

THE HISTORICAL *vs.* THE DEDUCTIVE METHOD IN POLITICAL ECONOMY.

THE offering which the most prominent leader of the younger generation of the historical school has made to the founder and head of that school, Wilhelm Roscher, at the fiftieth anniversary of his doctorate, is a most fitting tribute.¹ It is as if Schmoller had presented a laurel-wreathed portrait of the veteran's intellectual self. A vigorous sketch, which forms the centre of the book (pp. 147-171), shows Roscher's place and significance in political economy, and around this Schmoller has set a frame of older sketches, consisting chiefly of the literary portraits which he has made of other economists, as occasion served, during the twenty-five years between 1863 and 1888, and made, too, in the light of the historical school. It is this latter element which gives unity to the book. Embodied in these portraits a whole literary epoch stands before us—an epoch which includes the beginning and growth of the political economy founded by Roscher upon historical method, its battles and victories and renewed battles. The life-stage upon which Roscher's scientific mission was fulfilled is thus faithfully exhibited to us.

It is but suitable to the occasion that the reviewer, too, should devote his first words to that portion of the work which immediately concerns the person of the celebrated master. As Schmoller expressly says in a preface addressed to Roscher, he has no wish to honor the *jubiläum* "with a panegyric such as is customary at celebrations of

¹ This article is a translation of an article in Conrad's *Jahrbücher für National-ökonomie* (Vol. LIV, p. 75) by Professor Dr. v. Böhm-Bawerk, in which he reviews a late work of Professor Schmoller's on the Literature of the Political and Social Sciences ("Zur Litteraturgeschichte der Staats- und Sozialwissenschaften"). The book is dedicated to Professor Roscher, in honor of the fiftieth anniversary of his doctorate.

this sort," and he has not attempted it. On the contrary, he has sought to analyze psychologically, and with the strictest objectivity, the character and the work of his master. He has therefore, as is reasonable, placed duly in the light all the merits and superiorities of the man, but he has also not hesitated at least to indicate his weaker points. We have thus a character sketch which is, above all else, true, and which, in spite of a few wrinkles that the brush of the painter has not softened, is extraordinarily honorable to its subject and creditable to its author.

As is natural, Schmoller honors Roscher above all, as the road breaker of historical investigation in political economy. He shows how Roscher began as a philologist and historian, later to find his life-work in transplanting abstract political economy to historical ground. He rightly values Roscher's rare historical gifts, his happy tact in interpreting and estimating an often incomplete set of facts, and his enormous, many-sided learning. He does not fail to point out, in this connection, that certain peculiarities "for which historical minds are not usually distinguished" are also lacking in Roscher. He reviews the master's most important works—his researches in historico-political science, his history of the literature of economics, and his system of political economy, which last is his most widely-known, but not his most important, work. He shows that important inequalities exist between the different volumes of the "System;" that in the two special volumes which treat of the economics of agriculture, of trade and manufacture, the peculiarities of Roscher's historical tendencies are seen at their best, while the first volume on general theoretical economy and the volume on finance are of less value. He enlivens the description by means of comparisons, in which he contrasts Roscher as a systematic writer with Rau, and as a literary historian with Dühring, and finally he sums up his study with the following concise and significant words: "Roscher has the poly-historical feature in common with the older Göttingen

writers of the history of civilization ; he has caught from Rau and the whole elder generation the deepest respect for Adam Smith, Ricardo and Malthus. His temper is that of a sensitive, markedly conservative, scholar, who strives to destroy nothing suddenly, but slowly to rebuild. He wishes quite as much to remain a dogmatic economist as to enlarge the tenets of the old school by historical work. He stands between two scientific epochs, closing the old and opening the new."

It is not my task to review separately each of the essays collected into the present book. They are all more or less familiar to the public. I will merely indicate which of them are best known. They are the essays on Schiller (1863), on Fichte (1864-65), List (1884), Carey (1886), Lorenz von Stein (1866), Knies (1883), Schäffle (1879-88), Funck-Brentano (1876), Henry George (1882), Hertzka (1886), besides an essay on the more recent views of statistics of population and of morals (1869), and a critical review of the writings of K. Menger and W. Dilthey on the methodology of political and social science (1883).

I am led, however, to notice one theme which forms the essential centre, a theme which we meet in each of the essays, but which is treated most clearly and decisively in the last—perhaps not without the intention of giving it the impressive position of a "last word." This theme is the method of investigation in political science.

I am encouraged to do this by the following considerations: In the first place, the question has lost nothing of urgency since the first appearance of the essay referred to; the question how political science should be pursued, whether by "exact" or historical methods, is just as pressing to-day, just as important and as far from being settled, as it was years ago. It seems to me that the occasion of the reprinting of the essay is for more than one reason more favorable to a fruitful discussion of its contents than was that of its appearance. It is well known that the year 1883 was a hot one for the discussion of methods, and passion is seldom a good counsellor in scientific debate. The excitement has

long since cooled, and I, for one, will make an honest endeavor to treat the subject *sine ira et studio*, and with the most unimpassioned objectivity. Moreover, Schmoller himself has set me a noteworthy example, in that, although he still upholds his fundamental thesis, he has yet by a trifling change of style removed the personal bitterness against his former opponent. And finally, during the last six years much has come to light which essentially facilitates a fruitful discussion of the question. There is no task more thankless than an abstract discussion of methods. It is extremely difficult to form a true conception of a method and still more difficult to pass just judgment upon it when it is presented only *in abstracto*, without an exhibition of its operations. From this fact the so-called empirical method suffered greatly six years ago. None, or at least few, had seen its workings, and we were at liberty, therefore, to form of it any conception which might please our fancy. To-day the case is different. To-day we have before us a whole series of works based on that method. He who will, can by concrete examples see how the empirical method works, to what it is applicable, and to what it is not. In short, if it be true that experiment outweighs theory—and in questions of method this precept seems to me indubitable—we are to-day far better equipped than we were six years ago to make an impartial estimate of the rival methods of investigation.

