

*The* **Nature**  
*and*  
**Essence**  
*of*  
**Economic**  
**Theory**



**Joseph A. Schumpeter**

*English edition and new introduction by Bruce A. McDaniel*

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 **Routledge**  
Taylor & Francis Group  
LONDON AND NEW YORK



First published 2010 by Transaction Publishers

Published 2017 by Routledge

2 Park Square, Milton Park, Abingdon, Oxon OX14 4RN

711 Third Avenue, New York, NY 10017, USA

*Routledge is an imprint of the Taylor & Francis Group, an informa business*

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Library of Congress Catalog Number: 2009042972

Library of Congress Cataloging-in-Publication Data

Schumpeter, Joseph Alois, 1883-1950.

The nature and essence of economic theory / Joseph A. Schumpeter.

p. cm.

Includes bibliographical references and index.

ISBN 978-1-4128-1150-7

1. Economics. I. Title.

HB75.S4566 2010

330.1--dc22

2009042972

ISBN 13: 978-1-4128-1150-7 (hbk)

This book is dedicated to  
Ms. Johanna von Keler  
Widowed Schumpeter



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# Contents

Preface	ix
Acknowledgments	xxi
Introduction to the English Edition	xxiii

## Part I

1	Introduction	3
2	The Point of Departure of Our Theory	15
3	Barter Relations	31
4	The Discussion of the Question, How the Barter Relation Can Be Grasped and Some Other Points	35
5	More Remarks about Our Method	49
6	The Methodological Individualism	57
7	About the Term Value	65

## Part II—Section I

1	Introduction to the Following Exposition	77
2	Critique of the Common Exposition and Its Relation to Ours	97
3	Static and Dynamic	119

## Part II—Section II

1	Preliminary Questions of the Cost Theory	129
2	The Liability Problem and the Questions That Are Attached to It	147
3	Elements of the Price Theory	181
4	Foundations of the Monetary Theory	193
5	The Theory of Economizing	209

## Part III

1	The Income: General Things	223
2	The Wages Theory	235

3	The Theory of Ground Rent	263
4	About the Third Static Branch of Income	275
5	About the Theory of Entrepreneurial Profit	313

#### **Part IV**

1	General Part	323
2	Examples	349

#### **Part V**

1	Nature or Essence of the Exact Economy	383
2	Value of the Pure Economy	405
3	Once More the Boundaries and Defects of the Economy	425
4	About Reforms and Attempts at Reform	433
5	The Developmental Opportunities of the Theoretical Economy	441

	Index	461
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## Preface

The following saying makes a lot of sense: To understand is to forgive. Better even: Whoever understands sees that there is nothing one has to forgive. And that is also true in the field of knowledge.

The layman sees perfection in the body of knowledge at his time. Axioms of former systems he sees as “wrong.” For example, the “wrong” Ptolemaic system has to make room for the “correct” Copernican one, which is accepted now. If he would see that even the most modern theory is nothing but a temporary framework—to be replaced sooner or later by new descriptions (because sciences are nothing else than that)—he would lose faith in science. The term “failure of science” describes perfectly the impression that a lot of people have of this insight. In our area, that is not only the standpoint of “laymen.” While physicists and mathematicians are like well-trained soldiers who stay in their foxhole even when the battle turns ugly, our authors do not display such steadfastness. This might be a sign of the relative young age of the social science that its representatives rather easily change directions and often ignore previous work in the area. It also seems that scientists in our area tend to ignore commonalities and rather stress differences, to the point that reforms are pushed through as radically as possible, or in other words: That one prefers a totally new construction instead of building onto an existing foundation. Therefore, the contrasts in our field seem to be so insurmountable, not only between the various directions but even in the underlying theory which will be discussed in this book.

This is not my standpoint, though. Like so many of my colleagues, I agree that each “direction” and every individual author is “right” with his claim: Compared to the aim or purpose of each claim, there are few that make no sense and can be dismissed as wrong. We may have reason to prefer a different viewpoint but that usually does not give us the right to dismiss the one that is opposed to ours. The old idea has fulfilled its function and a new one might not even be possible without the previous

one. To think a thought all the way through is important, even if it turns out in the end that it was utterly useless. But most of the times we can gain something out of any theory.

