axis. He does not admit that there are any constructive phenomena outside of the explanations which, in his view, cover these deflections. His complaint regarding my own method is that I "appear to treat each building as if it were the product of a single mind, constructed in a single building campaign." As opposed to this method he reminds us that "the great majority of the larger churches of the Middle Ages have been built on the sites of earlier structures." His next step is to take the "most common and simple case, in which reconstruction was commenced by the erection of a new choir. The old nave would remain standing, kept in use as long as possible, and temporarily closed toward the east till the new choir could be occupied. Then the reconstruction of the nave would be undertaken, frequently not as a whole, but in successive sections of a few bays at a time, each section being temporarily partitioned off until it was completed. . . . In these circumstances, and in the absence of such instruments as would now be used, it must frequently have been a matter of great difficulty to ensure accurate setting out, and a trifling initial error might easily lead to wide divergence and marked irregularity.

* The measurements as given here vary slightly from those of the Edinburgh cartoon, which was hastily prepared without access to the original memoranda.

† *Edinburgh Catalogue*, pp. 40–45.

And this difficulty would be greatly increased by more complicated conditions than the simple ones just suggested, such, for example, as the building of aisle walls outside of and beyond an existing nave, the walls or arcades of which would not be taken down until these new aisle walls had been carried up, and perhaps only then taken down piecemeal as the new piers and arcades were built in sections. Is it any matter for surprise that under such conditions irregularities or incorrect alignment should result? . . . M. de Lasteyrie has recently stated his opinion that in the majority of cases deviations of axis are the inevitable result of the conditions under which the builders of the Middle Ages worked.* "

In considering the above quotations I am necessarily led to take notice of the few sentences with which the Comte de Lasteyrie has honoured me in the course of his recent publication, as above referred to, and as also again referred to at the close of Mr. Bilson's Paper.† M. de Lasteyrie displays the same misunderstanding of my position, and the same oversight of the arguments in support of it which afflict Mr. Bilson. His publication, as a whole, relates to the theory that the deflected cathedral choirs symbolise the bending of the head of the Saviour on the Cross; but in contending for the wholly accidental and fortuitous origin of these deflections he is led to consider briefly my own alternative suggestion, as also opposed to symbolic interpretation, viz. that they were intended to give a more picturesque vista and to complicate the perspective and optical effects. M. de Lasteyrie's arguments are those which Mr. Bilson has repeated, and the arguments of both these gentlemen may thus be disposed of at once.

As preliminary rejoinder it may be said, first, that I have never published any plans for Northern Europe, and consequently that I have never published or caused to be surveyed any plan in Northern Europe which has a deflected choir; secondly, I have never published, and have never seen, *a deflected mediæval choir* in Italy, and am not aware that there are any in that country, with the solitary exception of S. Lanfranco, near Pavia.‡ On the other hand, the numerous plans of *oblique type* which the Brooklyn Museum has surveyed in Italy belong to a class which has never previously been described, analysed, or published.§

These plans were first made known through the *Architectural Record*.* All of them were shown at Edinburgh, and the circumstances and causes of the deficient illustration of the catalogue in such particulars have already been explained.

It is rather amusing to find Mr. Bilson (as just quoted) wandering off into a long account of hypothetical occurrences which have visibly no relation to these plans, when a single glance has been given to them. It is more amusing to find him quoting from p. 63 of the *Edinburgh Catalogue* and omitting the passage on the same page which states that these oblique Italian plans are unlike those with deflected choirs. The Catalogue says, as quoted by our critic: "It has been tentatively held by various high authorities that the deflected choirs of the northern cathedrals are due to building at various dates and to imperfect orientation, or joining together, of constructions of different periods." But this passage continues, as *not* quoted by our critic: "It will be observed that the Italian plans oblique to the façade cannot be thus explained. They are very frequently small churches of manifestly homogeneous and contemporary construction. In the second place, the plans so far illustrated have straight walls which are not deflected."

We are thus led to point out that Mr. Bilson has repeatedly spoken of "oblique" and "deflected" plans as though the terms were synonymously used in the *Edinburgh Catalogue*. In so doing he has confounded two types of plans which are quite distinct, and one of these types has been first made known through the Brooklyn Museum surveys. Hence if the terminology adopted in speaking of this type be not followed, such confusion results as we find in Mr. Bilson's argument. M. de Lasteyrie's comments on my views also fail to notice that his explanation of deflected plans, which Mr. Bilson has followed, does not apply to those which I have published as oblique. The distinction is this: In an "oblique plan" the walls are straight from start to finish, but they are not normal to the façade or, in many cases, to the terminal wall of the choir. In the Italian oblique plans the walls are oblique in one and the same direction, generally to the right, but at the same time the lines of piers or columns, also straight from start to finish, in many cases, are generally oblique in opposed directions as regards one another, diverging in alignment towards the choir, whereas the walls themselves, as far as observed, have not been found to be oblique on

* La déviation de l'axe des églises est-elle symbolique? Paris, Librairie C. Klincksieck (1905).

† "It may be well to see how these theories of his have been received in France"; then quoting the scepticism of de Lasteyrie and Lefevre-Pontalis, but omitting to mention the independent announcements of Enlart and Choisy which are identical with my own in several directions.

‡ De Dartein, *Etude sur l'Architecture Lombarde*, pl. 68. St. Peter's at Rome has a deflected axis, but need hardly be quoted here.

§ The plan at Annaberg is published as having straight walls, which are oblique to the façade, by Dehio and von Bezold, pl. 453, book iii. No other oblique plan is known to me in Northern Europe. It must be remembered that the designation of "oblique" plan, as here used, excludes the plans with deflected walls and those with converging sides.

* Mainly in vol. vi. No. 3 (1897), "Constructive Asymmetry in Mediæval Italian Churches." It has already been observed that this publication is not mentioned by Mr. Bilson.

both sides to the façade, although they frequently diverge as regards the amount of obliquity in the one given direction.* Among such plans are the following: Ruvo, obliquity from the normal axis of 8 feet, nave widens 2 feet; Troja, obliquity of about 8 feet, nave widens 1 foot; S. Nicola at Bari, obliquity of 8·60, nave widens 1 foot; S. Chiara at Assisi, obliquity of 5·30, walls exactly parallel (builder's error of 0·00); Castel S. Elia, near Nepi, obliquity of 8·25, nave alignments parallel (builder's error of 0·15). The Cathedral of Cremona is oblique 13 feet on the median line drawn from the centre of the main doorway to the centre of the apse. This median line divides the nave into equal halves, although the nave widens 7½ feet in the same distance of 200 feet. The left aisle widens an additional foot, and the right aisle widens 2 feet, so that the outer walls widen about 10½ feet. Although there are wide transepts, the alignment of the piers is true through nave and choir, and the walls of the choir are not deflected from the alignment found in the walls of the nave.

Thus it will be observed that the debated hypothetical explanation of Bilson and de Lasteyrie for deflected plans, that they are due to screening off portions of a church during construction and to the resulting difficulty of continuing a true alignment, does not in the slightest particular cover these plans, which are oblique as regards the façade and terminal wall of the choir (one or both), but which are straight in the alignment of walls and of oblique interior arcades. On the other hand, if any asymmetric plans appear to be intentional—viz. those which have been described, and which these gentlemen have not considered and apparently have not examined—it is quite evident that the suggestion of design for a certain number, at least, of deflected choirs could not very well be rejected.

My own suggestion in explanation of the deflected northern choirs was originally reached in the following way. Among the Italian basilica plans without transepts or exterior indication of the choir, there are some small churches having exterior straight walls with diverging but straight alignment of columns in the nave, and having deflected straight alignment of the corresponding arcades of the choir. In these small churches the theory of incorrect alignment, as due to a screen, shipwrecks not only on their size, but also on the true alignment of the walls. Thus S. Pietro at Toscanella (Byzantine-Romanesque) is less than 125 feet long. The arcades of the choir are both deflected to the right in parallel directions, as compared with the arcades of the nave, and to the extent of 18 inches as compared with the left arcade. The arcades of the nave widen 5·50 toward the choir in straight alignment from start to finish. Both side walls have true alignment. The left wall is normal to the façade, and the right wall is oblique to the right, only 1·90 for the length of the nave, which widens 5·50. S. Maria at Toscanella (Byzantine-Romanesque), less than 100 feet long, and S. Pietro at Assisi (Gothic), less than 90 feet long, are similar instances as regards an interior deflection in the arcades of the choir which has no relation to the plan of the exterior walls.* These were the churches which originally suggested an analogy with the deflected choirs of Northern Europe, as representing interior deflections of the choir which appeared to be the Darwinian predecessors of the evolution of the exterior bends. They are also closely related to the oblique plans which are straight throughout in their interior and exterior alignment, and thus appear to form a connecting link between two other types of plans which are in themselves distinct, but which produce analogous optical effects.

It is undoubtedly true that the existence of a predetermined asymmetric plan postulates for some mediæval builders the habitual consideration of optical effects. The proof that such effects were considered is offered by the existence of mediæval perspective illusions of varied, and frequently of very subtle, character,

It is also true that the existence of a predetermined asymmetric plan presupposes an artistic pleasure in its effects, and this again postulates the absence of an abnormal or over-conspicuous irregularity. The question thus arises, What amount of variation from symmetry is consistent with the optical oversight which produces, even if it did not intend, an optical illusion, or which produces such a complication of optical effects as to mystify, and consequently interest, the eye? On this head it may be said that the plan of Cremona Cathedral is published by Dehio and von Bezold as absolutely normal and absolutely rectangular and parallel in alignment. The actual plan of the Cathedral of Cremona has just been described. It thus appears that an obliquity of 13 feet and a widening in the walls of 10½ feet have been disregarded or overlooked at Cremona by Dehio and von Bezold, or by some other authority from whom they have borrowed this plan. Such oversights are an almost universal rule in the publication of modern surveys,

* It is frequently supposed that arcades which widen in alignment toward the choir tend to diminish perspective. This overlooks the point that the attendant foreshortening of the arcades increases perspective effect of distance, and is thus contradictory to the effects of line in plan. Conversely, when arcades narrow in plan toward the choir, perspective effect in plan is contradicted by an effect of increase in the height of the farther arcades, as compared with the effect which would result if the arcades were parallel. Generally speaking, asymmetries of plan, if intended at all, must have been intended to complicate the optical effects, rather than to produce an effect for one direction only, which was reversed in the opposite direction.

* Measurements and plans of all these churches in the quoted article.

which are generally carried out on the theory that one measurement represents a given dimension for the entire church. Thus Reynaud's *Traité d'Architecture* publishes S. Maria Novella at Florence as having equal bays, when they actually diminish toward the choir to a total amount of 13 feet. In both instances my own observation shows that variations up to the mentioned amount are not noticed by the eye. In all cases of oblique plans it must be remembered that the optical effect is invariably translated by the eye into an obliquity in elevation. This obliquity already exists optically in all horizontal lines which are not seen in parallel perspective. Hence the effect of an oblique plan is simply to optically decentre the church, to shift in some particulars, but not in others, the assumed position of the spectator, and to confuse and complicate the optical effects from any given point of view.

It is certainly not to be expected that critics, such as Mr. Bilson, who have ignored the proven existence of mediæval optical effects in the way of perspective illusions, will be prepared to admit a mediæval practice which presupposes the consideration of other optical effects. None the less, the existence of perspective illusions in some of the same churches which exhibit oblique plans or interior deflection of the choir arcades is not to be forgotten by the unprejudiced critic. Thus S. Pietro at Assisi, which is planned as a perfectly true rectangular parallelogram, and which has an interior choir deflection of 1·80, has arches which descend toward the choir 2·60 and a pavement which rises toward the choir 1·70. The margin of builder's error, as obtained by comparing pairs of measurements in the nave, is 0·20. S. Nicola at Bari, which is oblique 8·60 on the median line, has also perspective schemes in elevation in the direction of the choir of a very pronounced character.*

From another point of view the oblique plans suggest analogies with another class of asymmetries which Mr. Bilson has also ignored, and which Ruskin and Choisy have considered interesting and pre-determined. They display the "fixed scorn, if not dislike, of accuracy in measurements," which Ruskin pointed out, and they remind us of Choisy's view that, "generally speaking, the architects of the Middle Ages avoid formal regularity." It is not a long step from my observations of asymmetric arcades and of the obliquities in elevation at Pisa to these obliquities in plan, and they invariably have similar effects. All of these phenomena were, in fact, originally described in the same publication. Once more we recur to the point of view, so well explained by Ruskin and by Choisy, that both accepted and designed irregularities represent a single mediæval virtue, and that the acceptance of local and accidental variations is a phase of the same artistic spirit which also purposely designed them.

* Section in *Architectural Record*, vol. vi. No. 2.

It is not, however, my mission to argue with Mr. Bilson on any points which do not meet his own contention. My aim is simply to point out that my suggestion regarding deflected choirs was based upon some oblique plans which are not deflected, and upon others which are only deflected in the interior alignment and not in the exterior walls. These plans are palpably not explained by Mr. Bilson's theory.