As a believer in frank statement of opinion, I am willing straightway to admit that I am a defender of the method called by Menger "exact,"¹ and attacked by Schmoller;

¹ While there is still so much to be cleared up in the subject itself, it is scarcely worth while to quarrel over the name. I will, therefore, briefly remark that the method in question is spoken of by many, especially by its opponents, as the "*a priori*" method, by others as the "abstract-deductive" method, neither of which is quite correct, as we shall see. Menger, not without calling forth sharp opposition, calls it the "exact" method. I would rather call it the "isolating" method; for, as I not long since pointed out (*Göttingische gel. Anzeigen*, 1 June, 1889, review of Brentano's "*Klassische Nationalökonomie*"), its essence consists in, first of all, observing the different aspects of complex phenomena separately, but let it be understood, what is often forgotten, that it should not content itself with a partial view, or mistake the part isolated in thought for the whole, but, as far as possible, should construct out of the clearly grasped parts a complete whole.

and in order that there may be no misunderstanding as to the extent of this admission—a misunderstanding which after certain singular events of recent date I have cause enough to fear—I wish by express explanation to place beyond the possibility of a doubt, what is in question and what I advocate. I expressly affirm, therefore, that so far as I am concerned, I have absolutely no thought of casting doubt upon the complete right of the historico-statistical method to a wide and important province in economic inquiry ; I have still less intention of denying and belittling the enormous services that historical and statistical work has rendered and still renders to economic science ; and least of all is it my intention—an intention which, through a singular misunderstanding, has often, and again quite lately, been imputed to the advocates of the abstract method—to assume an attitude either hostile or even indifferent to social and political reforms. Quite recently we have heard Brentano accuse the “abstract” school of thinking that with the refutation of certain socialistic errors, “the perfection of the present economic organization is demonstrated ;” the conduct of the school gives rise, he says, to the idea that “all is in order in the economic world, and there is no misery except what people bring upon themselves ;” “incomprehensible” to him are the followers of that school “which thinks that by newly formulating old truths, by a new definition of interest, or by the argument that differences in productiveness of land, and hence rents will be found even in a socialistic state, it can get rid of these difficulties.”¹ There is, indeed, something incomprehensible in the matter, but it is the fact that Brentano could impute to me and my scientific associates views which we not only have never anywhere expressed, but which are diametrically opposed to our scientific convictions. It should be entirely superfluous to stop to defend one’s self against the imputation of such antediluvian ideas ; but, when an economist of Brentano’s standing has not disdained to speak such slan-

¹“Ueber die Ursachen der heutigen sozialen Not.,” Leipzig, 1889, pp. 28, 4 and 5.

ders, I, too, may not disdain once emphatically to contradict them. I state here then, once and for all, that so far as I am concerned, and I am confident that in this instance my colleagues of the abstract school will not gainsay me, such opinions are utterly foreign and repulsive to us. It really does not seem to me possible to overlook the existence of much that is lamentable and in need of reform in the present condition of society. I consider an indifferent, *laissez-faire*, *laissez-passer* attitude toward them wholly wrong; I sympathize most warmly with active efforts at reform in favor of the economically weak and oppressed classes, and endeavor as far as is in my power to further them. To be sure, I have as yet written no book on the subject; but can that fairly be made the ground for charging me with the very opposite of what I believe, and as a teacher always profess?

If now, we put all misunderstanding aside, the *status controversiæ* may be reduced to the following simple terms: The question is not whether the historical method or the "exact" method is the correct one, but solely whether, alongside of the unquestionably warranted historical method of economics, we shall not recognize the "isolating" method. Many, I among them, maintain that it should be so recognized. Many others, most of them adherents of the historical school, reply in the negative. It is a practical negative when Schmoller, for example, relegates to a despicable "little corner of the great structure of our science" all that can be accomplished by that method (p. 293), or when he emphatically declares that a new era of political science is opening, "but only through the conversion of the whole historical and descriptive material now in our possession, *not* through the further distillation of the abstract theses of the old dogmatism, already a hundred times distilled," (p. 279.)¹

In favor of the equality of the two methods, which is

¹ Cf. also Brentano's "Die Klassische Nationalökonomie," *passim*, especially pp. 4 *et seq.*, 28 *et seq.*

denied by the historical school, I will now attempt to set forth certain experiences and impressions which have, in the course of years, been forced upon me, and which do not seem to have been sufficiently emphasized thus far in the progress of the strife. If my plain speaking shall seem to take a polemic character, it will be entirely against my will. My desire is rather to heal the strife than to embitter it. Certain truths must be spoken unless all hope of coming to an understanding be explicitly renounced. In any case, I beg those who differ with me to believe that I am able to separate the personal side of the discussion from the material, and that, in spite of the unavoidable opposition which I am forced to offer to certain statements of the leaders of the historical school, I continue most gladly to pay tribute to their real and important services to our science.