We would like to work hard to understand and appreciate each and any of them, especially by spelling out their underpinnings, which are often neglected even by their authors. And then most of the times we find that the ideas are absolutely logical and any controversy would be mute. We want to understand, not fight; learn, not criticize; analyze and find what is correct in each sentence, not just simply accept or dismiss.

We not only want to do this with different opinions within the theory but also with the different “directions” of economics and we will always stress that there are absolutely no dichotomies, in the sense that one might be worthless and the other one “correct.” In this way we do not share the partiality of most economists, but are absolutely willing to do justice to everybody, as long as we understand them. And today we are not alone with this viewpoint. But there is a point that makes understanding difficult in our field; and that is the problem that the researcher is at the same time politician and that his research is very often not unbiased; but we believe that theory and practice not only can be separated but they actually do not have anything in common. Even if you do not agree with us in this point, you could not disagree with us when we say that the quarrel between pure theory and history is a thing of the past. In any case, we do not want to be part of it and rather investigate in each separate case, whether the one or the other methods of investigation should be preferred. This way, we will not get to an all-encompassing answer but to separate ones for each individual case.

So, this book is not partial. The reader will find absolute composure in this book. We do not follow any type of scientific or political dogma. It would not have been difficult for me to write exactly the opposite of what you read here, if I would have thought it to be correct. And why not? I am far away from any practical politics and only try to find understanding; and there is no reason for me to prefer one specific method or different materials from the ones I used. If they would better lead to my goal, there would not be any reason to stick to my previous ways. On the contrary, it would be a pleasure—and I would expect a lot of stimulation and satisfaction—to switch to a different method and gain additional knowledge.

And I also do not care where an axiom that I support came from, what the origins of theories or directions are. It is the task of the dogma historian to be just in this case—we care about the problems not the persons.

Just in order to be clear, I do talk about “Ricardo’s Systems” or “the Austrian School” etc. because these are common expressions. Everybody knows right away what is meant by them without having to go into long, intricate descriptions, although they might be more correct.

On the other hand, in order to fully understand a theorem we must understand its dogma-historical background. But we cannot and we will not strive for perfection in this regard. Since this book is not geared towards the beginner or layman, we expect the reader to have a certain background knowledge of our science and will understand my abbreviated outlines. Only with a rather exact previous knowledge base of our science the reading of this book will be fruitful. I have to stress, though, that unfortunately beginners in our field all too often do not have this background of all aspects of our science and its theory due to the fact that they specialize too early. I had to write the way I did in order to prevent this book from becoming too unwieldy. For example, when I talk about the “Boehm-Bawerk Theory,” I do not go into all its details. All the theory’s elements will be discussed but that will not be sufficient for a full understanding if the reader of this book has not also read the original work. But I will pay special attention to my German audience by providing as much background information as possible in an area, which they might not know as well.

In general I try to avoid citations and names as much as possible. Here I follow the English tradition, which seems to have a lot of advantages: completeness in the discussion of the literature is impossible and it is unjust to praise or blame individual writers. The reader has to know which approaches or lines of thought are being discussed when we use the phrases “a popular theory” or “it is often said”—individual writers do not play a role here. And the reader will also have to be able to judge what in this book is an original idea and what is just a summary of previous knowledge. If someone finds common knowledge, which I thought to be my own original thought ... that would only make me happy.

The work of the latter grows organically out of the work of the former and I happily cling to the old views where it is possible. The less new we have to do the better. The author of this book seems to be closest to L. Walras and von Wieser.

Although you will not find many names mentioned in this book, it nevertheless contains most ideas of the current science of theoretical economics, and it therefore gives a complete overview of our discipline. I hope I have included all points of departure for further development. It was always my aspiration not to deconstruct but to develop our field



further. Any exact science has to build its rocky path, step by step, and not care that the general public sees our efforts as insignificant. Unfortunately, that does not happen enough in our area—often it seems every new book wants to found a new economy. That will improve with the maturation of our discipline. We want to remember and appreciate all the previous work in our field but also not deny that there is a lot more left to do. The number of basic thoughts is small; quite a few—but not all yet—are complete.