Thus by the study of Italian basilica types with perfectly straight exterior walls, without transepts, and without any choir whatever, as regards exterior form, the conclusion has been reached that asymmetries of plan were deliberately designed in Italy. This conclusion has been extended to externally deflected choirs in Northern Europe and has been offered as one possible or probable explanation of them, without asserting that it includes them all. There is not the slightest doubt that local irregularities of building plots were occasionally accepted as determining an asymmetric plan. The bend in plan of the Cathedral of Saint Lô is clearly determined by the bend in the street. Saint Jean-au-Marché at Troyes is another example of a church which is certainly built to the bend in the street on the south side. It appears, however, doubtful that purely local irregularities could explain such a vast number of deflected choirs as are known to exist in Northern Europe; for the question then rises: Why is the bend so frequently found exactly at the choir? On the other hand, the hypothetical explanation which is offered by Mr. Bilson may possibly apply to other individual instances; but since it cannot be invoked to explain the Italian plans, there is no reason for straining it to apply to all the northern ones. In the case of a screened choir or of a screened nave there is no reason why external sighting and justification of alignment could not be carried out, and this appears to be a fatal objection to any very wide occurrence of the reasons invoked for a deflected alignment of external walls. When a nave was built on to a screened-off choir, why could not the masons lay out a true right angle from the outside walls of the transepts? When a nave was to be prolonged, why could they not pass outside the screen and continue the line of the exterior walls?

In view of the wonderful engineering accomplishments of the Middle Ages and of the existence of so many instances of severely accurate mediæval planning, it would appear that Mr. Bilson's theory lays too much stress on the "imperfect instruments at their command." M. de Lasteyrie also insists on the mediæval ignorance of modern instruments as an all-sufficient explanation of the deflections which resulted from screening off an unfinished church, but he does not explain how the master-masons frequently contrived, in spite of this ignorance, to lay out plans with perfect precision under other circumstances. Viollet-le-Duc, however, assures us that the mediæval masons were consum-

mate masters of geometric science, and he attributes such knowledge not only to the master-masons, but also to the ordinary workmen who were employed on the Gothic cathedrals.*

The most "complicated" plan can be surveyed with accuracy by the use of a peg, a cord, and a measuring tape. It follows that such a plan can be laid out accurately with the same "imperfect instruments." Having assisted an accomplished surveyor who was employed for several months in Italy by the Brooklyn Museum under my direction I can speak with authority on this subject. This gentleman, Mr. John W. McKecknie, who also plotted all the Italian surveys, had a complete surveyor's outfit of modern instruments, but frequently found it convenient to forgo their use. To establish a true rectangle it is sufficient to inscribe two intersecting segments of circles which are drawn with a piece of string and a pencil peg, from two opposite extremities of the same radius, which is the base of the procedure, and this radius is generally laid down through the centre of the main entrance and in true alignment with the façade. From the centre of this radius a cord is stretched which passes through the point of the intersecting segments of circles. This establishes a true rectangle and a true normal line. The cord is then stretched through the given points to any desired length, to establish a longer normal line. All lateral measurements are then taken to this line, or to points established from it. The subsequent procedures are matters of simple detail. Having personal experience as an interested assistant in this surveyor's work, it is within my personal knowledge that a true rectangle and a true plan can be laid out without any instruments whatever. In the case of a walled-off choir all that would be needed in planning a correctly aligned nave would be to carry out a true normal line through a door in the wall, and this normal line could be established by the simple procedure which has just been described.

My own studies of oblique plans have abundantly convinced me that no theory excepting that of premeditation will explain a number of them, knowing how simple a matter it is to lay out a true rectangle, and how constantly this was done not only in neighbouring and contemporaneous mediæval buildings, but even in the very building which may be oblique in other particulars. Thus S. Nicola at Bari has sixteen true rectangles in deeply recessed doorways in a plan which is 8·60 feet oblique on the median line. The church stands on open ground without contiguous buildings and with wide piazzas on three sides, and there is a street on the fourth side from which the plan might have been removed to any desired distance.

* *Dictionnaire*, ix. p. 204. "Une méthode pareille exigeait, il est vrai, une pratique très complète de la géométrie, non seulement de la part du maître, mais aussi chez les metteurs en œuvre."

Therefore in considering the topic of asymmetric plans, the first question to be determined, before considering the deflected choirs, is whether any of those appear to be intended which are not externally deflected, and the oblique plans which are straight as regards the exterior walls and interior arcades offer a problem which appears not to have dawned on Mr. Bilson as even existing. If he should fall back on the suggestion of an obliquity of site, after this problem does dawn on him, the adverse local arguments and facts in the individual cases which have been surveyed are too numerous to mention. It may simply be said that all the churches quoted in this Paper, and many others, stand absolutely free and isolated as regards surrounding buildings, and that their oblique alignment of plan has certainly never been caused by local asymmetries of the building plots. If scepticism still insists on this point we have simply to appeal to the interior obliquities of straight alignment, in arcades, which differ entirely in direction from the exterior obliquities. Finally we are able to quote normal plans with walls forming true rectangles, which are oblique in straight lines as regards the interior arcades, and in such cases these also generally widen in alignment toward the choir. S. Pietro at Assisi thus widens 1·60 feet with a wholly normal and rectangular exterior, and also has a carefully constructed oblique apse which is set in a normal terminal wall. Orvieto Cathedral widens 0·90 (foot decimal) in the piers of the nave, with straight alignment, and is normal and rectangular as regards the side walls and façade.

A decisive case of an intentionally devised asymmetric plan is found in the transepts of the Pisa Cathedral. This instance is not mentioned with a view to the conversion of Mr. Bilson, for he is presumed to be already acquainted with it, as described in the *Edinburgh Catalogue* at p. 31. It is mentioned for the benefit of others as an instance of the kind of evidence that is insufficient to convert Mr. Bilson. These transepts are delicately fan-shaped, with sides converging toward the nave, as shown by the following measurements:—

South Transept.		*North Transept.*	
South wall . .	57·63	North wall . .	57·88
North opening .	56·60	South opening .	56·78
Convergence toward the nave	1·03	Convergence toward the nave	1·10

Thus the corresponding measures tally on the outer walls of the transepts within 0·25; they tally at the openings into the nave within 0·18. The measures for convergence tally within 0·07. This is the kind of evidence which leads me, in spite of Mr. Bilson's protest, to treat certain buildings as though they were and are "the product of a single mind, constructed in a single building campaign." There is also a simple

explanation for this construction as being intended for exterior effect, and to exaggerate the optical effect of the exterior obliquities in elevation of the transept string-courses. These are described in the *Edinburgh Catalogue* in connection with the corresponding obliquities in elevation of the great string-courses on the side walls.

A very curious question is finally raised by the theories of Bilson and de Lasteyrie as to screens and temporary partition walls. If the externally deflected choirs of Northern Europe are really explained by these theories, how does it happen that there are practically none in Italy? Surely the conditions described as explaining these choirs cannot have been confined to the northern territories; and if they existed in Italy, why did they not produce there many similar exterior deflections? It is expressly declared, by Bilson and de Lasteyrie, that their theory does not presuppose gaps of time or delays of construction, but that the screening in successive sections of partially finished cathedrals which were erected with rapidity and within an unbroken short period is what caused their deflections of exterior plan, as due to the assumed deficiency of proper instruments. The law of chances would not allow us to presume that none of these very frequent "accidents" happened in Italy, and yet when the observations which Mr. Bilson has attacked are examined it turns out that the Brooklyn Museum surveys have not included any externally deflected choirs in their very thorough examination of Italian churches.* It is certainly not my intention to assert that the modest church of S. Lanfranco, near Pavia, is the solitary instance of an externally deflected Italian choir, although it is the only one known to me by publication or by observation. What can be safely said is this, that none of the widely known Italian cathedrals have externally deflected choirs, and that many of the widely known northern cathedrals notoriously do have them. According to the law of chances and the universal practice in cathedral building which Mr. Bilson has invoked, this is a most curious state of affairs. If it were assumed that the Italian interior deflections of choir arcades, as in S. Maria and S. Pietro at Toscanella, represent a type of earlier date and of more refined design, and that the external choir deflections represent a later northern exaggeration, derived from the more subtle Italian system, then the deficiency of deflected choirs in Italy would be explained. On the other hand, if the oblique Italian plans are accidental, it would again be difficult to explain why a considerable number of them are not known in Northern Europe, and also why they are not more common in Italy. About thirty-five examples only are known at present. An examination of French Romanesque plans to ascertain if they also present instances of choirs which are deflected in interior planning, without exterior bends, might throw additional light on this subject. At present the known deflected choirs of the north appear to be almost wholly Gothic, and the oblique plans of Italy are mainly Romanesque.

Mr. Bilson has quoted at fourth hand (on two occasions), and M. de Lasteyrie has quoted at third hand, the story of the architect of Metz who "died of grief and distress" because he was "ashamed of having made his work so crooked." M. Anthyme Saint-Paul has very justly remarked of this story that its value would be much greater if the date of the chronicle mentioned by Kraus were known to be contemporaneous with the event.* In default of assurances on this head we find Mr. Bilson's sarcasm *à propos* of this tradition rather tedious. This sarcasm might have point if I had ever published the plan in Metz, or if I had said or knew anything about it. That there were blockheads among the mediæval builders is possible; that many mediæval builders did not practise refinements is certain; and that some of the blockheads may have belonged to their class is probable.

The contention that deflections of alignment in church construction are due to the successive screening off of successively constructed portions of the church is also curiously oblivious of the existence of curves and bends in plan which are developed above the ground plan, and without occurring there. For optical effects these would be equally effective as found above the level of the eye. For instance, there are parallel curves in plan in the gallery parapets of the Pisa Cathedral which are not found in the alignment of the nave columns. The curves are convex to the nave on the north and concave to the nave on the south. Hence these curves, being parallel, are counter to thrust from the south aisle vaulting, and thrust from the aisle vaulting cannot consequently be invoked to explain either of them. The Cathedral of Rheims has curves in plan on both sides, convex to exterior, which measure 10 inches deflection to a side at the clerestory parapet, whereas the alignment of the piers of the nave and of the exterior walls of the aisles is perfectly rectilinear. Manifestly the theory of a screened-off church as explaining incorrect alignment cannot be invoked for a gallery or a clerestory when it does not apply to the ground plan, while the very proposal of such a theory concedes to the mediæval mason the ability

* The right wall of S. Maria at Toscanella bends slightly at the choir to exaggerate the original obliquity beginning at the façade. The church stands in the open country at some distance from the walls of the town, and there are no contiguous buildings. The other features of the plan have no relation to the theory of "crooked sites."

* *Bulletin Monumental*, 1906; M. Anthyme Saint-Paul, *Les Irrégularités de Plan dans les Eglises*, p. 135. "Sur ce texte, néanmoins, j'aurais souhaité un supplément d'information, car il perdrait singulièrement de valeur s'il n'était pas contemporain ou très voisin de la période 1371–1409, qui est celle de la construction de l'édifice, si ce n'était qu'un *fertur*, un écho d'une tradition populaire."

to build in true alignment when there was no definite cause to the contrary.

On the whole topic of horizontal curves which we are thus led to take up Mr. Bilson is silent as the grave. He disposes of the subject in six words by the parenthesis which has been quoted, referring to the *Edinburgh Catalogue* for the fact that curves of alignment "are uncommon in mediæval churches." Here is one more occasion for taking exception to the character of this "definite answer" to the teaching of the Edinburgh Exhibition. That a critic has a right to choose his own ground, that he has a right to bear hard on one point and neglect another, all this is true; but he has no right in so doing to make a pretence of disposing thoroughly of questions which he entirely ignores. If it has been said in the *Edinburgh Catalogue* that horizontal curves are "uncommon in mediæval churches," it has also been said that they are most numerous in the buildings which are otherwise the most remarkable for their refinements, and that their rarity, apart from such buildings, is an indication that they are not accidental in these. It has also been pointed out that these curves indicate an historical connection, in actual builder's practice, as between some of the refinements of antiquity and some refinements of the Middle Ages.

The evidence for this connection was the introductory subject of the Edinburgh Exhibition. The *Building News* of 22nd September 1905 says that: "in spite of considerable lapses of time between the examples the series is tolerably convincing." How can the teaching of the exhibition be refuted by an answer which not only slights this evidence, but which even ignores its existence? The *Building News* says: "Of the existence of the curves and of their being in a great many instances due to some preconceived plan, and not to mere accident, the present exhibition is evidence enough." Mr. Bilson certainly is privileged to doubt this statement, even although he has not examined the exhibits, but he is not privileged to misrepresent the Catalogue by a misleading quotation. Under the argument from which that quotation is taken the curves in plan of the cloisters of Verona and Bologna are catalogued as repeating the curves in plan of the second temple court at Medinet Habou. Under that argument the curves in plan of the Maison Carrée at Nimes and of Pæstum are catalogued as preceding and explaining those of S. Sophia at Constantinople. The quotation is connected with an argument describing the curves of S. Apollinare Nuovo at Ravenna, of S. Donato at Genoa, of the Fiesole Cathedral, of the Pisa Cathedral, and of St. Mark's at Venice, as related historically to the curves of antiquity through Byzantine Greek transmission.

The first fourteen exhibits of the Edinburgh Exhibition were wholly devoted to this subject, and the entire number of exhibits for horizontal curves was thirty-eight. The "Index of Exhibits according to Subject-matter" specifies nineteen buildings for curves and bends in plan and for curves and bends in elevation. Of these buildings sixteen represented true curves in plan as distinct from bends. Thus more than one-third of all the Italian buildings and more than one-fourth of the entire total of buildings in the exhibition were represented by exhibits for curves in plan. Mr. Bilson's parenthetic and solitary allusion to curves as stated to be "uncommon in mediæval churches" thus ignores a refinement which was represented in the exhibition by as large a number of buildings as the average allotted to any other single class of phenomena, and only slightly exceeded by the number allotted to perspective illusions.