It is admitted on all hands that the deductive method which, generally speaking, is one of the two processes upon which the mind is dependent for the extension of its knowledge, is applied with rich results in almost all sciences, even in those which are pronouncedly empirical, like physics and astronomy. If this mode of reasoning were to be entirely, or almost entirely, banished from political economy, it would be necessary to point to very positive reasons for distrusting so well tried a method.

If now we review the grounds upon which stand the objections of the historical school to the "abstract-deductive" method—and I use this name because it is convenient as a name, not because I am entirely satisfied with it—we meet two principal arguments. On the one hand it is sought from the peculiarly complex nature of social phenomena to *deduce* that the method of investigation in question is not adequate; into this territory I shall not to-day venture, since far too much has already been deduced both for and against the opinion. Only in passing, I would point out the curious fact that though the historical school looks so contemptuously on deduction, yet to establish the

proposition that abstract-deductive reasoning is not applicable to political economy, they make a brave use of abstract deductions. On the other hand, and chiefly, they combat the method with an argument drawn from experience: the classical political economy worked by this method; it erred; therefore, they conclude, the method is proved to be unfit for the inquiries of political economy.

In this favorite argument of to-day the unprejudiced thinker finds something quite remarkable, and that is the extraordinary assurance with which claim is here laid to strictness of experimental proof. In most cases one scarcely ventures to regard a general statement as proved by experience until its correctness has been established by a whole series of separate concurrent instances of as many various sorts as possible, while here the pretended experimental proof that abstract deduction accomplishes nothing in political economy rests upon one single instance—that of the classical school! And what sort of an instance is that? The case of a young science in its beginnings! When did ever a science win the whole truth at the first throw? Has not every one, even the most rigorously empirical—have not astronomy, physics, history, floundered for centuries from one error to another, fished up out of each phase of error scattered grains of truth, one here, another there, and slowly, very slowly, gathered them into the upward tending structure? And what no other method has been able to do the abstract-deductive method is expected to accomplish for political economy. Rich treasures it has, indeed, brought to light, as its opponents well know, and of these, too, not a few. But that at the first onset it should bring forth the whole truth, and nothing but the truth, that is too great a miracle to ask. Because it has not wrought the miracle shall it be now and always denied all confidence?

The rashness of this judgment is not the only peculiar feature which the historical economy's conception of the classical exhibits; therewith is associated a singular con-

tradition in its estimate of the classical economy, a strangely inconsistent coupling of depreciation with exaggeration of the ability of the latter. That is to say, on the one hand the body of classical doctrine is somewhat shortly and dryly designated a mass of fatal error, and therefore repudiated; on the other hand, a superhuman ability is imputed to the same men whose achievements are so bitterly censured. It is implied that all knowledge which could be derived by their methods they had derived to perfection from the material within their reach, so that a "further distillation of the abstract theses of the old dogmatism already a hundred times distilled" offers absolutely no prospect of further gain. The men are thus in the same breath represented as both fallible and infallible—fallible in the choice of methods, infallible in the use of them.

It seems to me that under such conceptions the scientific situation cannot fail to be misunderstood. The classical economists have probably accomplished more in some directions than the historical economists are willing to allow. With that point I am not now concerned. It is quite certain that in other directions they have accomplished very much less. They are far from having distilled from familiar facts and hypotheses all that could have been derived by their method—quite the contrary. They have but very imperfectly distilled, and in countless cases they have been unable to derive the best conclusion from their premises. Their theory swarms with vital questions, not of method alone, which they have wrongly handled. Instead of many examples take one, the very popular and important doctrine of value. The experience from which the general theory of value is to be derived was about as complete for the classical economists as it is for us. What did they make of it? Some declared that the creative principle and measure of value was the amount of human labor involved; others, more numerous, that it was the cost of production. Both definitions are false, as every one to-day knows. But

—and this is the point to be emphasized—they were branded false, not by the discovery of some new and startling fact which advanced empirical investigation has brought to light, but by the everyday experiences of the world, experiences which were necessarily as familiar to the classical economists as they are to us. It was not that in this case their empirical knowledge was insufficient, but that they here, as in countless other cases, “distilled” badly.

If the question under consideration were only to correct a somewhat platonic, adscititious judgment as to what rank in personal esteem is to be accorded to the heroes of the classical economy, it would be scarcely worth while to bring it up here. I think, however, its significance goes much deeper. I think, in fact, that herein lies the key not only to an understanding of the position of the historical economists in regard to methods, but also to an understanding of their great mistake. Had the classical school indeed fully accomplished all that could be accomplished by their methods, the historical school would then be quite right in maintaining that further progress could be made only by a change of method. They conceive the present condition of our science to be that of a mine out of which each man in his turn has dug a certain amount of ore, that ore being now completely exhausted. If one hopes to find more metal here, he will not be likely to smelt again the barren slag, but rather dig for new ore; or, as Schmoller would have it, he will not be likely for the one-hundred-and-first time to distill what has been distilled one hundred times already, but would collect new material out of the store of historical and statistical experience.

