these values being obviously independent of the choice of B.

Finally, I fail to see any advantage in the use of the so-called separated divisions of E. The results are exactly the same and a useless complication is avoided if E is only divided into parts exterior to each other.

Now divide a measurable set E into a countable sequence of measurable subsets δ_i exterior to each other and denote by $\overline{\int}_E f$ and $\underline{\int}_E f$ the lower and upper bounds of $\Sigma M_i \delta_i$ and $\Sigma m_i \delta_i$, where m_i, M_i are the lower and upper bounds of f on δ_i. By these definitions, the upper integral is never smaller than the lower integral. And if $f \equiv 1$, both integrals are equal to meas. E.

This new definition is very similar to that of Riemann. The real difference is *not* as Professor Pierpont asserts for his own that it makes use of an infinite instead of a finite number of parts of E. It lies essentially in the use of measurable parts of E instead of intervals. For instance when f is bounded over E, the definition is not altered if the parts δ_i of E are assumed to be in infinite (variable) number.

When $\underline{\int}_E f = \overline{\int}_E f$ the common value of both integrals is equal to the value of the corresponding Lebesgue integral.

UNIVERSITY OF POITIERS.

REPLY TO PROFESSOR FRÉCHET'S ARTICLE.

1. REPLYING to the foregoing criticism I begin by quoting. Professor Fréchet says: " But unfortunately the happiness of this idea is lessened in Pierpont's work by the choice of an inappropriate definition. Professor Pierpont intended to generalize the definition of Lebesgue integrals by defining upper and lower integrals of any function on any linear set E_μ. Such definitions should not of course be arbitrary ones, and there are some primary conditions to be fulfilled unless these definitions are to become quite artificial and uninteresting."

The implication that the reader will easily draw is that I have not fulfilled these primary conditions and that my theory is therefore quite artificial and uninteresting. Certainly flattering to the author.

To be historically accurate, I had no intention whatever of generalizing Lebesgue's integrals. When years ago I hit on my definition of integration, I did not know how it was related to Lebesgue's theory. I found out later that when the field of integration is measurable my integrals are identical with Lebesgue's and I have therefore called them Lebesgue integrals throughout my book. To prevent misunderstanding let me note that my definition is not restricted to a single variable as one may have gathered from the passage just quoted; this however is a minor matter. Professor Fréchet calls my definition inappropriate. Since my integral and Lebesgue's are the same when the field of integration is measurable, any defect in my integral is equally shared by Lebesgue's in this case. I infer therefore that his strictures apply only to the case where the field of integration is non-measurable.

I lay no great importance on this side of my definition. No non-measurable field has yet been studied as far as I know, and it may turn out that they have little value in the theory of point sets.

Theoretically they do present themselves in a rather awkward way in the theory of double integrals. Let $\mathfrak{A}$ be a measurable limited field whose projection is $\mathfrak{B}$ and whose cross sections are $\mathfrak{C}$. If $f(x, y)$ is limited and integrable in $\mathfrak{A}$, we would like to write down, as in the calculus,

$$\int_{\mathfrak{A}} f(x, y)dxdy = \int_{\mathfrak{B}} dx \int_{\mathfrak{C}} f(x, y)dy. \tag{1}$$

Now it turns out that although $\mathfrak{A}$ itself is measurable, an infinite number of the sections $\mathfrak{C}$ *may not be*. If now we do not define integrals over non-measurable fields, the symbol

$$\int_{\mathfrak{C}} f(x, y)dy$$

which enters (1) is not defined and the same is true of the right side of (1). This difficulty may be turned in a variety of ways; one way is to use a definition of integration which does not depend on the measurability of the field.

This Professor W. A. Wilson did in 1909. He replaced the intricate and highly artificial reasoning of Lebesgue* by

* Cf. Lebesgue, *Ann. di Mat.*, 1902. Reproduced by Hobson, Functions of a Real Variable (1907), p. 576.

simpler and more direct methods as given in Volume II of my Real Variables.*

2. Let us now consider some of Professor Fréchet's objections in detail, it being understood that the field of integration is *non*-measurable. He says: It is to be expected that for any $f(x)$ and any E

$$\overline{\int_E} f \geqq \underline{\int_E} f. \tag{2}$$

Now Professor Fréchet thinks he has constructed an example which contradicts the relation (2), i. e., he thinks he has shown that in a certain case

$$\overline{\int_E} f < \underline{\int_E} f. \tag{3}$$

If this were true, my theory of integration for non-measurable fields would be in a sorry plight.

To establish the *false* relation (3) for a special case, Professor Fréchet divides the unit interval $E = (0, 1)$ into two parts by taking a non-measurable component δ_1 and its complement δ_2 such that

$$\overline{\text{meas}}\, \delta_1 + \overline{\text{meas}}\, \delta_2 > \text{meas}\, E. \tag{4}$$

So far so good, but Professor Fréchet now states that $\mathfrak{A} = (\delta_1, \delta_2)$ is a *separated* division, for he says: "But of all separate divisions of E take those two, the first of which consists of E itself and the second of (δ_1, δ_2)."

Professor Fréchet has been misled at this point; there is no separated division of $\mathfrak{A}$ such that (4) holds, and his example establishes not an error on my part but a carelessness of reasoning on his.