Curves in plan which are parallel, including parallel curves which begin at the foundations, were repeatedly instanced in the exhibition. Such parallel curves can never be due to thrust, for in these cases the causes of thrust are always eliminated on one side of the given church. It is for Mr. Bilson to explain how a series of mistakes in alignment due to screening off successive portions of a church during construction can be invoked to explain the parallel curves of S. Apollinare Nuovo at Ravenna. As to the cloister curves of Bologna and Verona, which are convex to the centre of the court, and which begin at the foundations of the walls and parapets, the "definite answer" is once more, total silence.*

Let us now examine more carefully and requote, for that purpose, our critic's opinion, that "the chief value of Mr. Goodyear's investigations will be found in the fact that they repeatedly afford in themselves evidence in disproof of the theories which he bases upon them." For we are now able to estimate the slender knowledge of the Edinburgh Exhibition upon which Mr. Bilson has based his "criticism" of its "teaching." Upon this same slender knowledge must consequently rest his complaint regarding my "neglect of historical material," which is very formally made as follows:—

"It is curious to observe how little considerations of this kind (viz. regarding documentary history, regarding repairs and renewals, and regarding existing condition of the building) seem to have influenced Mr. Goodyear in his investigations."

This sweeping general assertion loses a considerable amount of its importance when we have ascertained exactly what Mr. Bilson is talking about, and, above all, when we are able to show what Mr. Bilson has not been talking about. So far as the

* None of my publications on horizontal curves are mentioned by Mr. Bilson's bibliography. They are as follows:—"A Discovery of Horizontal Curves in the Maison Carée at Nimes," including observations in Egypt (*Architectural Record*, vol. iv. No. 4 [1895]); "A Discovery of Horizontal Curves in Mediæval Italian Architecture" (*Architectural Record*, vol. vi. No. 2 [1897]); "Architectural Refinements in Italian Churches" (*Journal of the Archæological Institute of America*, vol. vi. No. 2, New Series [1902]).

Edinburgh exhibits have been considered in connection with his review of them, it is clear that this imputation must fall to the ground; for the double reason that it is evidently misplaced when the character of the evidence is considered, and that Mr. Bilson does not himself control the subject-matter in such a way as to justify him in having anything to say about it.

The index of the *Edinburgh Catalogue* specifies sixty-one buildings as represented in the exhibition, and if the "index according to subject-matter" be examined, it will appear that forty-six of these buildings have been so far covered by the various subdivisions of this rejoinder. Three of these forty-six buildings, the Pisa Cathedral, S. Maria della Pieve at Arezzo, and the Cathedral of Trani, also figure, however, in the list for vertical curves, or for the delicate horse-shoe form in construction of the nave, making a total of eighteen buildings in this latter class.

We may thus estimate the weakness of the pedestal upon which Mr. Bilson has reared the fabric of his "criticism"; for whatever significance this criticism may have can only relate to a single class of phenomena out of the many which have been described. If there be any substance whatever in his Paper it must be sought among these remaining eighteen buildings, and we will now proceed to search for that substance in this quarter.

At the opening of our rejoinder it appeared that Mr. Bilson had rather hastily classified the "great majority" of the Edinburgh exhibits in two divisions, viz. asymmetric plans, as just disposed of, and a second class, which he thus describes:—

"Walls, piers, and columns out of plumb, sometimes straight, but more generally described as 'vertical curves.'" Under this vaguely alarming but not very explicit caption, our critic's general introductory remarks are as follows (after closing his argument on asymmetric plans with the story of the architect of Metz): "It is equally impossible, however, to arrive at any satisfactory conclusion with regard to the irregularities included in Class II. (which Mr. Goodyear describes as 'vertical curves,' 'widening refinements,' 'parallel leans,' &c.) if they are to be regarded simply as isolated facts. They, too, must be considered in their relation to many other facts which go to make up the story of the structure. We must not only know the architectural history of the building in question, and the precise order in which the various parts were erected, but we must study its structure, in order to judge how far the problems of unequal loading, abutment of vaults and arches, &c., have been successfully solved, and what weaknesses would be liable to develop from any defects of design and construction; we must exhaust the documentary history of the structure, from its erection up to th present day, in order to ascertain what repairs, renewals, or alterations have been carried out, and the reasons for their execution; and, finally, we must carefully examine the building in its existing condition in order to detect any indication of movement in the fabric. Then, and then only, shall we be in a position to understand the real cause and meaning of these 'refinements.'"

This rather pompous programme may be condensed to the self-evident statement that we ought to be sure that there has been no thrust and no other accidental movement before the vertical curves or the horse-shoe form of nave construction can be announced as refinements.

Mr. Bilson's method of attack on the observations for vertical curves, and for the horse-shoe form in nave construction, appears to be not wholly justifiable. This method consists in wholly ignoring sixteen out of eighteen churches which were illustrated by large photographs at Edinburgh for these refinements;* in suppressing all of the various forms of evidence which were represented by these sixteen churches, and in omitting from his alleged bibliography all my publications for the numerous Italian churches in which these phenomena were first observed and originally described (including two of the most famous cathedrals in Italy, which have been formally and officially verified as constructive instances of my "theories" by the architects in charge of repairs in the given churches).

The philosophy underlying this procedure appears to be the assumption that no chain is stronger than its weakest link, or the link which appears to be weakest to our critic. It must be remembered, however, that Mr. Bilson's argument rests entirely on the suppressed premise of the inherent improbability of the asserted facts. If a single case of mediæval vertical curves or of mediæval horse-shoe construction of the nave be demonstrated to exist, this inherent improbability disappears, and it immediately becomes not only probable, but absolutely certain that other cases will be found. In the search for these other cases it might occur that an over-enthusiastic investigator had included unsatisfactory evidence, or mistakes of observation, or cases of accidental movement; but if right in any case the general contention for the existence of such a mediæval refinement would still be established, and this is, of course, the only matter really in debate.

Mr. Bilson, however, insists that all my eggs are in the one basket at Amiens, and that he has broken them all there; but in the same breath, and with enviable self-assurance, he explains that his reason for confining his attack in detail to a single cathedral is the fact that the large number of "churches covered by his (my) investigations renders it difficult to test his theories in each particular building."

* Beauvais, Noyon, and Saint-Remi at Rheims were not included in the Edinburgh exhibits. The Cathedrals of Paris and Amiens are the only other buildings which are mentioned by Mr. Bilson.

Therefore, the argument continues, in effect, I shall test his theories on a single church and you may take my personal word for it that "equally conclusive proof" could be found in all other cases "that their refinements exist only in Mr. Goodyear's imagination" if M. Durand would only write the necessary monographs.

The upshot of the matter is this, if Mr. Bilson's standpoint be accepted, that none of the evidence is worth consideration because one kind of evidence is mistaken or insufficient. But from my point of view there was no instance represented by photographs at Edinburgh for the given phenomena in which the individual building did not offer some new and distinct constructive feature in favour of the general thesis.

From my point of view there was not a single building illustrated in which the existence of repair or reconstruction of the vaulting, if demonstrated by documentary history, would throw this submitted constructive evidence out of court.

It has never been claimed that the horse-shoe widening and the vertical curves were a talisman or magic formula, involving the necessary preservation of a vaulting or ensuring it against the need of repair. On the contrary, it is morally certain that since the tradition of such construction was lost sight of, many repairs have been carried out simply because the widening horse-shoe form has been ignorantly described to thrust. It is self-evident, assuming vertical curves or horse-shoe widening to have been constructed, that the buildings which exhibit them were not less subject to decay or downfall than their fellows. It is self-evident that, when the tradition of such a system of construction had been lost, these particular buildings would be more exposed than any others to wholesale and brutal repairs, or to unnecessary rebuilding. Hence it is begging the argument to assume that every documentary account of a repair in piers or vaultings applies purely to a weakness in the building, which weakness such a curve or widening is assumed to represent.

This is the suppressed premise in Mr. Bilson's citation of repairs and rebuilding at Beauvais, Noyon, and Saint-Remi at Rheims. It did not, however, occur to him to mention that these buildings were not included in the exhibits at Edinburgh, or to inquire what the reason for this omission may have been. This reason was that there were at least eighteen better cases,* as regards visibly constructive evidence for this particular class of phenomena, and that the limitations of space involved the omission of much evidence in more than one direction.†

It would be unreasonable to suppose that impregnable constructive evidence could be furnished for all the cited instances of these particular phenomena. Some of these instances must take their chances as debatable cases long after the general facts are accepted. As the number of definitely proven instances increases, so will the tendency of scepticism decrease to contest every individual instance which is not definitely proven. As to the mentioned churches, which were excluded (with many others) from the Edinburgh Exhibition, the citations which I have made in publication of Beauvais have been extremely guarded.* My defence of all these churches will be involved in my account of some general facts which are also vital to the argument regarding the Cathedrals of Amiens and Paris, and these last are the only churches of the entire Edinburgh series of eighteen which Mr. Bilson has mentioned in any portion of his "criticism," incredible as this may appear to those who have not carefully studied his review. A brief account of the special publications in this particular field of the investigation, which have been overlooked by Mr. Bilson, is consequently in order here.

Among the seven *Architectural Record* articles which formed the original report on the Brooklyn Museum surveys of 1895, was the one on the mediæval Italian columnar and pilaster entasis, already mentioned as having been ignored by Mr. Bilson. In this Paper an entasis (vertical curve) in mediæval engaged columnar forms and pilasters was shown to be related to a mediæval columnar entasis, which was then also for the first time (as far as my knowledge goes) announced in publication.† These facts were found to have analogies and counterparts in the Italian Renaissance in monuments for which the similar facts never have been, and never will be, disputed.‡

The report was then continued by an article entitled "An Echo from Evelyn's Diary." This article was devoted to the attenuated horse-shoe form in nave construction (otherwise the so-called "widening refinement") as found both with and without the vertical curves or entasis. This was the first detailed account of the class of observations in which the Cathedral of Amiens has been subsequently included (the quoted title drew attention to Evelyn's mention that two English architects of the seventeenth century, Chicheley and Pratt, had considered an outward widening of the "maine walls" § of the Old S. Paul's as intended for optical effect).

I was by no means unexpectant of the general scepticism which might await these announce-

* This was written before the recent photographic surveys at Noyon and in Saint-Remi at Rheims.

† The arrangement with the Edinburgh Architectural Association was for 450 feet of linear hanging. The exhibits actually covered 550 linear feet.

* "The accident at Beauvais is not to be overlooked" (p. 125, *Edinburgh Catalogue*).

† The existence of the columnar entasis was independently announced by M. Enlart at a later date.

‡ Notably the Teatro Olimpico at Vicenza, the Church of S. Stefano at Vicenza, and still more notably the Church of S. Giorgio Maggiore at Venice. Two of these buildings are by Palladio, and S. Stefano belongs to his school.

§ Supposably referring to the clerestory.

ments, and special stress was therefore laid upon the fact that they were corroborated by Mr. John W. McKecknie, the expert in architectural engineering construction who had been employed by the Brooklyn Museum to act as my assistant in 1895. His certificate as engineering expert was included in this article. This publication is also excluded from the Bilson bibliography.*

It was not until 1901 that an opportunity was offered to continue the investigation; but when this opportunity was offered I made a trip to Italy, mainly to establish relations with the architects in charge of repairs of the Pisa Cathedral and of St. Mark's, and to make additional photographs and measurements of the Italian churches described in the "Echo from Evelyn's Diary." Another year elapsed before my museum routine work allowed me to publish the results of this expedition, including the certificates of Signor Annibale Messerini for the constructive vertical curves and other refinements at Pisa and of Commendatore Pietro Saccardo for the constructive horse-shoe form in the nave and transepts of St. Mark's. These certificates were published in the *Brooklyn Museum Memoir*, No. 2, a monograph of 111 pages, with 44 illustrations and 14 plans. This publication is also not mentioned by Mr. Bilson.†

In 1903 an opportunity was offered to resume the investigation, and this was the first occasion when it was possible to examine the Byzantine churches at Constantinople and the Gothic cathedrals of Northern France. Mr. Bilson's bibliography is wholly confined to the publications concerning this latter expedition. How far his success in dealing with the research at Amiens justifies such a procedure has now to be determined.

The suppressed premise in Mr. Bilson's argument on the Cathedral at Amiens, as implied in his wholesale exclusion of other evidence, and in suppressing mention of the existence of the various publications which have been cited, is this: "If the vaulting thrust at Amiens accounts for the phenomena there, it will account for all the phenomena elsewhere." I propose to meet his argument in both particulars; by showing that the vaulting thrust at Amiens does not account for the phenomena there, and by showing that the phenomena occur elsewhere where there is no vaulting thrust, and that they occur again in other cases where the thrust is opposed to the receding surface. Once more, therefore, will appear that same indifference or ignorance as to the existence of crucial cases, not covered by his own argument, which has been demonstrated to be Mr. Bilson's constitutional debility as a critic throughout this controversy.

At Amiens Mr. Bilson is at fault in his facts, and therefore in his conclusions. But wholly aside from Amiens, where the enormous altitude and large dimensions of the cathedral surround the obtaining of accurate measurements for the verticals with undeniable and tremendous difficulties, Mr. Bilson's neglect of proven constructive facts must now be illustrated by tangible examples.