The case of our science is, however, not this. It is more correctly conceived as follows: From the universal experience of mankind, from history and statistics, an enormous treasure of empirical raw material was even before our day brought to the furnace. The classical economy, which, in spite of its prematurely given name was only an incipient, embryonic science, with its well-meaning but primitive and

untaught art extracted much gold, but that which required a finer process it could not reach. What then shall we do who come after? The answer, both metaphorical and literal, is a simple one. We have evidently a double task: we must by means of an improved, more careful, process, a stricter "distillation," extract the countless grains of truth still in the old one, and we must at the same time mine new empirical material for future refining, the richer the better. But what would the historical school have instead? They would first of all open an era devoted to the collection of materials. They would have, first of all, in addition to the enormous mountain of raw material which lies still unworked, a second mass, if possible still larger than the first, before we begin again to "distill."¹

Under some circumstances this might be all very well, but under the given conditions it seems to me a highly unsuitable plan, and for more than one reason. In the first place we should not in science, any more than in everyday life, put off till to-morrow what can be done to-day. In the second place each ray of light, even the dimmest which theory has yet drawn from the old facts, helps to lighten the search for the new. Empirical investigation, even in its lowest form—the collection and tabulation of facts—is far from being merely mechanical work which one could do just as well with his eyes shut. For he who would merely gather facts with success must be a seer who knows what he wishes to find, and when and how he can find it. Consciously to give up the present preparation for a "distilling theory" is consciously to prefer carrying on the work of collection in the dark rather than in the twilight, or in the twilight rather than in full day. And finally, I cannot repress a grave doubt whether the historical school will be either able or willing, when the future to which they point us shall have become the present, to redeem the promises with which they put us off. They promise to

¹ Cf. Schmoller "Zur Litteratur-Geschichte, etc.," p. 278 *et seq.* Brentano's *Die Klassische Nationalökonomie*, p. 28 *et seq.*

build us a theoretical structure sometime in the future if only they first be allowed to provide for it the broader empirical foundation which they deem necessary. That would be all very fine if we could be sure that the historical school is to-day hindered in construction by nothing else than a lack of material. There are those who suspect that the present theoretical sterility of the historical economy is due to quite another trouble—not to the lack of material from which to construct, but to their manner of treating it. To speak plainly, there are those who suspect that in order to extract an adequate general theory from the given empirical material, just that due measure of abstraction and use of deduction is necessary which the historical school definitely reject. In favor of this view there is much that could be said, upon which, for fear of becoming prolix, I will not enter; I shall later on have occasion in another connection to make some remarks that will serve to support it. But assumed that this view is the correct one—a supposition which is not at all precluded by previous experience—what of it? It follows that the historical school, if they continue in their present course, will after twenty or fifty years of collecting material be as far as ever from a constructive theory. They will have to put off the fulfillment of their promise from decade to decade, until, tired of fruitless expectation, men seek its fulfillment elsewhere; and hereby precious years and scores of years will have been irrevocably lost to German political economy.

It is far from my intention by these remarks to depreciate the value of empirical investigation. To prevent the possibility of any misunderstanding, I will be still more explicit. In my opinion there is not the slightest doubt that the extension of the empirical material of the science is advantageous to its development. A difference of opinion can exist only in regard to the respective degrees in which, in the present actual circumstances, the purveyance of new empirical material on the one hand, and on the

other hand the "abstract-deductive" consumption of the old material will further that development. In regard to this question, which is less a question of principle than one of fact, it seems to me that they of the historical persuasion err, misled by their previously mentioned misconception of the actual condition of the science. They too highly magnify the office of empiricism. Starting with the optimistic assumption that the former "distillation" accomplished all that was then possible, and that the advance of political economy is hindered only by the lack of "descriptive empirical material," they can naturally expect nothing from repeated distillation of the old material, and are forced to stake everything upon the enrichment of it; this enrichment from empiricism they regard not only as advantageous, but as the only means in the present condition of affairs to the accomplishment of our end.

This conception seems to me not to correspond with the facts. The case appears to me to be rather as follows: The problems of political economy, from a technical point of view, fall into two classes. Upon those of one class the mark of the historical school is indelibly stamped. Nor would I proceed otherwise than by the historical-statistical method if I had to study, for example, the influence of retail trade upon price or the economic influence of the division of property in land. Even here we cannot do wholly without deductive reasoning, as is shown by the masterly combination of sharp-sighted deduction with solid empiricism in the works of many of the strongest advocates of the historical method—Nasse, Conrad, Miaskowski, Schanz, Schönberg, and others. But at any rate, for this class of problems the rule holds good that if one wishes to further the solution of them he must begin by enlarging his knowledge of facts. Every addition to our experience is in such a case valuable and fruitful. Without claiming to define with precision the limits of this class, it may be said to include most practical or social-political problems

as well as questions of detail attaching to theoretical problems.

With the second class of problems, however, the case is different. For them experience has already done all, or nearly all, that it can do. It has thousands and millions of times over brought to our notice certain obvious outward relations, and a further pursuit of empirical investigation would have no other result than to bring out, perhaps ten thousand or ten million times, the same familiar outward relations. The difficulty, and at the same time the scientific problem, is not in acquiring facts, but in mastering and correctly interpreting the facts long since acquired. A few examples will make this clear more quickly than will any abstract analysis. Man's estimate of the value of property has been millions of times observed. Science has to answer the question, What is the final principle and measure of our estimate of value—the labor involved, or the cost of production, or the degree of service rendered, or the estimated final utility, etc.? I do not suppose anyone thinks we shall be nearer the answer to the question when we shall have collected a second or third or tenth million of estimates of values, when we shall have emptied all the arsenals of history and statistics in the determination to find out what meat and grain were worth to the citizens of Elberfeld in the Fifteenth Century, or what cotton is worth to-day in England, Sweden, Turkey and India, although I would not deny that for certain details of the theory of value this information will always be useful. All the elements for the solution of the problem are contained in the first million of instances—perhaps in the first thousand—as surely as in the last. The art is merely to set them free, and to that end it is not greater extension, but greater profundity, that is needed. Again, the existence of a certain empirical relation between cost and market price has long been beyond a doubt. Science has to answer the question on which side is the cause and on which the effect—whether the high cost of production is the cause