3. Professor Fréchet now attacks the correctness of the relation

$$\overline{\text{meas}}\, A = \overline{\text{meas}}\, B + \overline{\text{meas}}\, C, \tag{5}$$

where B, C is a separated division of $\mathfrak{A}$. He says:

(1) "The assumption made in the second line of the proof of this theorem is not altogether obvious, so that the proof is not convincing. (2) Moreover, the theorem itself does not

* Wilson's results were given by me in a course of two lectures delivered at Clark University in September, 1909. Cf. also a paper by Hobson, *Proceedings Lond. Math. Soc.*, vol. 8, Part 1. Issued December 23, 1909.

hold in every case; for instance, it does not when A is an interval and B is a non-measurable part of A."

As far as the writer can see, (2) is a bald statement unaccompanied by a shred of proof. A charge as serious as this certainly deserves some support on the part of the person making it.

Let us look at (1). The assumption in question is that

$$\mathfrak{E}_n = A_n + B_n + C_n \tag{6}$$

is an ϵ_n-enclosure of $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$ simultaneously.

The author made this statement without proof because in his judgment an attentive reader who had used the machinery of superposition up to this point would admit its truth as obvious. However to make it clear even to him that runs, we add the following, using the notation of my book:

Let

$$A_n = \{a_{n\kappa}\}, \quad B_n = \{b_{n\lambda}\}, \quad C_n = \{c_{n\mu}\}.$$

Each cell $a_{n\kappa}$ is a measurable point set containing points of $\mathfrak{A}$, $b_{n\lambda}$ one containing points of $\mathfrak{B}$, etc. Now if e_{nj} denote a cell of $\mathfrak{E}_n$, e_{nj} is by definition $Dv\{a_{n\kappa}, b_{n\lambda}\}$ or $Dv\{a_{n\kappa}, c_{n\mu}\}$. But any point of $\mathfrak{B}$ (or of $\mathfrak{C}$) is in some $b_{n\lambda}$ (or $c_{n\mu}$) and since $\mathfrak{B}$ (or $\mathfrak{C}$) is a part of $\mathfrak{A}$, in some $a_{n\kappa}$. Therefore $\mathfrak{E}_n = \{e_{nj}\}$ contains all points of $\mathfrak{A}$. Now $\mathfrak{E}_n$ is a part of A_n and hence meas $\mathfrak{E}_n \leqq$ meas A_n; therefore $\mathfrak{E}_n$ is an ϵ_n enclosure of $\mathfrak{A}$. Let B_n' denote those parts of B_n contained in $\mathfrak{E}_n$. Then meas $B_n' \leqq$ meas B_n. Hence B_n' is an ϵ_n enclosure of $\mathfrak{B}$. Similarly C_n' is an ϵ_n enclosure of $\mathfrak{C}$. Thus $\mathfrak{E}_n$ is simultaneously an ϵ_n enclosure of $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$.

4. The next charge is that the relations

$$m\overline{\overline{\mathfrak{A}}} \leqq \underline{S}_D, \quad \overline{S}_D \leqq M\overline{\overline{\mathfrak{A}}} \tag{7}$$

are false. The proof that they are *correct* is given on page 372; as it requires but four lines, I reproduce it textually, viz.,

$$m \leqq m_i \leqq M_i \leqq M.$$

Hence

$$\Sigma m\overline{\overline{\delta}}_i \leqq \Sigma m_i\overline{\overline{\delta}}_i \leqq \Sigma M_i\overline{\overline{\delta}}_i \leqq \Sigma M\overline{\overline{\delta}}_i.$$

Thus

$$m\Sigma\overline{\overline{\delta}}_i \leqq \underline{S}_D \leqq \overline{S}_D \leqq M\Sigma\overline{\overline{\delta}}_i. \tag{8}$$

But by § 376, 2,

$$\Sigma\overline{\overline{\delta}}_i = \overline{\overline{\mathfrak{A}}}. \tag{9}$$

So far as in my book. To get (7) put (9) in (8). The only point in this simple demonstration which Professor Fréchet can attack is the relation (9). But this brings the question around again to the fundamental relation (5) which we have already discussed. Professor Fréchet is thus repeating himself.

In connection with the relation (7) I quote the following remark of Professor Fréchet. He says: " Curiously enough Professor Pierpont did not think it useful to mention that the inequality (2) should hold." I suppose I did not mention the relation (2) because I thought it too self-evident. The proof is immediate:

Let $D_1, D_2 \cdots$ be an extremal sequence as defined on page 374. Then replacing D by D_n in (8) above we have $S_{D_n} \leqq \overline{S}_{D_n}$. Let now $n \doteq \infty$, we get the relation (2) by § 383, 1.

5. Finally at the close of Professor Fréchet's article I am told by him that I do not know the real significance of my own work. " The real difference is not as Professor Pierpont asserts for his own (definition) that it makes use of an infinite instead of a finite number of parts of E (as in Riemann's definition). It lies essentially, etc. . . ."

Professor Fréchet will pardon me if I still hold to my original opinion in spite of his illuminating remarks. He has been so often wrong, as I hope I have made clear, that he may well be wrong here also.

The rest of Professor Fréchet's remarks relate to matters of taste and as de gustibus non est disputandum I refrain from entering the controversy. Professor Fréchet claims that I have erred on three counts, viz.: 1. The relation (2). 2. The relation (5). 3. The relation (7).

The only proof he has adduced is an example whose validity depends on establishing the *vital* fact that $\mathfrak{A} = (\delta_1, \delta_2)$ is a separated division of $\mathfrak{A}$. I expect he will hasten to remove this lacuna.

JAMES PIERPONT.

YALE UNIVERSITY,
January, 1916.