The Cathedral of Trani is an unvaulted basilica with timber ceiling. It has a light "triumphal arch" at the opening of the nave into the transept, which also forms the choir. The wholly inconsiderable thrust of this arch is buttressed by the transept walls on either side. The shafts which support this arch are attached to these walls and widen delicately and continuously from the pavement up, in straight lines, which diverge at the capitals only 10 to 12 inches, as compared with the width at the bases, in a height of 48 feet, and the width between piers is only 25 feet at the bases. The facts are corroborated by Mr. McKecknie, who photographed them in 1895, and have been mentioned and illustrated in each of the publications which have just been quoted. They were also illustrated at Edinburgh.* Is it Mr. Bilson, or is it I, who is indifferent to the problems of "loading and abutment of arches" in this instance? That these two shafts can slope outward in an even and regular manner to the amount of only 5 to 6 inches on each side, in slopes beginning at the bases and rising throughout the entire vertical height of 48 feet, from any accidental cause, is impossible. It is insulting to the intelligence of any expert even to suggest the hypothesis of accident.

This delicate widening in attenuated horse-shoe form, and under equally convincing conditions as regards thrust or subsidence, is a widespread appearance in mediæval churches. Many examples have been published, but Mr. Bilson's only reply is that the large number of churches covered by my investigations renders it difficult to test my theories in each particular building. In other words he refuses to consider the case at Trani because there are others like it.

Trani is thus an example of a perfectly simple proposition in physics and in engineering construction. It is not contended that the arch at Trani has never been repaired. I know nothing about it, and the consultation of documentary history or the publication of a monograph in two heavy volumes about this church could not pos-

* *Architectural Record*, vol. vii. No. 2 (1897), "An Echo from Evelyn's Diary."

† *The Architectural Refinements of St. Mark's, Venice; with Remarks on other Churches showing a Similar System of Leaning Verticals, especially those in Orvieto, Vicenza, Milan, Pavia, Bologna, and Arezzo, and including the Renaissance Church of S. Giorgio Maggiore at Venice.* (New York: Macmillan.) As the title indicates, this Memoir was an effort to summarise the evidence for all parts of Italy for this particular system of construction.

* Exhibit 223. "When the absence of vaulting, the inconsiderable thrust of the arch, and the resistance of the deep transept walls are considered with reference to the diverging verticals, which rise in straight lines from the pavement up, it is evident that we have here a constructive refinement, and that an accidental movement cannot be invoked to explain the divergence."

sibly affect this problem. The proposition is simply this, that under the given conditions thrust could not operate to produce a slope of such delicacy and such uniform character at a distance of 48 feet below the springing of the arch. In fact thrust could not produce any kind of a regular slope in such a transverse wall.

Thus the existence of delicate slopes beginning at the pavement, and rising in straight lines to the springing of an arch or vaulting, is *prima facie* good evidence for the existence of the given refinement, wherever the abutment is furnished by transverse walls of considerable depth. Thus the apses of the Balaban Aga Mesjid at Constantinople,* of the Capella Palatina at Palermo, of Saint-Radegonde at Poitiers, and of the Schottenkirche (Renaissance) at Vienna† are examples of this class.

Where the abutment consists of the transverse terminal walls of the church aisles, and the outward slope begins at the pavement and continues in straight lines, the same positive evidence is found and is again independent of any question of repairs. This is the case in Saint-Loup at Châlons. The diverging piers at the apse are united with the terminal transverse walls of the aisles. This widening at the choir is 8 inches in a height of 27 feet (exhibit‡). A widening is found in all the piers of the nave, which also diverge in straight lines from the pavement up. This church has been published with illustrations in all of the publications which are quoted by Mr. Bilson, but he chooses to ignore it.§

Where there is an abutment consisting of the transverse walls of the transept and the outward slope begins at the pavement and continues upward in straight lines, the same kind of evidence is found. These are the facts in St. Mary Diaconissa at Constantinople (sixth century), in the Church of the Monastery of the Chorah at Constantinople (eleventh century), in St. Mark's at Venice, where the transept walls abut on the choir and nave, in the clerestory transept walls at Amiens,* in the crossing piers of Troyes Cathedral, and in the eastern crossing piers of Notre-Dame at Paris. Although the latter curve slightly it is a curve which recedes outward, from the pavement up, and may therefore be included with the foregoing cases. An especially significant instance of inclination against the abutment of transept walls is furnished by St. Jacques at Rheims (exhibit).

Leaving Amiens for special later consideration we pause for a moment to consider the question of repairs and accidents at Troyes as typical for the entire class of Mr. Bilson's objections appealing to the existence of repairs. No cathedral in Europe has had more misfortunes than Troyes. Hardly another has such a discrediting record, unless it be Beauvais, if a general history of its accidents were set against a general mention of its leaning verticals. And yet it is positive that no vaulting thrust could operate against the resistance of the transept walls in such a way as to carry the crossing piers outward so as to slope from the pavement up. Still less could it be explained how thrust could produce a diminution of the outward slope in the upper half of the eastern piers. At the level of the clerestory a slight elbow and change of angle to diminish the outward slant is seen in each of the eastern crossing piers at Troyes, showing that the slope, as constructed from the pavement up to that point, was changed, because found to involve too great a widening if continued as begun (two exhibits).

I may allude here, in passing, to Mr. Bilson's ridicule of the lack of uniformity in the treatment of the verticals of Notre-Dame. That the shafts of the choir curve into the nave instead of away from it, like the crossing piers, and that they converge (6 inches) instead of diverging, may be intended to develop by contrast the divergence at the crossing, and the arrangement certainly has that result. At all events the latter facts cannot be ascribed to thrust, and as they are uniform they cannot be ascribed to carelessness. Mr. Bilson ridicules the lack of uniformity in these arrangements as between choir, crossing, and nave. Perhaps a difference of period and a change of builders might explain them, if I were allowed to take a leaf from the sceptic's own book.† Another point which Mr. Bilson finds ridiculous is that the tower piers at Paris and Amiens do not diverge like the piers of the naves.‡ Might it not have appeared unsafe to diverge these piers in view of the heavy loading of the towers? There are many churches, however, in which the view toward the choir appears to have been the one most carefully considered. Generally speaking, the

* *Edinburgh Catalogue*, No. 15, with ill.; *Memoir* No. 4, p. 8, with ill.; *Architectural Record*, vol. xvi. No. 2, p. 122, with ill.

† *Memoir* No. 4, p. 65; a Renaissance instance dating 1638.

‡ The word "exhibit," where subsequently used, refers to a series of ninety enlargements, 20 × 24 inches, made during the summer of 1907, for use in the preparation of the latter part of this Reply, and for exhibition in the Brooklyn Museum. These exhibits were shown in September to various officers and members of the R.I.B.A. in London. A selection from these photographs was shown by M. Auguste Choisy at a session of the *Academie des Inscriptions et Belles-Lettres* in Paris on 23rd August.

§ *Edinburgh Catalogue*, Nos. 227, 228, 229, two ills.; *Memoir* No. 4, pp. 11, 12, two ills.; *Architectural Record*, vol. xvi. No. 2, pp. 128, 129, two ills. See also, later on, M. Aubertin's official certificate furnished in 1907 for publication in this Reply.

‖ Here there is also a constructive stepping back of the three upper bands of casing.

* Photographed in 1907.

† The crossing piers of the Cathedral of Pisa converge in their vertical curves.

‡ They diverge, however, in Rheims Cathedral, where the tower buttresses are much heavier than they are at Paris or Amiens.

effort to make the arrangement inconspicuous is apparent in the construction of the widening refinement. It is evident that the diminution of spread frequently found near the entrance has this result, and on the same principle the absence of spread in the tower piers at Paris and Amiens could be explained. I fancy, however, that the heavy loading of these piers is one important explanation. It is easy also to quote churches in which the system employed is uniform as between entrance, nave, crossing, and choir. Notable examples are St. Mark's and St. Mary Diaconissa.* Rouen Cathedral widens to the full amount (about 24 inches) at the entrance, and has very little widening at the crossing (three exhibits). The variety of the phenomena as found in the same church or as found in different churches is not all a matter for ridicule. It is a matter for serious study.

We return now to the characteristic feature of Notre-Dame for the present phase of the argument, viz. the divergence of the eastern crossing piers, as being fully as convincing as the widening at Trani and as representing the same kind of evidence. In Notre-Dame, as at Trani, if the facts are clearly understood and clearly realised, it is difficult to understand how any building expert can attribute these facts to accident. It must, of course, be understood that we are speaking of the eastern piers, because in them only the divergence starts at the pavement and is in lines which are practically straight. An outward thrust of the vaulting at the crossing could only operate by compressing the upper part of the transverse transept wall. This compression might conceivably result in a crumbling or displacement of the upper transverse wall, or the vaulting might conceivably compress the resisting transverse wall so as to bend or curve the crossing piers for some little distance downward, but that the force of thrust could operate to lean out the piers in lines which are practically straight from the pavement up, against the resistance of the transept walls, is positively inconceivable.

Another but analogous form of evidence is offered by the churches which widen vertically in the nave in straight lines, beginning at the pavement, against the thrust and massive weight of the aisle vaultings. In S. Ambrogio at Milan the piers which carry the vaulting shafts slant vertically outward throughout the nave from the pavement up and in straight lines, 6 inches on each side, against the thrust of the aisle vaults, which are very massive, and which carry massively constructed galleries at a height closely equal to that of the nave piers. Thus the piers of the nave widen 12 inches in straight lines in opposition to the aisle thrusts, and to the heavy loading of the galleries above them. That the vaulting thrust of the nave has carried these galleries and their heavy vaultings bodily outward 6 inches to a side is wholly impossible. In fact the suggestion appears absurd. That the nave vaultings have been partly repaired and partly rebuilt is well known, but it is expressly stated by de Dartein that the ribs of the nave vaulting retained their integrity and survived this repair.*

The Thomaskirche at Strassburg is another church of this class. The piers of the nave lean outward in straight lines from the pavement up, 5 inches to a side, against the thrust of aisle vaults, which are nearly as high as those of the nave. The piers of S. Ouen at Rouen incline outward, in straight lines from the pavement up, 6½ inches to a side (exhibit). Those of Rouen Cathedral incline outward a maximum of 12 to 13 inches to a side, in straight lines from the pavement up (two exhibits). In S. Ambrogio, in the Thomaskirche, in Rouen Cathedral, and a large number of other churches, a new form of evidence appears in the aisles, in which deep transverse chapel walls with massive responds are battered, so as to give a widening effect on the exterior side. The 1907 enlargement exhibit for the aisle responds of Rouen Cathedral shows a maximum inclination of over 8 inches in a height of 37 feet. The inclination is in straight lines, from the pavement up, against chapel walls which are 13 feet deep. It will be understood that the quotation of "exhibits" is confined to photographs made during the past summer, and that all buildings mentioned in this Paper, and many which are not mentioned, are represented in the Brooklyn Museum by enlarged photographs.

The same battering is found in the responds of transverse chapel walls exterior to the aisles, of other churches in which the widening of the nave begins, not at the pavement, but at the capitals.

We have thus reached the class of buildings to which the Amiens Cathedral belongs, those, viz., in which the outward divergence begins at the capitals of the piers of the nave, and, first and foremost, those are to be considered, like the nave and choir of the Cathedral of Amiens (as distinct from the curves of its crossing piers), in which the divergence is in straight lines from the capitals up.

Since at the height of the springing of the aisle vaultings there is an all-sufficient resistance to outward thrust from the nave vaulting, it is impossible that the nave vaulting thrust could operate in straight lines or even in a bent or curved line at so low a point, with sufficient force to carry the aisle vaultings bodily outward. This argument presumes, of course, that the piers of the nave are perpendicular; for if they have been pushed into the nave by aisle thrust then a return outward lean just above the springing of the aisle vault would be involved. That the nave piers up to the capitals

* The latter with the qualification that the widening is in straight lines at the entrance and in bends at the choir.

* F. de Dartein, *Étude sur l'Architecture Lombarde*, p. 134.

(as distinct from the vaulting-shafts) are generally perpendicular at Amiens is shown by twelve special exhibits [see fig. 1, p. 38]. Under these conditions we should expect to find the vertical bend in the nave at Amiens, if due to thrust, not lower down than the triforium string-course, rather than at the springing of the aisle vaulting where it actually begins (exhibits for each individual pier), in details which reach from the capital to the triforium [see fig. 2, p. 39].

The force of this consideration is much increased by the existence of a number of cases of this class in which the weight of one or more substantial galleries is added to the load which tends to resist an outward accidental lean in straight lines. At Laon, where the lower vaulted gallery is of great width, the widening of the nave begins in straight lines at the capitals of the piers, against the thrust of this vaulting and at a remarkably low height when compared with the height of the church (exhibit). At Strassburg the same widening in straight lines begins at the same related point. In Rheims Cathedral the same widening in straight lines begins at the same point (four exhibits) [see fig. 7, p. 50]. In Saint-Pierre at Caen the same facts appear.

Saint-Remi at Rheims is a church of this class. The widening begins at the capitals and rises in straight lines against the thrust of the aisle vaulting and against the thrust of a very wide vaulted gallery (exhibit).

Thus, when Mr. Bilson cites the nave vaulting of Saint-Remi as having been taken down and as being entirely rebuilt, the answer is that the nave vaulting could not have thrust in straight lines down to the mentioned point against the gallery vault and the aisle vault. Consequently the undoubted defects of the nave vaulting were independent of the quoted facts.

Although in St. Mark's at Venice the widening is not in straight lines, the facts are all the more pregnant on that account. The piers of the nave incline delicately outward from the pavement up. Thus we are positive that the strong recession immediately above the capitals is not due to the piers leaning into the nave. The strongest pitch in the widening is found directly above the capitals, against the thrust of the aisle vaults and against the weight of the galleries.