or the effect of a high price of the product. Here, again, the increase of empirical material is of no benefit. Numerous observed instances tend to support the former notion, as when, after rise in the price of hops, a rise in the price of beer follows. Other instances support the latter view; for example, the rise in the price of brick-kilns after a rise in the price of bricks. And in every thousand and every million of instances cases of both sorts will be found side by side in almost equal proportion, so that after the tenth thousand you are no wiser than you might have been after the first. Here, then, it is not a greater accumulation of observed cases that will help, but a deeper insight into them. And thus it is with the whole category of theoretical problems—as the question of the true character of the influence of supply and demand upon price, of the true function of capital in production, of the origin of interest, of the relation between saving and the accumulation of capital, etc. The most general and fundamental questions of theory belong, as it seems to me, to this class of problems.

Hence springs a double consequence: first, it is not for all questions of political economy equally that we are to look for advantage from the further accumulation of empirical material, for in important spheres of investigation such accumulation is useless and purposeless, and advance is to be expected only through more effectual “distillation” of facts already known; secondly, in the same spheres mere quiescence and hope from the future are out of place. What can be done at all can be done as well, and better, to-day than after twenty years more of industrious observation. These years would bring us nothing but disillusionment and a mass of superfluous material, and we must even then begin what I and my colleagues are doing to-day, and what the historical economists would prevent us from doing—the intellectual exploitation of the treasures of experience by all the methods which our mental constitution allows, and among them by the method of abstract-deductive “distillation!”

If it is instructive to hear what a man says in regard to a question of practical conduct, it is still more instructive to observe how, under the given conditions, he himself acts. For such observation the conduct of the adherents of the historical school in this connection furnishes an excellent opportunity. It is noticeable that in questions which, like those above mentioned, from their nature require abstract-deductive treatment, the historical economists invariably have used, and still use, the very same abstract deduction which they, in words, repudiate. In the elder generation this is extremely easy to prove—is even notorious. They still treat chiefly theoretical problems, or, at least, discuss them, and that being the case, they felt themselves forced into the method which the nature of the problems demanded. Everybody knows that Roscher filled the greater part of his "*Grundlagen der Nationalökonomie*" with the scouted doctrines of the classical economists. In Knies' highly-esteemed works on "*Wert*" and "*Geld und Kredit*" abstract-deductive reasoning predominates,¹ and Hildebrand takes the same road when he is led to an opportune digression upon some similar subject—value, for instance, and the explanation of the phenomenon that useful things often possess little value, while comparatively useless things possess great value.

The same trait in the younger generation of the historical school is, at first sight, less striking. Theoretical problems of a general nature are to-day almost entirely avoided, and—I say it with no depreciatory double meaning—if the history of the development of this or that municipal guild or the price of grain in a certain place, or the population of some mediæval town, or any similar subject, is to be investigated, little occasion is offered for delving into theoretical problems. But yet it is not possi-

¹ Schmoller himself says of Knies, that "in one of his youthful works he had, indeed, demanded the historical method, but that in his riper works on '*Money and Credit*,' which will always belong to the best that German theoretical political economy has produced, he has, in all essentials, abandoned this method for the more or less abstract method by which Menger himself works." *Litt. Gesch.*, p. 293.

ble entirely to avoid contact with the latter. It is absolutely impossible to make a report upon a question of economics, much less to discuss it, without touching upon general theoretical conceptions and propositions. For example, whoever has occasion to explain a rise in the price of meat by a rise in the cost of raising cattle indirectly recognizes the law, although it may be with many limiting clauses, that between the price of goods and the cost of production a causal relation exists; or whoever, in speaking of a fall in rates of interest, says that the "price of capital" is lower, unconsciously acknowledges himself an adherent of the view that there is such a thing as a real, independent "use" of perishable and representative commodities, and that the existence of interest on capital must be explained by the real existence of that "use" as an independent element. In this wise the younger generation let slip, unconscious, veiled acknowledgments, perhaps the more significant because unintentional.

The instructive point is that the substance of these acknowledgments is almost always borrowed from the older abstract-deductive teaching. "*Naturam expellas furca, tamen usque redibit.*" The historical school have discarded abstract deduction, but they cannot write three pages on economic questions, even in the historical style, without general theory, and one can construct no general theory without the use of abstract deduction. For facts are not so accommodating as to present themselves to the eye of the investigator ranked in a scale ascending from the most special facts to ultimate, general facts. Even if such an orderly structure really exists, it is seldom or never immediately visible in its completeness; single links, single terms of the scale, are almost always hidden at first, and their existence must be assumed by a deductive operation, the results of which are afterward to be verified by experimental test. To take again one example instead of many: If we let only the facts, which are obviously perceptible, speak for themselves, we shall never discover whether the value of

materials is the cause or the effect of the value of the product. It is easy to see that there is a relation between the two, but the whole structure is to our eyes so distorted by complications that by simple observation we cannot tell which of the two facts occupies the lower and which the higher place in the scale of causation. What the outward eye cannot here unaided see, the mind must construct by means of a series of abstract speculations. The theory of final utility, as is well known, was thus worked out, and in such a manner that the internal series, as well as the experimental proof, was actually complete.