But still, the reader might not find full satisfaction in this book. Some points, which we think are especially important or previously not addressed well enough, we discuss in depth but others we only touch upon briefly. What we see as having been discussed thoroughly in the past—even if it is very important—we might not cover at all. Please, dear reader, remember that this book is neither a textbook nor a systematic work that sees it as its task to cover all aspects of our discipline in equal depth.

What is being offered here is supposed to add to the existing knowledge, not repeat what already has been done. We want to look forward—backwards only when necessary. Furthermore, we are not very interested in the theorems as such, but more in their nature and their place in the system of our science. We use important practical questions just as examples for the types and findings of our thinking processes. So, we probably will sometimes stop our discussion at a point, where it will become interesting to some readers and in those places our incompleteness will become apparent. But it lies in the nature of this cause that only few questions can be discussed in depth. I still do not think that anybody who bothers to read this book will accuse us of superficiality or a lack of knowledge in our area.

The topic of our discussion is just a very narrow area of the social sciences, which allows for a very exact discussion.

It may be that just the term “exact economics” will make some readers shudder. Those of you who do not like exact disciplines ... please put down this book without reading it. I would not want to blame anybody for it. And those of you who are of the opinion that we cannot deduct answers to practical questions ... you are right! Some things are important for the practitioner, others are important for the theorist.

The classic system of national economics lies in shambles. Nevertheless, it is still seen by some as *the* economics. Many authors turned their backs on it and instead dedicated their work to other areas, which do not have much in common, neither methodologically or content-wise. In ad-

dition, a new theory was developed, based on different basic assumptions and different goals. That looks confusing, even chaotic.

What is the relationship between the theory and the other directions and what is the relationship between the old and the new theory? And what can we expect from these relationships? And in general: what now? Are there paths that lead forward and where are they? All this has been discussed in detail but the picture has not gotten any clearer—although an answer starts to shine through. We got to this point through basic, general, and *a priori* argumentation—often from outside from our field—but never going into details. The discussion looked more like a political fight: slogans used by partisan supporters instead of calm analysis piled up many misunderstandings, which will be hard to get rid of. Although advanced researchers are beyond this bickering over methods, we can say without exaggeration that many economists are utterly confused by these questions and do not know where to go from here. Every one of them can say where he stands and defend his stance with commonplace arguments but they cannot say anything about the basics, about national economics in general, about its problems, its nature, meaning, and future; unfortunately, all these areas are shrouded in a fog—and not only for people at the fringes of our field.

We want to try to find answers here. But not with common argumentation, which would not be wrong but would lead nowhere; not through a “dialectic approach” that proves everything and nothing; but rather through our work.

We always will try to clarify each of our theorems, its value, and its nature. Thus, we will develop something like a “cognition theory of economics” or at least try to move into that direction. I am convinced that only this way we can solve all problems, not through common argumentation. Up to now, every economist has started his analysis with *a priori* axioms about the nature of economic activities or human activities and then deduced assertions for this or that method. That cannot lead to any results. The sentence “Everything that happens is subject to causality, therefore exact laws have to be possible in the area of economics” does not prove anything. Aside from the fact that the modern theory of cognition would reject it, the question still is whether the causal connections are simple enough to allow for axioms of a sufficient interest. And that’s what is important.

On the other hand, a sentence such as “Laws like we have them in the natural sciences cannot be used in the humanities” is also worthless. Again, besides the question whether and in what sense it is correct, it does

not exclude the existence of regularities, which can be described exactly. And whether they really exist can only be shown by the analysis of individual problems. That leads us to a second point that often is neglected. General methodological books often do not deal with concrete problems; they discuss common assertions; often they show a lack of knowledge of details of the theory—and that is not only true for the works of Wundt and Siegart but also of trained economists. Even the field of economics is staked out through general reflection. And even methodological discussions in prefaces of books that deal with concrete problems show this dilemma: There is no connection between these discussions and other followers but they rather are nothing more than a mere creed that belies the reality of human actions. They, for example, profess the necessity to include historical materials or that they do not just want to compile data but rather find “laws”—but in reality they do not do that. They say that they would not make any practical suggestions, only for the reader to find out that they do exactly that. They talk about the necessity of statistical basics but in reality they often just mention statistical data in examples but their conclusions are being reached through reasoning not through statistical analysis. So, it is no wonder that we can sometimes totally agree with the argumentation of an author although there is a total bewilderment of his direction.