Thus in approaching a more specific examination of Mr. Bilson's criticism of my work at Amiens the following points, which appear to have been established, may be rehearsed. First, it is desirable not to confine the controversy to a single cathedral, because the apparent inherent improbabilities attaching to the announcement for that cathedral are considerably minimised when a greater number of churches are examined. Secondly, if the argument so far tends to show that the alleged refinement could not be produced by thrust in the quoted cases, the result also follows that such a system of construction would not aggravate or favour the disrupting tendencies which thrust is known to promote.

There is a natural prejudice that things "out of plumb" are unsafe, and Mr. Bilson's description of "walls, piers, and columns out of plumb" has an ugly sound, and this ugly sound was intended. But when we consider the actual thickness and weight of walls and piers in the buildings named, there does not appear to be anything alarming in the facts described. If the arrangements are such that thrust could not produce them, then it follows also they are arrangements which would not aggravate thrust or tend to endanger the building. Outside of St. Mark's, where the divergence is greater than usual, and is produced in the upper walls by battering, the extreme amount of inclination in any building mentioned has been some 11 or 12 inches to a side.* Now, when this amount of inclination is distributed through the entire height of the church or, in other cases, from the capitals up, the actual amount of lean in the clerestory wall is inconsiderable, and here only, if at all, could it be dangerous to the building. Whatever that small amount of lean might be, it would also evidently tend to forestall the upward lifting and grinding of the flying buttress which an accidental outward thrust and settlement would tend to produce.

In so far as piers have been found to be "out of plumb," the arrangement again tends to solidity, when the aisle thrust is considered.

When responds are "out of plumb," they face solid transverse walls and are again constructively stable.

When crossing piers are "out of plumb," they lean against a transverse transept wall, and the arrangement again promotes solidity or is certainly not antagonistic to it.

As to "columns out of plumb" I have never quoted any in the naves or constructive features of churches, or aside from arrangements similar to those in the clerestory of Notre-Dame, where the problem of solidity is not in question.†

Mr. Bilson might describe the sides of a Doric column as being "out of plumb," and this appears to be his way of looking at a slightly receding entasis such as we find in the responds facing the chapel walls in the Cathedral of Vicenza or in the slender pilasters in the angles of the choir at Laon. The transverse chapel walls at Vicenza are 25 feet deep, and the entasis has no more recession than that of a Corinthian column. Still these responds are undeniably "out of plumb," from 3 to 6 inches in a height of 30 feet! So are Palladio's pilasters "out of plumb" in S. Maria Maggiore at Venice and in the Teatro Olimpico at Vicenza. These

* What the amount is at Amiens will be considered presently.

† I make the usual distinction here between a column (monolithic as to diameter) and a pier (possibly round) composed of masonry, which I assume to be familiar to the terminology of Mr. Bilson.

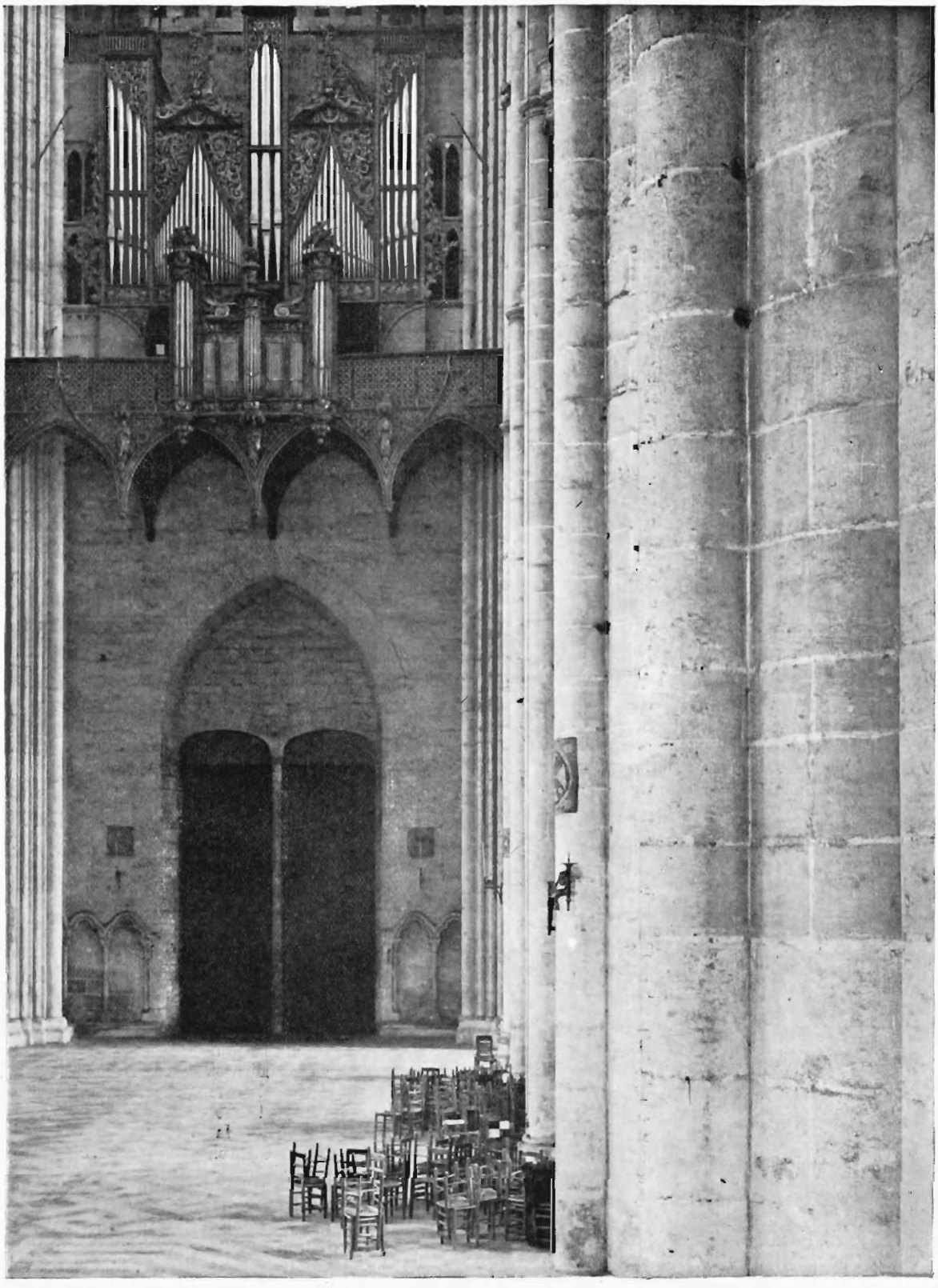

FIG. 1.—Plumb-line at pier No. 5 (Durand plan), north side of Amiens Cathedral (the fifth pier from the crossing, north side, in the numbering of this article). From No. 5 of the Brooklyn Museum enlargements for Amiens Cathedral, Series of 1907.

FIG. 2.—Plumb-line at vaulting-shaft, from capital to stringcourse, of pier No. 1 (Durand plan), north side, Amiens Cathedral (the sixth pier, north side, in the numbering of this article). The disc on the plane of the plumb measures 20 by 25 cm., or 8 by 10 inches. From No. 4 of the Brooklyn Museum enlargements for Amiens Cathedral, Series of 1907.

buildings offer a close and exact analogy to the facts in mediæval architecture; for the widening in the majority of mediæval cathedrals is not greater in relation to height than that obtained by the entasis in S. Giorgio Maggiore. At Noyon the curves of the nave are so delicate that they recede only 4 inches when the upper face of the piers is compared with the lower. The piers curve into the nave 1 inch and curve away from it 4 inches (exhibit). But Mr. Bilson assures us that this amount of widening is what led to the taking down and rebuilding by Pierre Tarisel of the nave vaulting! At all events, in Mr. Bilson's terminology, the curves at Noyon are "out of plumb."

The decisive proof that "refinements" exist at Noyon, even when enclosed in quotation marks, lies in the aisle responds, which face transverse chapel walls 17 feet deep, measured to the interior of the wall. They have an entasis of 2 inches recession in a height of 17 feet 8 inches (exhibit).

Thus we recur to Mr. Bilson's category in Class II., "Walls, piers, and columns out of plumb, sometimes straight, but more generally described as 'vertical curves.'" It is not worth while to quarrel about words, and Mr. Bilson has accurately described, according to his lights, the vista between two Doric columns. The "divergence" in mediæval churches is not relatively as great as it is at Pæstum. As regards vertical curves, the instance of the choir at Laon is absolutely impregnable in the matter of thrust (three exhibits). As usual in such cases, Mr. Bilson passes by this one without mention, probably because the "widening" in the central light of the choir is one of the most convincing proofs ever offered for the existence of this refinement. The central window widens 6 inches (two exhibits). I have already mentioned that Noyon, Saint-Remi, and Beauvais, which are the only churches attacked by Mr. Bilson, aside from Paris and Amiens, were not included in the Edinburgh Exhibition, and a final word may now be said about Beauvais.

If the contention be admitted that the widening system was very frequently employed, and that the constructive evidence is good in a considerable number of cases, it undoubtedly follows that some of the serious accidents which were bound to happen during the craze for extremely high vaultings must have happened to churches which employed this system. In the *Edinburgh Catalogue* I mentioned Beauvais as follows: "The accident at Beauvais is not to be overlooked, but it would be begging the argument to assume that a church in which accidental movement has occurred had no divergence before the movement took place."

A re-examination of this cathedral in the summer of 1907 shows that I have been in error in my estimates of the divergence there. It throws an interesting light on Mr. Bilson's disposition to argue that the widening refinement is always related to accidents in the vaulting that I am able to report that the cathedral whose vaulting fell twice has very little north and south widening at the crossing piers. The sixteenth century north-east crossing pier does not diverge from the perpendicular in the north and south directions, and it does not bulge; the south-east crossing pier has a delicate outward southward recession, as far as the thirteenth century portion of the pier is concerned, and does not bulge. The upper sixteenth century rebuilt portion does not diverge beyond the thirteenth century limit.

There is one point, however, which must not be forgotten, and that is to indicate the error into which Mr. Bilson has fallen in quoting this church. He says (p. 408): "So we have this curious fact, that Pierre Tarisel, who had executed at Amiens the costly and difficult work of inserting iron ties all around the cathedral in order to arrest the movement in the crossing piers and prevent their developing farther 'refinements' . . . was the expert called in to advise on the plans of the transept at Beauvais, which its architect, according to Mr. Goodyear, actually built with a widening even more pronounced than that which his adviser had been doing his best to arrest at Amiens." Mr. Bilson has forgotten his own quotation from the report of the Amiens Cathedral Commission of 1497–8, a commission including Pierre Tarisel, which is as follows: "The four principal pillars of the crossing of the said church are bent and arched on both sides by the thrust of the aisle vaults." The iron ties in the triforium at Amiens, according to Mr. Bilson's own quotation, were therefore intended to arrest a buckling below the triforium caused by "the thrust of the aisle vaults." The documents show that they had no reference to the widening above, which must have been due to a vaulting thrust from the crossing vault, if caused by thrust. At Beauvais the crossing piers have not buckled. Thus the satirical remark just quoted seems to lose its point, and neither Pierre Tarisel's consistency nor mine seems to be involved. Mr. Bilson ought not to forget, either, that the eastern crossing piers at Beauvais were originally built before the transept was planned, and that the south-east crossing pier is still thirteenth century work for three-fourths of its height.

As regards the iron cable in the triforium at Amiens, even if no documents were in question, it is evident that it would be useless as a means of arresting an outward spread above its own height. This appears from Mr. Bilson's own publication of the drawings in the workshop of the Amiens Cathedral, and thus we reach the debatable matters of fact at Amiens which are connected with these drawings.

Mr. Bilson's satirical remark about the accidental development of "refinements" in the Amiens crossing piers leads me to point out, as one among the weak spots of his argument for Amiens, his total neglect of the nave as regards attested and accurate observations, and his reliance on two

drawings made thirty-two years ago, in 1875, of the two southern crossing piers, for the amount of divergence throughout the cathedral. He assures us, regarding the nave (p. 415), and without offering any evidence whatever for his assertion, that "the divergences are so small that they are not appreciable to the eye, and whatever may be their precise extent they are certainly not greater, and probably are much less, than those of the crossing piers." If Mr. Bilson had "sighted on a plumb-line" he would hardly have ventured this statement, which I shall presently show to be glaringly inexact.

"Sighting on a plumb-line" occurs in quotation marks, which are supposed to be satirical, several times in Mr. Bilson's criticism. It is not, however, more ridiculous to sight on a plumb-line than to sight on a level. There is no other method of determining facts about the vertical lines of piers, excepting to sight on a plumb-line, unless the line can be hung directly against the pier. Mr. Bilson knows very well that this cannot be done in the Amiens nave, or in any great cathedral nave, without enormous expense. This is obviously the reason that he and M. Durand confined their own research to the publication of the drawings for the two southern crossing piers which were made thirty-two years ago, instead of taking measurements of their own throughout the cathedral nave.

In the Amiens transepts lines can be hung from the triforium which can be sighted on the piers if let down in front of them, but lines cannot be hung against these piers without a scaffold, unless a surveyor is slung on a platform by ropes let down through the apertures of the vaulting. In the transepts, if the plumb-line is sighted on the edge of a pier at the capital, the amount of vertical divergence below the triforium, to the height of the capital of the pier, can be estimated with approximate accuracy from the known diameter of the outer rib at the base. Supposing that the line, when sighted from the edge of the pier at the capital, cuts off about a quarter, or about a fifth of the diameter of the outer rib at the base, and that this diameter is 15 inches, then we obtain a rough estimate of the vertical divergence as a quarter or a fifth of 15 inches.