What position, now, does the historical school¹ take upon this and similar questions? As already shown, they must have one view or another, and it is impossible to arrive at one without an abstract-deductive operation; the historical school on principle deny even the intention of performing such an operation; hence there is no other course left for them than either consciously or unconsciously to appropriate what others before them have deduced—in other words, to appropriate the doctrines of the old classical economists. The historical economists have thrown them out at the front door of science only to have them slip in again through a back door into their own works. Anyone who will take the trouble to observe may gather an imposing array of utterances which can be construed in no other way than as acceptance, probably involuntary, of the officially repudiated classical doctrines.

This state of affairs naturally has dubious consequences. The least of them, since it is only personal, is that the historical school lay themselves open to the charge of inconsequence. Far more serious is the objective injury done by the historical school, in that, while scouting for all present and future time the use of abstract deduction as a “further distillation of the old dogmas already a hundred

¹ Under this head I include, as the connection shows, only the historians of the strictest orthodoxy, the methodically *exclusive* writers; not those numerous economists who, although they have been active chiefly in historical or statistical investigation, have yet kept in sympathy with the “dogmatic” economists..

times distilled," they yet remain fast in the same sorry slough of dogma in which the first imperfect attempts of the old economists involved them. To deny a fault is to hinder its correction. If they decline the more recent advances in the theory of value or of capital because they are acquired by abstract-deductive methods, but yet cannot themselves avoid referring to value, the influence of cost upon price, the productiveness of capital and the like, they remain grounded on the same contradictory, half-apprehended, or misapprehended conceptions which the classical economists brought to light. That adherents of the historical school themselves occasionally find fault with the doctrines does not help the matter at all. A bad doctrine is never made better by fault-finding alone, and whoever depends upon a creed which he himself finds worthless has a worthless creed indeed upon which to depend.

It seems to me that from one point of view the attitude of the historical school toward abstract-deductive theory is the same as that of loose, theory-disdaining routine to all theory. The man of routine who arrogantly rejects theory usually rejects only the new and advanced theory, while he remains unconsciously fettered by the superannuated notions which have filtered into practice, into the judgments and prejudices of "common sense." And the historian who purposes to reject abstract deduction in general, in reality rejects only its newer, improved results, and thereby remains, the more unsuspectingly, the more surely, under the dominion of the old false deductions.¹

If a tendency of method is more faithfully mirrored in the works than in the words of its followers, we can in no

¹ To what a compromising extent this contentment with the old incorrect platitudes and this unfamiliarity with their statements of theory—boldly criticised, however—and their substance can, in certain cases, be carried, out of sheer rabidity in method, is well shown in W. Sombart's latest criticisms of Wieser's "Natürlichen Wert" (*Schmoller's Jahrbuch*, Vol. XIII, 1889, Part 4, p. 238). I wish this critique the greatest publicity, for the critical reading of it will make the spirit in which it was written more repulsive to the unprejudiced mind than I could make it by the most cogent arguments. Moreover, I am far from wishing to measure the whole school by such extravagance.

way better obtain a fair presentation of the attitude of the party of the defendant than by holding up the glass to them. How do those economists really proceed whom it is the fashion to name the "abstract-deductive school?"

Supported by certain ambiguous words of Menger,¹ which were assuredly not intended by him to convey the meaning which our opponents have found it easy to foist upon them, the opinion has become widespread that the so-called abstract-deductive method is unempirical, starting with hypotheses which have no correspondence with the fact, and therefore spinning abstract conclusions without concern as to their empirical reality.² No representation could be further from the truth than this. The abstract-deductive method as presented in the German literature by C. Menger, Wieser, Sax, myself and others, is in its very essence a genuinely empirical method. It is by no means exclusively and finally abstract-deductive, for which reason I can accept the name only with qualifications; it has no fancy for *à priori* axioms as a basis for its inferences, nor does it confine itself to inferences and deductions. On the contrary, it starts exactly as the historical school would have it start, with observation of actual conditions and endeavors from this empirical material to derive general laws. But it recognizes the utility—for technical reasons above referred to—of tracing causal connections, not only from special to general, but also, for the sake of experiment, from general to special. It thereby often discovers links in the chain of causes which were, of course, present in the complex, empirical facts, but which were there so deeply inwrought that they would hardly, if ever, have been discovered by a purely inductive method. To

¹ "Nothing is so sure as that the results of the 'exact' method of theoretical investigation, measured by the standard of reality . . . appear inadequate and *unempirical*." *Methode der Sozialwissenschaften*, pp. 54 and 59.

² "Menger . . . supposes that political economy is an exact science, *i. e.*, that it desires, in a one-sided way, to derive from what it considers first elements a quantity of safe, deductive conclusions, *in opposition to which empirical realities* may be contemptuously put to one side as something immaterial." Schmoller, *Litt. und Gesch.*, p. 283.

the eye taught by preliminary deduction where and how to look, the same facts are easier to find and to verify, as the astronomer Leverrier, in an often-quoted but none the less forcible example, from given empirical premises, the disturbances of the orbit of Uranus, deduced the existence, the motion and orbit of the planet Neptune, and then actually found the planet in the orbit he had calculated.