It is our opinion that we should not cobble together methodological concepts in *apriority* but rather do what takes us the furthest regardless of our expectations. Especially the field of economics should not be staked out in advance. Instead, we should calmly approach the questions that interest us and clarify them. The method that seems to be the most useful to this goal still does not have to be generalizable. Of course, we will continue using the methods, which might or might not work but the latter does not mean our method is a bad one, nor does it mean the former is good. The tendency to generalize the usefulness of certain assertions (which are good to solve certain problems), leads to the odd situation that they can be defended through general reasoning and through certain examples without really satisfying anybody. It is easy for an opponent to prove exactly the opposite by using a different reasoning and different examples. And since both look at those aspects close to their heart, they will not be able to agree on anything. Each of them is totally convinced that he is right (and can actually prove it!) but the beginner in the field cannot get a grasp of anything.

These words summarize the whole history of the method dispute. It is not our goal to find generalizable axioms; the only actual axiom that

is generally applicable is: act rationally. We also do not want to support one side or the other. We want to put those axioms that we deem to be correct in relation to each other, want to show their limits and their range, and apply them to individual cases in order to see whether or not they pass muster.

One cannot separate the analysis of methods from concrete problems. Only then do methods make sense. The details are important ... great generalities have no substance. Only through our work we can come up with laws, which will then be subject to improvements, changes, and rejection. The description of the method should not be the first chapter but rather the last one. They are of little help to us. Besides ... economists still ignore the latest—which in our eyes is the best.

Just one example is the discussion about induction and deduction. First it was conducted in a very general language. The best that was said about it was that both processes were equally important—who would have expected differently? That was just obvious and does not help us very much. Interesting in this context is only the analysis of the character of each of our axioms, each of the steps we take. That is necessary to evaluate the meaning and value of it. And then it shows that some axioms were arrived at through deductive methods others through inductive ones. Each path, if based solely on one or the other method, had to be dissatisfying. When the field of pure economics was defined as “deductive,” the field was not treated fairly: While some of these attacks are not totally baseless, they went way too far. The controversies within the pure theory are quite similar. One example is the so-called “value controversy.” At first, people operated with the terms “false” and “true” instead of “useful” and “not useful.” That the sun “rises” is not “false” and does not contradict the axiom that that phenomenon is caused by the movement of the earth: both axioms are descriptions of the same process and are in themselves both true and false; towards some goals the one is more practical, for other goals the other one is—that is all. We then will not even try to have a *general* discussion about value or cost hypothesis. Instead, we will indicate why in the one case we use the one and the other one in a different case—if we do not use both. This—let me call it “pragmatic” approach—has not been used up to now. It allows our findings to be much more precise as if we would try to be very general. In addition, the controversy loses a lot of its edginess, it dissolves automatically, and it will become clear that justice and injustice can be found on both sides.

This approach to our problems might seem a little odd. But it is in line with the modern *cognitive* theory, which has grown out of practical work of the exact natural sciences. We do not want to go into more detail, because we want to avoid being perceived as biased: our comments should be seen as natural and impartial, as they were written unaffected by any axioms. Just in case that some of my comments or phrases will be seen as partial, I would like to note that I am not alone with my view of the cognition theory. I am prepared to take criticism for my comments on the value hypothesis and some related questions. I still believe that my approach is the only one that can expose the nature of the economic theory. And it should not be sacrificed for a more popular approach.

In this context I also want to mention that I do avoid the terms “cause” and “effect” but rather use the more poignant *functional concept*. We cannot go into a more detailed discussion at this point about how important it is, how it can add to the clarity and purity of reasoning. But I believe that it is especially important for exact economics to use rigorous correctness, even if the writing becomes dry and lifeless. For people in our field it is much more important than for people in those disciplines, which already have achieved a certain level of clarity in their foundations and reliability in solving concrete problems.