There is a further and complete accuracy obtainable—viz. by photographing the sighting, as taken under the conditions described, with a surveyor's rod and disc on the plane of the estimate to be made, which, in the Amiens transepts, would be at the rib of the pier. All my estimates in the Amiens transepts which were made in 1903 were verified in this way, excepting in those cases where the monuments placed in front of the piers interfered with hanging a line from the triforium. This method of photographic measurement, with a plumb-line "sighted" by the camera (if Mr. Bilson will continue to accept a phraseology which appears to amuse him), has the great advantage of dispensing with the necessity of hanging a line against the surface, which is impossible in any great cathedral as regards the piers, without a prohibitive expense.

So far I have explained the methods which I adopted in 1903 for "sighting on a plumb-line" in the Amiens transepts, and these methods are not only practical and defensible when the observations are attested and corroborated by enlarged photographs, but they are also the only methods which are practically available at Amiens in view of the enormous dimensions and altitudes of this cathedral. On the other hand, in the case of a plumb-line suspended in the Amiens nave, the openings in the vaulting near the crossing do not admit of an adjustment of the line which will allow accurate sighting (this adjustment is obtained in the transepts by having an assistant in the triforium who moves the line to the indicated sighting point). In spite of this apparent difficulty a carefully levelled photograph will allow of an accurate estimate, because the distance of the line from the edge of the pier at the base can be subtracted by compass measurement from the distance of the line from the edge of the pier at the capital. For this operation it is desirable, however, to have an enlargement. (For those sceptical persons who do not know that Goerz and Zeiss lenses are rectilinear it is sufficient to point out that a lens which distorts the pier will also distort the plumb-line, so that the punishment for taking an unreliable photograph will immediately fit the crime.)

During my first survey visit to Amiens (in 1903) which lasted only five days and a half, most of my time was devoted to the transepts, as the nave did not appear to vary from the facts elsewhere known to exist. On the fifth day of my work, the sacristan, M. Regnaut, suggested hanging a line through an opening in the vaulting of the nave. I gladly adopted the idea, which I had never previously tried. A black tape was used, which had served its purpose successfully in the transepts. The next step was to photograph this line, which was hung for that purpose, and then from the known diameter of the outer rib of the crossing pier to estimate the divergence of the crossing pier, after an enlargement had been obtained. It was, however, my first experience in suspending a tape from such a lofty height, and at the great distance from the camera, placed in the organ gallery at the west end of the cathedral, which this altitude involved. I did not realise that a tape will not show under such conditions, even in the most excellent negative, unless suspended in front of a light background of masonry surface. The negative was reported to be excellent by the Amiens photographer who developed it, and I left Amiens the next day; but when the negative was printed in Paris the tape could not be seen.

This is an exact account of my first plumb in the Amiens nave, and the only one taken until 1907,

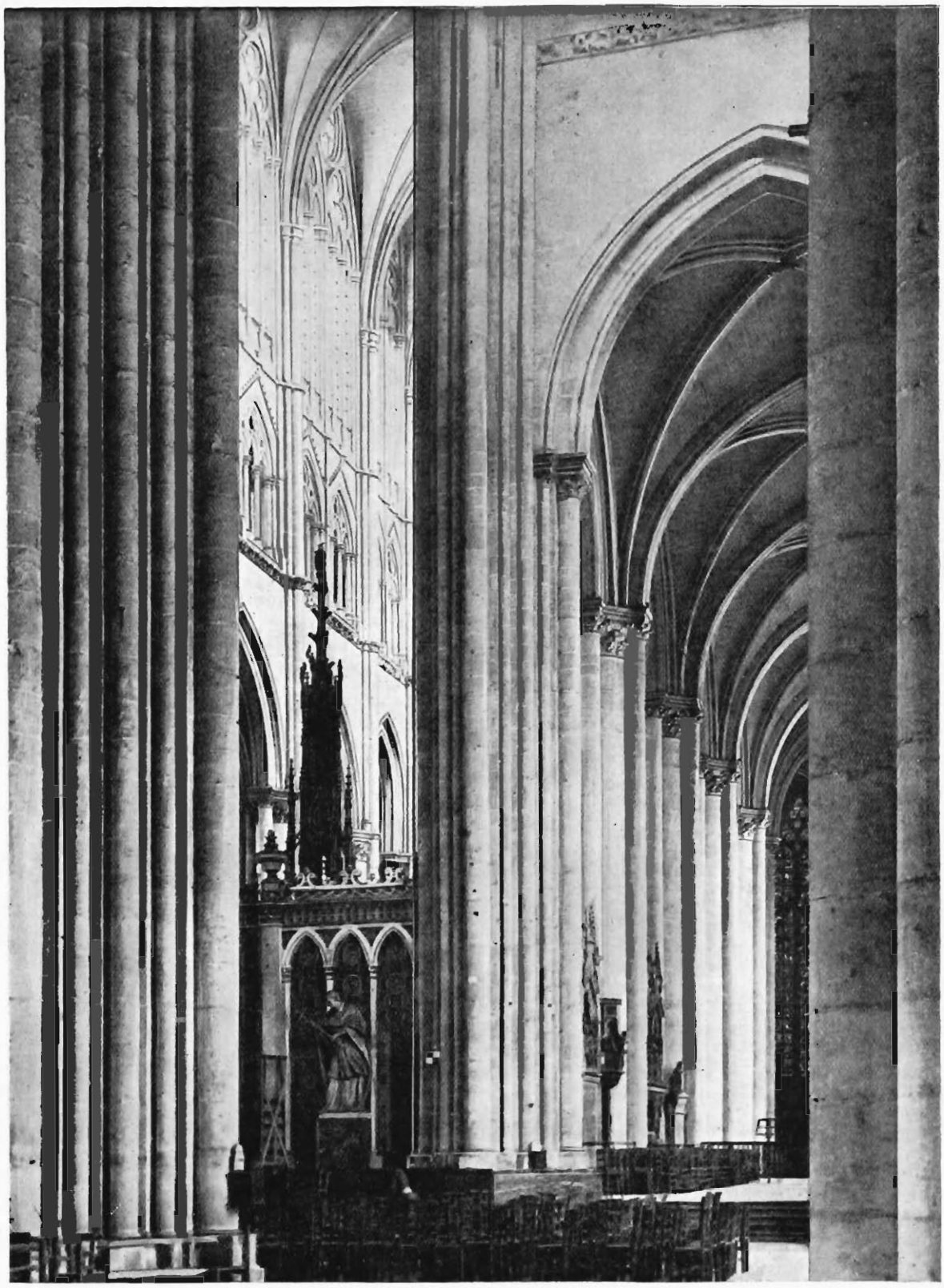

FIG. 3.—Cable plumb on the South-east Crossing Pier, showing the lean into the nave of the north face. From No. 65 of the Brooklyn Museum enlargement exhibits for Amiens Cathedral, Series of 1907.

when I have photographed over two hundred plumb measurements in the cathedral, a large proportion of which have been in the nave. Some fifty plumbs in the nave furnish photographic exhibits in the way of enlargements, made for reference in preparing this reply to Mr. Bilson. Meantime, and before the work of 1907, I have quoted an estimate which my photograph of 1903 would have guarded me from publishing if it had served the purpose for which it was taken. Strictly speaking, it was not possible to sight on the nave plumb-line of 1903, for it was necessarily some little distance removed from the crossing pier toward the centre of the nave. I remember using the known diameter of the rib as the basis of my estimate, but may have omitted to subtract the distance of the line from the base of the pier, in subsequent use of my notes. There is a remarkably strong optical illusion, due to the vaulting ribs, which very much exaggerates the appearance of recession at the Amiens crossing, which illusion can only be rectified by photographic compass measurement.

Mr. Bilson's success in making off-hand estimates of the divergence in the Amiens nave has not been phenomenal, for he is on record, as just quoted, for the assertion that this divergence is "inappreciable by the eye," whereas its maximum amount is 19 inches or over. Mr. Bilson has also apparently been the victim of the optical illusion occasioned by the vaulting ribs of the aisles which cause the piers to appear to lean into the nave. This is indicated by his assertion that "some movements have taken place in the nave caused by the thrust of the aisle vaults inward." It is now demonstrated by exhibits submitted for each individual pier of the nave (as distinguished from the crossing), and with a distinct and separate plumbing for each pier, that every pier on the north side is strictly perpendicular to the height of the capital [see fig. 1], and that two piers of the south side are strictly perpendicular (those next the crossing). In the remaining four piers the inward inclination up to the capital varies from less than 1 inch to $1\frac{3}{4}$ inches as a mximum. Thus the statement that "movements have taken place in the nave caused by the thrust of the aisle vaults inward" appears to rest on an optical illusion which I cannot myself overcome without the aid of the plumb-lines which Mr. Bilson has so scornfully and so imprudently rejected in his own observations.

For the student of Mr. Bilson's criticism I cannot offer a more convincing proof of his delinquencies as an observer than the extraordinary oversight which has led him to publish as reliable documents the drawings of the southern crossing piers, which make such an imposing appearance in his Paper. M. Durand has also been overhasty in his confidence in these drawings. That they are carefully executed is perfectly true, but that they are also unreliable is easily shown. If Mr. Bilson and M. Durand will "sight on a plumb-line" at the south-east crossing pier of Amiens Cathedral, they will immediately admit that these drawings are not reliable. For instance, the north lower face of the south-east crossing pier leans into the nave 3 inches (0·075 m.; two exhibits, Nos. 29 and 65), whereas the cathedral office drawing represents the north lower face as perpendicular [see fig. 3].

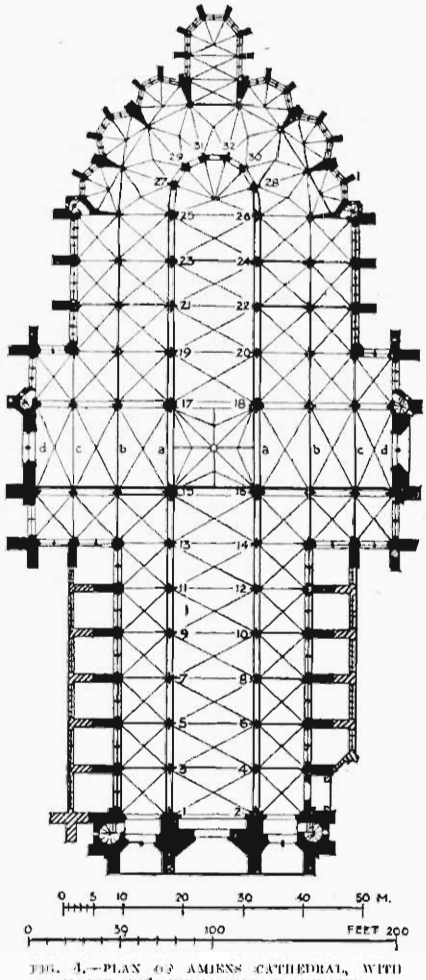

FIG. 4.—PLAN OF AMIENS CATHEDRAL, WITH M. DURAND'S NUMBERING OF THE PIERS.

Aside from the exhibits represented by fig. 3, I had made two photographs of this inclination in 1903 (after "sighting on a plumb-line") with the line in position, and a surveyor's rod beside the pier, to furnish the measurement. I had had these photographs enlarged to 25 by 35 inches for the Brooklyn Museum exhibit, and they are catalogued and described in the *Museum Memoir* No. 4 (Nos. 54-55), a publication which I sent M. Durand, and which is quoted by Mr. Bilson. Therefore, when the drawings furnished Mr. Bilson by M. Durand came to my notice in the JOURNAL R.I.B.A. I was inclined to view them with great suspicion, as probably containing other errors beside the one which was most obvious, and this suspicion has been verified. The drawing for the south-west crossing pier is in error for its upper measurement, north face, to the amount of $1\frac{1}{2}$ inches (0·04 m.), as shown by exhibit 37. The measurement should thus be corrected to 0·14 m. instead of 0·10 m. Since the drawing of the south-east pier is in error 0·075 m. for the lower face, the upper measure is to be corrected from 0·125 m. to 0·20 m.