However, we will not make words, but proceed to look at facts. Take a practical example. The best known among the doctrines of the abstract-deductive school is the theory of value based on final utility. How did they arrive at it? By some soaring *a priori* speculation? Not at all. In the first place they simply observed how men practically regard property, and they found that men value it not, as the theory of abstract value teaches, according to the utility of the whole species; neither, as the labor theory teaches, according to the amount of labor embodied in it, nor according to the cost of production, but they value it simply according to the effective increase of well-being which they expect to secure by the possession of it. A further empirical investigation, rightly conducted, and supported by deduction, gave this result; that invariably we must regard as the dependent effective increment of utility that one which, in a given case, one can most easily determine, if necessary, to do without, *i.e.*, that consumption of utility which, among all the consumptions made possible by the possessions of the given individual, is of least importance, or the so-called "final utility." With such a key in hand one can tirelessly investigate all the changes and complications of practical life, the commonest as well as the most fantastic, and need not rest until the test has been everywhere successful. Not long since one of the leaders of the historical method, by no means friendly heretofore to the abstract-deductive method, was impelled to recognize that the theory of the measure of value was derived from "an immediate observation of life!" I can claim a similar genuinely empirical basis for my re-

searches in the theory of capital ; for example, my inquiries into the productive function of capital, into the relation of saving to the accumulation of capital, into the existence and the rationale of the lower estimate placed upon future than upon present possessions of property under certain conditions, into the exceptions to this rule, into the origin of interest and the like. And if Schmoller rightly demands that political economy should provide itself with a sound psychological foundation (p. 282), he will find his demand nowhere so nearly met as in the works of the so-called abstract-deductive school, in the works of a Jevons or Sidgewick, a Menger or Wieser ; indeed, it would not at all surprise me if in the future this school should come to be called the "psychological school of political economy."

Again, it is a mistake to say, as has been said, that we would deduce everything from a single premise, the postulate of an enlightened self-interest, without troubling ourselves about other highly important factors of social economy—habit, custom, humanity, nationality and the like. Whoever will fairly review our works will find, indeed, that we now and then expressly concern ourselves with the working of an enlightened self-interest only, and analyze it in detail ; but he will also find that we never forget the influence of the other factors, and still less deny it. At times we also devote care and attention to this influence ; but life is short and art is long, and since we are not able to exhaust all things, we must often content ourselves with merely noticing the existence of such incidental influences, the necessity of taking them into account and filling in the gaps which for the present have been left open. For a typical example I venture to refer to my analysis of the theory of price already familiar to the reader, the first, general part of which I worked out in detail and then added the second part as a necessary complement.¹

A careful inquiry would probably lead to the conclusion

¹ *Jahrbuch für Nationalökonomie und Statistik*, N. F. Bd. XIII, p. 477, *et seq.*, especially 486 to 488.

that the doctrines of the classical school, decried as highly abstract-deductive, were by no means abstract-deductive in their origin, but were, in the last analysis, empirical. It seems to me highly probable that the doctrines of demand and supply, the theories of value based upon labor, cost of production, etc., were not originally the results of speculation, but were the products of crude empirical observation. However that may be, we who are living have a right to be judged, not by what an old prejudice fables about us, but rather by what we actually do, and in view of our actual attitude the charge of denying or ignoring living realities must in the end be withdrawn from our account.

"By their fruits ye shall know them" are the words Schmoller has put at the end of his reprinted essay on Menger. I accept the principle gladly. As to the adequacy of a method there is no juster index, and for us none more expedient than the sum of what the method has accomplished. But as a matter of course we demand that the principle, as a test of our method, should be applied to what we ourselves have done. This protest is directed against a treatment as remarkable as it is common, which Brentano not long since brought heavily to bear upon us in a dissertation upon the classical economists. It is a favorite practice to criticise the abstract-deductive economists of the past, and there nothing is easier than to point out errors and misconceptions under which our grandfathers labored 80 or 100 years ago, in the childhood of our science, and then to extend the same judgment, without qualification, to the abstract-deductive economists of the present day. That is unjust. If we are to be judged, make out the case against us and not against our grandfathers. For our own faults we will willingly be answerable, but to be made answerable for faults committed by others which we have ourselves no less strongly reprehended than have the historical school, we must decline.

If we strike a balance between what the historical school have done in the province of theory and what we

have done, it seems to me we have no cause to blush at the result. I say advisedly in the province of *theory*. For it is only here that the question of method is in dispute. In the province of practical social-politics, for technical reasons, the historical-statistical method is so unquestionably superior that I do not hesitate to declare that a purely abstract-deductive legislative policy in economic and social matters would be as much of an abomination to me as to others, but when Schmoller lays claim to the same superiority of the historical method in the province of theory proper¹ he seems to me to be out of touch with the facts. A glance at what has been done in Germany in the last thirty or forty years in the department of theory seems to me, rather, to show the following results: The historical school has unquestionably merited much in the province of criticism of theory, although the lion's part in the critical revolution which shattered the sovereignty of the classical economy was taken not so much by the historians as by the Socialists and the Socialists of the chair—by Rodbertus, Lassalle, Wagner and Schäffle. In the province of positive theory, however, the efforts of the historical school have been confined almost exclusively to questions of detail. They have made, for instance, quite remarkable investigations into the influence of retail trade upon price; into the fluctuations between the prices of different classes of goods in long periods of time, as well as into countless details of the subject of money, exchange, banks, etc. On the other hand, the great questions of general theory have either not been touched at all by the historical school, or else, as in the case of Knies, have been treated by another than the historical method. All the positive advances which general theory has made in recent times are notoriously due to other methods. In proof of this I would call to mind the investigations of Knies and Wagner in money

¹ Wherever healthy tendencies to new theoretical formulation show themselves to-day, the truth of this observation (that all help is to be expected from the empirical method) is strikingly evident. P. 280.

and credit ; the rich German literature on value which has lately culminated in the theory of final utility ; the corrections made with the help of this theory in the much scouted old law of supply and demand ; the accompanying illumination of the theory of cost, till then either not understood or misunderstood ; and perhaps I might name certain very recent investigations into the subject of capital, of wages and the theory of economic contribution,² as among those which have brought general theory a step forward.

Let us trust to the future for proof. I make but one modest request—not at all for a blind acceptance of abstract deduction, but merely for a truce to the blind repudiation of it which has become fashionable ; that the historical school should once and for all cease to pursue a course of treatment entirely contrary to the empirical principle in the name of which it is carried on ;—namely, an *à priori* repudiation of the results of our method of investigation, upon the ground of the preconceived opinion that nothing good can come of the method. I particularly do not desire, what an incomprehensible obstinacy still fondly accuses us of desiring, that the abstract-deductive method be put forward into a place of unseemly and exclusive sovereignty. I wish merely to protect it against its unseemly exclusion, against the continuance of an intolerance in methods which has lasted already far too long to the injury of German science. I desire no battle between the methods. I desire rather their reconciliation and fruitful coöperation ; I would awaken the feeling not only that there is room for us all in the vast field of political economy, but that there is need of us all, with all our variety of talents and methods ; that, instead of turning our intellectual weapons against one another and wilfully refusing to use a better way than our own when it is offered us, we should unite our forces

² The theory of the respective contributions made by a number of coöperating factors or elements, as land, labor, and capital to the total value of their joint product. Such a theory is the counterpart, and in the thought of the Austrian economists the basis of a theory of distribution.—ED.

and turn them against the common enemy, the untold army of problems of one of the most difficult of sciences.

I venture to hope that this call to reconciliation will not pass unheard. I notice with pleasure that Schmoller himself has lately expressed some opinions which betoken an approach to the standpoint I have been defending. "I admit," he writes, in a discussion of Roscher's science of finance,¹ "that a great part of the financial questions of to-day do not bear historical treatment. The problems of the incidence of taxation have been, up to this time, so little approachable by empirical investigation that the collective theories on the subject are limited to the deductive consideration and presentation of the views of various authors." And in his characterization of Roscher he says: "Greatly as the empirical history and statistics of price have cleared away the old errors, the fundamental phenomena of fluctuations in price yet recur with tolerable regularity, and hence historical investigation is not so very necessary. The same is true of many elementary natural and psychological phenomena which constitute and influence economic life. Concerning so many things, historical tradition is so faint that, for this reason, other forms of investigation must predominate."² It is necessary only to extend to a somewhat greater tale of problems what Schmoller has here observed of one problem, in order to arrive at that standpoint which I have reached in my foregoing analysis.

My hope is strengthened by the perception that the same call for harmonious coöperation in methods is raised in highly-reputed quarters, and, what seems to me especially important, raised by men who belong to or stand near to the historical school. Let me here refer the reader to Wagner's very remarkable paper on this subject,³ to the

¹ *Jahrbuch für Gesetzgebung, Verwaltung, und Volkswirtschaft*, Vol. XII, 1888, p. 256.

² *Zur Litteraturgeschichte*, p. 156.

³ "Systematische Nationalökonomie," in *Jahrbuch für Nationalökonomie und Statistik*, N. F., Vol. XII, p. 197 *et seq.*, especially p. 241.

calm and clear exposition of it by Philippovich,¹ to Conrad's thoughtful expression of opinion, in which, with praiseworthy objectivity, he warns his colleagues not to run to an extreme in the opposite direction, as the old classical school once did in theirs. If this school held too strictly to general rules, and gave too little attention to exceptions, the present scientific tendency is the reverse—to "the exaggeration of exceptions and disregard of the great average." And since Conrad considers the present stage, "in which we are inclined to underestimate, in the study of detail, the significance of larger features, and to let ourselves be deceived by obvious exceptions," an imperfect and transitory stage, this master of empirical research gives his followers a hint of the claims and the significance of general theory, which could not have been milder in expression nor plainer in meaning.² And on the other side of the ocean, where the strife between abstract-deductive and historical method is raging, we see one of the most gifted of the younger investigators, Prof. Simon N. Patten, who has drunk the empiricism of Germany at its source, and carried it over to America, nevertheless break a lance in behalf of the indispensability of the abstract-deductive method and its equal rights.³

My strongest hope, however, is based upon the undeniable and encouraging upward swing upon which theoretical investigation proper has just entered in so many nations. Where it has been established by unquestionable practical success that truth can be acquired by the abstract-deductive method, then her most stiff-necked opponents can no longer

¹ Ueber Aufgabe und Methode der politischen Oekonomie, Freiburg, 1886.

² Referat über den Einfluss des Detailhandels auf die Preise, erstattet in den Verhandlungen des Vereins für Sozialpolitik von 1888; siehe die Schriften dieses Vereines, XXXVIII, p. 152.

³ "The ultimate laws of a science cannot be investigated in any other way." The Consumption of Wealth, Philadelphia, 1888, Introduction, p. vii. Compare, also, the same author's essay on Malthus and Ricardo, read at the third annual meeting of the American Economic Association, at Philadelphia, December 29th, 1888, p. 12, ff; beside these, the spirited review of Schmoller's book in the Göttingen Gelehrten Anzeigen, September 1st, 1889, by H. Dietzel, which I did not see until after the completion of this article.

deny her the place she deserves, and there will be an end of a conflict the very possibility of which will be inconceivable to the coming generation. And then we shall finally realize what I to-day most profoundly desire; then will be little or nothing *written* about method, and so much the more will be *done* by all methods.

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