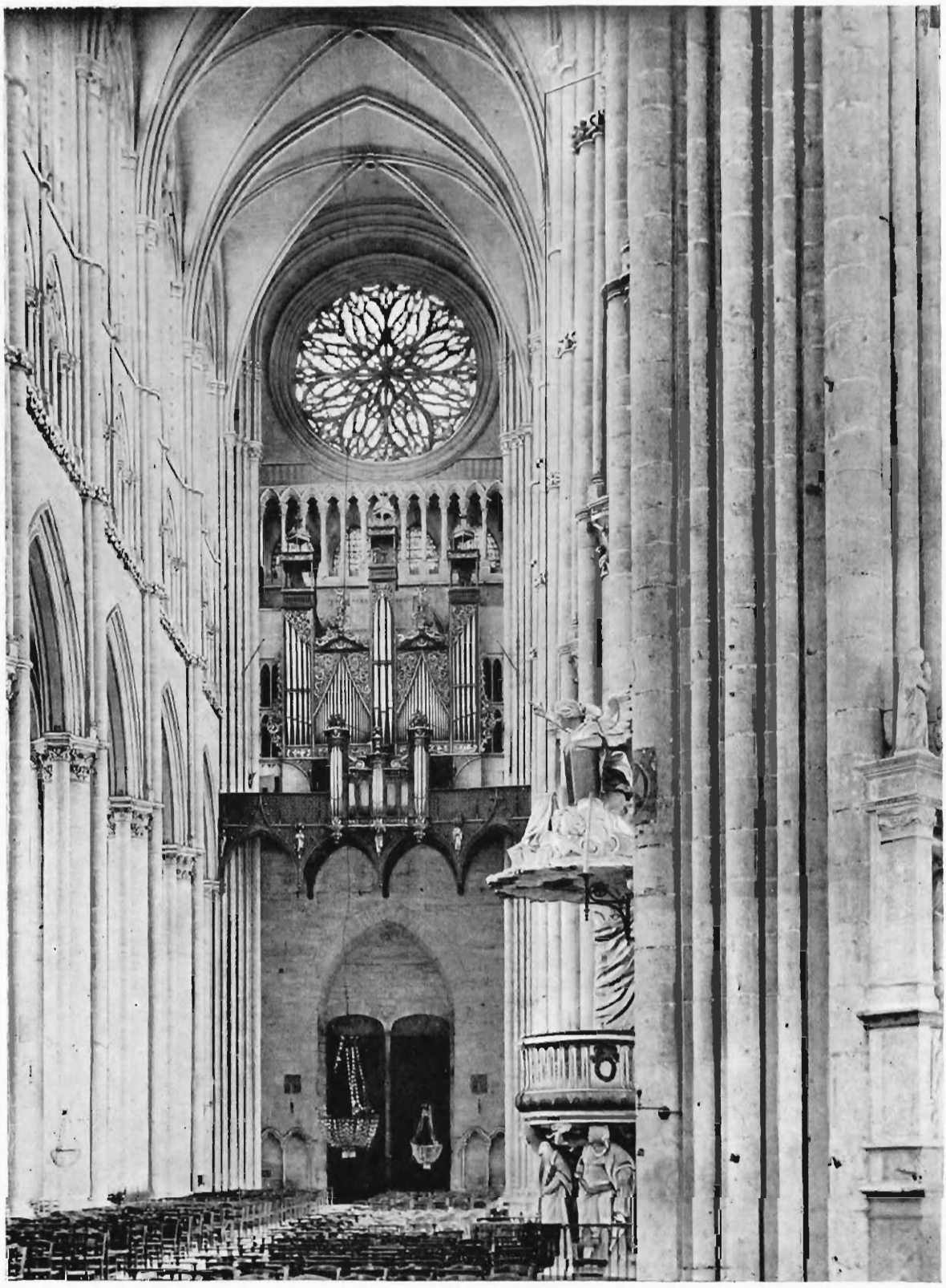

FIG. 5.—Plumb on piers 7 and 5 (Durand plan), north side (Nos. 4 and 5 in the numbering of this article). Surveyors' discs (8 by 10 inches) on the planes of measurement. From No. 42 of the Brooklyn Museum enlargements for Amiens Cathedral, Series of 1907. The plumb-line is concealed between the piers below the capitals, but these piers are shown as perpendicular to the height of the capital by fig. 1.

Exhibit 37 is represented, in much diminished size, by fig. 6. The reduction from 20 × 24 inches to 6 × 8½ makes the application of the compass test, based on the surveyor's disc to the left of the pier, an over-delicate operation, but the measurement of 5 inches for the divergence of this south-west crossing pier may be tested on fig. 6 with this warning. My own measurements are naturally based, in all cases, on the enlargements and not on the illustrations of reduced size. Fig. 3 displays more clearly the error of the office drawing for the south-east crossing pier, because the dimension is doubled for the given height.

I have not yet tested the accuracy of these drawings for the east and west sides of the crossing piers, the sides which face the transept, as this question does not concern the present argument, but each drawing is in error for the northern faces. When enlargements are made of the photographs of my north and south transept plumbs the test for the east and west faces will be an easy one. It ought to be added that the taking of plumb measurements from platforms slung from ropes let through the vaulting is an operation of great difficulty, and almost infallibly liable to error, and that the office clerk of 1875 who did or supervised this work had no easy task. Mr. Bilson's haste in accepting these rather ancient documents, in preference to making tests of his own, is possibly explained by the great expense and great difficulty of taking actual plumbs or by his contempt for scientific photography.*

M. Favry, the *Inspecteur des Travaux* at Amiens, is now aware of the unreliable character of these drawings, and I took pains to assure myself, with his assistance, that they have been correctly copied, and that the errors are in the originals. As just indicated, I have prepared two special photographic enlargements to illustrate the remarkable error in the drawing for the south-east pier, and one enlargement is submitted to show the error for the south-west pier. However, in spite of the premature and rather indiscreet hilarity of some portions of Mr. Bilson's Paper, I owe him much sincere gratitude for his article, especially so because the fact, immediately forced on my attention, when this article appeared, that the drawings furnished by M. Durand were not trustworthy, has obliged me to make a new and thorough study of the vertical measurements at Amiens, and has enabled me to correct my own errors as well as those of Mr. Bilson.

My photographic measurements of 1907 for the divergences of the crossing piers are a great deal less than my estimate of 1903, which was not verified by photographic methods, for reasons explained, and I am vastly pleased to be able to correct that estimate in the table of measurements which follows.

This table of measurements for the vertical divergences of the nave and crossing piers is supported by thirty-eight exhibits, to be subsequently enumerated and described. In my own numbering of piers I have continued the methods which I used in my photographic notes, but the numbers of the Durand plan are also supplied, as found in fig. 4. My own numbering enumerates the piers in order from the crossing, not included, as far as the organ gallery and the tower piers at the entrance, which are also not included, as having no divergence.

RECESSIONS IN THE PIERS OF THE AMIENS NAVE AND CROSSING (IN INCHES).

All recessions, except at the crossing, are in straight lines, and all begin at the capitals, or, in the case of the crossing piers, at the corresponding height.

NORTH.	SOUTH.
East Crossing (17).* Recession, 5; bulge, 1¼.†	*East Crossing* (18). Recession, 5; bulge, 3.
West Crossing (15). Recession, 5; bulge, less than 1 inch.	*West Crossing* (16). Recession, 5; bulge doubtful, if any.‡
First Pier (13).‡ Shaft recession, 7; pier perpendicular.	*First Pier* (14). Shaft recession, 5; pier perpendicular.
Second Pier (11). Shaft recession, 8⅛; pier perpendicular.	*Second Pier* (12). Shaft recession, 6¼; pier perpendicular.
Third Pier (9). Shaft recession, 8⅞–8; pier perpendicular.	*Third Pier* (10). Shaft recession, 7–6; pier leans in, 1¾.
Fourth Pier (7). Shaft recession, 9½–9; pier perpendicular.	*Fourth Pier* (8). Shaft recession, 5¾–6½; pier leans in, 1.
Fifth Pier (5). Shaft recession, 12–11½; pier perpendicular.	*Fifth Pier* (6). Shaft recession, 7½–6½; pier leans in, 1¼.
Sixth Pier (3). Shaft recession, 7½; pier perpendicular.	*Sixth Pier* (4). Shaft recession, 6½; pier leans in, 1¼.

It will be noticed that double estimates, offering slight variations, are given for the third, fourth, and fifth piers on each side (Durand plan 9, 10; 7, 8; 5, 6). The right hand measurements in

* M. Choisy has expressed the opinion to me that my own photographic methods for such cases are much more reliable than actual plumbs from scaffolds or slung platforms.

* These numbers relate to M. Durand's plan, as reproduced in fig. 4.

† The recessions are figured without including the bulges; in other words, they are figured from the base to the capital at the vaulting ribs. This method differs from that of the cathedral office drawings. On that method the recession would be 6½ inches for the north-east crossing pier and 8 inches for the south-east crossing pier.

‡ The cathedral office drawing gives 0·15 m. bulge, or less than ⅝ of an inch. If a bulge exists, it is certainly not more. My own observation shows a slight builder's error in some of the masonry courses rather than a bulge. The photographic detail enlargement will enable each student who examines it to form his own conclusions. M. Choisy's impression of this enlargement coincides with my own.

FIG. 6.—Plumb on the South-west Crossing Pier. Amiens Cathedral (No. 16, Durand plan). From No. 37 of the Brooklyn Museum enlargements for Amiens Cathedral, Series of 1907.

these cases are from photographic surveys, with plumb-lines reaching, in the photograph, from the base to the capital of the shaft at the vaulting, and with a surveyor's disc on the plane of the given pier at its exact centre. Fig. 5 (p. 44) represents in much reduced size an enlargement of this class. The left-hand measurements for the same piers are figured on a wholly different method, which was also applied to all the piers for which single measurements are quoted—*i.e.* by combining the measurement furnished by a specially enlarged photographic detail of a plumb with disc measurement from the triforium string to the capital of the pier (where the outward slant begins); with another estimate for the triforium and the clerestory based on actual plumbs inside the triforium. As the divergences are visibly in straight lines and are attested to be so by a very large number of enlarged details, and other special photographs, the method has been followed of computing the divergence of the upper triforium and the clerestory from an actual plumb with line of 15 feet at each shaft in the triforium. The close correspondences of measurement obtained by these wholly different procedures are notable. The method of the combination estimates has been applied to the first, second, and sixth piers, and these are the only estimates offered for these piers. Fig. 2 represents the type of enlargements for the vaulting-shafts between the capital of the pier and the triforium string. The measurements for the recession of the crossing piers are obtained from plumb-lines, reaching in the photographs from base to capital at the vaulting. Fig. 6 represents this type of enlargements. The measurements for the bulges of the crossing piers are obtained from photographic enlarged details, separately taken, for the height from the base to the level of the triforium string.

The estimates published are verified, as stated, by thirty-eight exhibits of photographs enlarged to 20 × 24 inches, made up as follows: Eight details to a side for the piers of the nave up to the capital of the piers—*i.e.* to the springing of the aisle vaults, and for the crossing piers to the triforium string; six details to a side for the vaulting-shafts of the nave, from the capitals of the piers to the triforium string; five photographs to a side for the third, fourth, and fifth piers, and for the crossing piers, taken from the base to the capital of the shaft at the vaulting—each and every photograph of the entire series with plumb-line, and with a surveyor's disc on the exact plane of the desired measurement. As far as the illustrations of the present article are concerned, the method of obtaining the measurements from the disc is best shown by fig. 2, because the disc appears there in larger dimensions than in the other illustrations.

It will be noticed that the measurements on the south side of the nave are invariably considerably smaller than the corresponding ones on the north side, excepting at the crossing, where the recessions correspond in all four instances (although the bulges do not). It is also worth noting that, although the divergences are all greater on the north side, it is the piers of the north side which are all accurately perpendicular. The measurements of the south side, which are taken from the base to the vaulting for the third, fourth, and fifth piers, eliminate, however, the slight leans into the nave of the corresponding piers.

All the vaulting-shafts of the north side step forward slightly (about one inch) at the capitals of the piers; a fact shown by every photographic detail for the north side. An inch is therefore subtracted from each combination estimate for the north side. This stepping forward is found in only one pier of the south side—the sixth in my numbering, No. 4 of the Durand plan (the pier next to the organ gallery and to the tower pier).

The fifth vaulting-shaft on the north side steps back two inches at the clerestory, and is the only one which steps back on either side (exhibit No. 42). Thus two inches are added to the combination estimate for the fifth vaulting-shaft, north side (No. 5 Durand plan), in order to obtain a just comparison with the photographic measure, from base to vaulting of the same pier. The two distinct estimates then tally within half an inch.

I am now able to return to an argument which Mr. Bilson has contemptuously waived aside, after declaring (p. 415) that "the divergences (in the nave) are so small that they are inappreciable to the eye, and whatever may be their precise extent they are certainly not greater and probably are much less than those of the crossing piers."

If the Amiens nave has spread accidentally, in amounts varying from 12 to 19 inches, the crown of the vaulting-arches should have subsided at least one half the given amount of divergence. If these vaulting-arches have gone down they must have become distorted, and if they have been distorted that distortion must be visible. Mr. Bilson lays great stress on the extent to which a vault may settle without falling in, but he does not, and cannot, say that such vaultings have not been depressed and distorted. Now the Amiens nave vaulting is true throughout to a single pitch, which has visibly not gone down at the crown (exhibit 49 and several others). There is a slight distortion of the ribs of the vaulting which correspond to the fifth pier north of my numbering (No. 5, Durand plan), but there is no subsidence there at the crown.

M. Durand's excellent book gives us to understand that the Amiens nave vaulting has never been repaired. Viollet-le-Duc says in his *Dictionary* (article Cathedral): "Cependant cette nef dont la hauteur est de 42.50m. sous-clef et la largeur

d'axe en axe des piles de 14.60m. ne s'est ni déformée ni déversée. La construction n'a subi aucune altération sensible."

Mr. Bilson will hardly say that the Amiens vaulting could settle downwards 10 inches at the fifth pier (one-half the widening at that point) without calling for some repairs, at some time or other, or without showing some signs of depression and distortion which would have attracted the notice of Viollet-le-Duc, and which would also be visible now.

But there is a better argument than this, because it is not an argument, but a palpable fact. It is a fact which leaves on one side all debate as to measurements, which must be extremely tedious to everyone but Mr. Bilson and myself.

A nave vaulting thrust cannot push outward the surfaces and vaulting shafts below the triforium, because below that line it cannot push bodily outward the enormous masses of the aisle vaultings and their buttresses. If such a thrust were possible, it can certainly never be suggested that it could operate in straight lines, and in uniformly flat surfaces, from start to finish. Hence I leave this part of the subject with a final reference to the twelve exhibits for the perpendiculars of the piers of the nave, which prove that the recessions, beginning in all cases directly above the capitals, are not caused by inclination of these piers into the nave. I also call attention to the twelve exhibits for the outward slope between the capitals and the triforium, as proving that this slope begins invariably at the capitals. In other words the entire surface of the masonry has a constructive batter up to the triforium string.

As regards the transepts at Amiens, Mr. Bilson is undoubtedly right about the tapering of piers, and he is just as undoubtedly wrong about the leaning windows and their mullions. He speaks of one; but there are two, one in each transept, with sills which are oblique in directions opposed to the lean of the mullions (four exhibits, Nos. 53–56). It is only quite recently that I have mastered a real knowledge of the Amiens transepts (sixteen exhibits, 50–64 and 59A). This knowledge I will endeavour to convey to Mr. Bilson and M. Durand in some future publication.

There are numerous points in Mr. Bilson's argument about Amiens which I have not noticed, but there are none which I have not covered. He accuses me of neglecting to notice the repairs of the choir. What is the use of considering them when there have been no repairs in the nave vaulting? His argument on the crossing piers also has no point, because it does not cover the nave, which, aside from vague and erroneous assertions, he has entirely neglected. The cracked and broken lintels in the triforium have been explained by M. Durand in a most interesting and conclusive way, which has escaped Mr. Bilson's attention. Suffice it to say that M. Durand proves that they are not due to thrust from the nave vaulting.*

I may add that Mr. Bilson has shown a certain lack of caution in one of the closing paragraphs of his Paper. He says: "It may be well to see how these theories of his have been received in France by those most competent to pass an opinion upon them," then quoting the names of three distinguished scholars, none of whom are engineers.

May there not be other French scholars who are also both engineers and architects who have formed and publicly expressed a more favourable opinion? Mr. Bilson takes much upon himself in undertaking to state so summarily and so absolutely what the opinions may be of those in France who are most competent to pass an opinion on my work. I commend to Mr. Bilson's attention a pamphlet which I have recently published on the Cathedral of Rheims, containing some extracts from a letter addressed to me by M. Auguste Choisy (one among very many letters from him which are full of friendly encouragement and praise).

I will not follow Mr. Bilson farther in the dangerous path on which he has entered. I will only say that MM. Lefèvre-Pontalis and de Lasteyrie are gentlemen from whom it is a real pleasure to differ, and I commend to Mr. Bilson a more intimate study of their polished and delicate style of dissent. When choice of weapons is allowable, I much prefer the rapier to the club.

Another and not less extraordinary phase of Mr. Bilson's effort to discredit my work in France is his closing sentence: "When the other French churches investigated by Mr. Goodyear have been the subject of monographs as exhaustive as that of M. Durand on Amiens, there will be no difficulty in producing equally conclusive proof that their 'refinements' exist only in Mr. Goodyear's imagination."

I am not aware of anything in M. Durand's book which antagonises my observations at Amiens. On the contrary, this book has been of the greatest possible assistance to me. It is my main authority, aside from Viollet-le-Duc's statement, for the fact that there have been no repairs or rebuilding in the vaulting of the Amiens nave, which is to my argument a fact of considerable moment. M. Durand's book was written without knowledge of my investi-

* *Cathédrale d'Amiens.* Georges Durand. Tome 1er, page 222: "Les têtes des arcs boutants sont soutenues par les colonnes engagées 1, k, qui appartiennent au maitre pilier; elles sont raidies par les deux colonnes superposées G., H., formées de pierre en délit, s'élevant à environ 60 centimètres en avant des colonnes engagées 1, k, et posées sur le mur de clôture du triforium renforcé d'un petit contrefort en porte à faux sur le mur élevé sur l'arc de décharge qui passe par dessus le doubleau du bas côté. Ce petit contrefort a été relié au maitre pilier par deux linteaux monolithes E. F. formant parpaing."

Note 1. "La compressibilité des parties intérieure et postérieure du pilier se trouvant inégale par suite de la rigidité de la colonne G, qui est en délit, *un certain nombre de linteaux E se sont brisés.*" (*Italics* by W. H. G.)

gations, which were not published for Amiens or for France until some months after his book appeared. The book was also evidently written without any knowledge of the true amount of widening in the Amiens nave, inasmuch as M. Durand furnished Mr. Bilson with drawings for the crossing piers, which are presumed by Mr. Bilson to show a greater widening than occurs in the nave. M. Durand evidently has held the same mistaken opinion. Consequently it again appears that the topic of a widening in the Amiens nave is quite foreign to M. Durand's book. That M. Durand personally does not accept my views about the Amiens Cathedral is a fact for which Mr. Bilson's statement must be accepted; but since M. Durand's book does not debate the subject which I have agitated, how can it offer "conclusive proof" that I am mistaken?

Clearly, in Mr. Bilson's opinion, it must be M. Durand's history of repairs which indirectly discredits my opinions. But this same history of repairs is my authority for the statement (aside from Viollet-le-Duc's utterance) that the nave vaulting has never been repaired. If the nave vaulting had been rebuilt or repaired, M. Durand would have known it and M. Durand would have mentioned it. That is what his book is for. Truly a most excellent book! And yet I fear that this same good book may be the cause of M. Durand's opposition to my views. It is not easy for an author who has written the finest cathedral monograph extant (aside from Cattaneo's text in the Ongania publication for St. Mark's) to admit that he has omitted to mention in that book the most interesting feature of the Amiens Cathedral, and this is exactly what M. Durand has omitted to do.

From another point of view, I consider Mr. Bilson's fratricidal effort to crush me under the weight of M. Durand's ponderous two volumes equally extraordinary: "When the other French churches investigated by Mr. Goodyear have been the subject of monographs as exhaustive as that of M. Durand, there will be no difficulty in producing equally conclusive proof that their 'refinements' exist only in Mr. Goodyear's imagination."

This is placing M. Durand on a pedestal from which he will certainly be in great danger of falling as soon as it appears that these "refinements" exist also "in the imagination" of (let us say, for example) the *Inspecteur des Travaux* at Amiens or the *Inspecteur des Travaux* at Rheims.

Now, this is exactly what has happened, and this leads me to quote the following letter from the *Inspecteur des Travaux* at Rheims:

ADMINISTRATION DES CULTES. EDIFICES CULTUELS ET MONUMENTS HISTORIQUES.

Agence des Travaux, Reims, le 3 Juillet 1907.

MONSIEUR,—Je suis heureux d'avoir pu constater et avec beaucoup d'intérêt certaines déformations à notre Cathédrale.

En effet, et ainsi que vous avez bien voulu me le faire remarquer, j'ai pu voir qu'à Reims les piliers de la grande nef avaient conservé leur verticalité, mais qu'à partir des chapiteaux ils se déversaient graduellement vers l'extérieur jusqu'à la naissance des voûtes.

Ces déformations remarquées également par vous dans d'autres édifices de la région me paraissent voulues par les architectes chargés d'édifier ces monuments et avoir été faites en construisant.

Elles ne sauraient être attribuées au manque de résistance des arcs boutants très puissants à Reims; d'autant plus que ces mêmes déformations sont constatées sur tous les piliers et même aux environs du transept où nous avons des maçonneries rendant toute poussée des voûtes impossible.

Il faut voir là, à mon avis, certains effets voulus de grandeur, tout à l'avantage de l'édifice.

Veuillez agréer, Monsieur, l'expression de mes sentiments distingués. L. MARGOTIN,

Architecte diplômé du Gouvernement,
Inspecteur des Travaux à la Cathédrale de Reims.

M. WM. H. GOODYEAR.

Certificate of M. Leon Margotin. [*Translation.*]

SIR,—I am pleased to have been able to verify, and with great interest, certain distortions (*déformations*) in our cathedral.

In fact, and as you have been kind enough to point out, I have been able to observe that at Rheims the piers of the nave have preserved their perpendicular, but that, above the capitals, they diverge gradually toward the exterior up to the springing of the vault.

These distortions, also observed by you in other buildings of our territory, appear to me intended by the architects who were charged with the construction of these monuments, and to have been arrangements of construction.

They cannot be attributed to a lack of resistance in the powerful flying buttresses at Reims, especially since these same distortions are verified in all the piers and even near the transepts, where the masonry constructions make any thrust from the vaults impossible.

In my opinion, we must recognise certain effects of grandeur as having been purposed, which are wholly to the advantage of the edifice.

Please accept, Sir, the expression of my cordial regard.

L. MARGOTIN,
Architecte diplômé du Gouvernement,
Inspecteur des Travaux à la Cathédrale de Reims.

These "refinements" also "exist in the imagination" of an architect who is intimately acquainted with the Church of St. Loup at Châlons, as appears from the following letter:

A. AUBERTIN, *architecte.*

Châlons-sur-Marne.

Je soussigné, Aubertin, Albert, Architecte à Châlons-sur-Marne, auteur de la flèche et autres réparations à l'Église Saint-Loup, estime que les hors d'aplomb très visibles sur les quatres gros piliers du transept et sur les piliers des chapelles latérales ne sont pas le résultat de poussées, mais que cet état date de la construction de l'Église.

Il y a là un effet voulu par le constructeur, les piliers et les arcs forment ainsi une sorte de fer à cheval, destiné à corriger ou plutôt à modifier les effets de la perspective.

Le hors d'aplomb part de la base des piliers; aucuns désordres ne se manifestent aux alentours, ni dans les murs, ni dans les arêtes des arcs doubleaux et autres voûtes. Fait à Châlons le 27 juin 1907.

A. AUBERTIN.

Fig. 7.—Plumb-line (suspended from a bamboo pole), north side of the nave, Rheims Cathedral. From a Brooklyn Museum enlargement, Series of 1907.

Certificate of M. Aubertin. [*Translation.*]

A. AUBERTIN, *architect.*

Châlons-sur-Marne.

I, the undersigned, Albert Aubertin, Architect at Chalons-sur-Marne, author of the spire and other works of repair on the Church of St. Loup, consider that the very visible inclinations of the four great piers at the transepts, and of the pillars of the aisle chapels, are not the result of thrust; but that their condition dates from the construction of the church.

Here is an effect designed by the builder, and the arches thus display a sort of horse-shoe form, destined to correct, or rather to modify, the effects of perspective.

The inclinations begin at the bases of the piers; there are no defects in the adjacent masonry, either in the walls or in the vaulting ribs or vaultings.

Given at Châlons, 27th June 1907.

A. AUBERTIN.

Not only do these "refinements" thus appear "to exist in the imagination" of the *Inspecteur des Travaux* at Rheims and of an architect at Châlons, but, sad to relate, the *Inspecteur des Travaux* at Amiens, M. Favry, who furnished the drawings of the crossing piers to M. Durand for Mr. Bilson's use, is another dangerous lunatic. Not only do the "refinements" now "exist in his imagination" at Amiens, but he even has "theories" to explain them! The full account of M. Favry's conversion to my dangerous heresies would make an interesting story. Suffice it to say that during fourteen weeks, mainly spent in Amiens, the intercourse between M. Favry and myself was limited to the initial interview in which he most courteously provided me with the assistance needed for my work.* Meantime (knowing also that he had furnished the drawings of the crossing piers to M. Durand), I gathered from a slight coolness on his part during casual and accidental meetings that he was not an enthusiastic believer in the "widening refinement," which, however, we did not discuss. At the end of fourteen weeks my ammunition was in readiness in the shape of sixty-five enlargements of the Amiens Cathedral. These were transported to the Sacristie des Chanoines, and M. Favry was invited to inspect them. "*He* came, *he* saw, and *I* conquered." M. Favry and I did not on that day, or previously, inspect the cathedral itself, and we have never been together in the Amiens nave. He is certainly as familiar with that nave as anyone can be, as far as eyesight is concerned; and I tell this story because I know no other way of putting the point so clearly that a true knowledge of the measurements for the verticals in any great cathedral can only be obtained or conveyed by scientifically photographed plumb-lines.

There are three distinct optical illusions by which the observer is constantly deceived in cathedral interiors, and which can only be overcome by photography. There is the illusion, due to perspective convergence of the verticals, which causes them to appear perpendicular when they really diverge. This is the illusion which has deceived Mr. Bilson and M. Durand in the nave at Amiens. There is the illusion produced by the aisle vaulting ribs, which give an appearance of leaning into the nave to piers which are sighted on the side of the aisle. It is also true for standpoints in the nave that divergent verticals above the capitals of the piers are naturally assumed by the eye to be perpendicular. In that case the piers again seem to lean inward. Then there is the illusion of the vaulting ribs of the nave or choir which tend to exaggerate an actual divergence, or to create an effect of divergence and curvature where none really exist. These illusions operate variously, according to the position of the spectator and according to the special feature on which the eye is centred. They are also undoubtedly affected by the personal equation and by the personal disposition or prejudice in favour of a preconceived idea. The illusion due to perspective convergence explains the general modern oversight of a divergence in the naves of mediæval churches, which is undoubtedly much more general in its diffusion and much more pronounced in its amount than has hitherto been realised by mediæval archæology. The Brooklyn Museum photographs represent the first modern effort to illustrate these facts. An acquaintance with these photographs will dispense with a vast amount of discussion and debate. In the case of the majority of experts such acquaintance will produce a final and definite conviction. My experience with M. Favry offers a case in point, and has been cited for this reason.

Mr. Bilson will be much amused to learn that I still, occasionally, "sight on a plumb-line."

* Since the broad black tape used in 1903 had turned out to be invisible for photography in the cathedral nave, I began work this season with a cable of an inch and a half diameter which was suspended from six different apertures in the vaulting. The united efforts of three or four workmen were needed to hoist the enormous length of this heavy cable through the vaulting apertures. After two weeks' work in photographing the cable, I devised less cumbrous methods for obtaining photographic measurements.