Clarity in the foundations and reliability in the solving of specific problems: That is what we are striving for, something for which we admire the exact sciences, and to which we want to contribute. There are many stumbling blocks in our path before we can even tackle the real problems in our science, and all the discussions we have already had have not been able to push them aside completely. Our task is not so much to find new ways to solve problems but more to prove that it is possible to navigate around them and not get stranded. The questions of “Telos” and “Causa” cannot be solved in the framework of an exact discipline, they can just be neutralized, so to speak: One can show that they do not barricade our path—and that is the same with many other similar difficulties.

It would be unnecessary to quarrel whether economics is a “life science,” as it is often described, and therefore closer to biology as, for example, mechanics, if one can show it is utterly irrelevant for our results. And such comments have made a big impression on a lot of people and shaken their trust in our discipline. There are a lot of such slogans and all contributions to a “cognitive theory” of our science are full of them. We finally have to respond with precision to the question what of that

is actually true and which implication they have. And this response to many important points will come out of this book.

We want to work out what we should think of today's pure economics, what its nature is, what its methods and findings, and where we should go from here. We want to show its limitations and weaknesses and we want to show to the reader how the latter can be improved. Even in this point people are too rigorous: Either one thinks the existing is perfect and does not need any further development or one rejects it totally. Both are equally superficial and convenient. Looking at individual cases, though, we see that neither of these two opinions is totally correct but rather each has elements of the truth. Everybody feels that without being able to say which of the two fits which concrete axiom: this is something we will do here.

You will not find here the general arguments, neither about political nor methodological nor other basic questions. What has to be said about them has already been said and is common knowledge. Only in very few areas will we add to this knowledge but we will not waste our time on generalities which are true and cheap: Our work on concrete problems teaches us our method and gives us our basic stance on the basic questions and in the individual directions of our science. We do not assume beforehand that economic facts show enough regularity that would allow us to come up with exact rules, but some will rather reveal themselves to us including their nature, their limitations, their weaknesses, and their value. We will see that we will be able to use certain axioms and that these axioms will form a consistent, self-contained system and will show its value and whether it can be generalized, and also in how far it is based on arbitrary presuppositions and definitions or on observations of facts. The results of this work on details will be quite different from those general *a priori* discussions. But enough of this.

I would almost say that the concrete results are only of secondary significance for my goal. As I said before, I do not strive for systematic completeness. I will bring up only a small number of basic axioms. In the center lies the problem of balance, which from the standpoint of practical application of the theory is of negligible importance but it is fundamental for the science. In Germany, it has not received the attention it deserves but it is important to stress that it is the basis of our exact systems. The barter, price, and money theories and their most important application, the distribution, are based on it and therefore it will be discussed in length. These things form that part of economics, which is ripe to be discussed.

My presentation is based on the fundamental differentiation between “statics” and “dynamics” of the economy, a point that cannot be stressed enough. The methods of pure economics today are just good enough for the former and the most important results apply to them. “Dynamics” is totally different from “statics,” in their methods and in content. Of course, the differentiation is not new ... especially American theorists have stressed it. But in Germany it has been widely ignored and its implications have not been recognized in other countries either. We will see that it actually contains the key to the solutions to many controversies. It is so important that it cannot just be summarized in this preface but it will come up with almost all concrete problems. We will only deal with statics; the field of dynamics will just be mentioned in passing.

In this context, I want to mention a point close to my heart, and that is what you will read here about the problems of capital and interest. I apologize in advance that you will only read negative results of these problems. The most important axiom you will find here is that interest is not a “static” income but that it is more closely related to entrepreneur profit than to pay or income from property. I know very well that most theorists are of a different opinion. But I am convinced that that fact explains my dissatisfaction with all interest theories I am familiar with. But I had to stop with this result, so that my own interest theory would not be compromised by a premature discussion. I hope I will have the chance in the future to discuss this matter more completely than I can do it here. I do not like to call the existing theories insufficient... but I have to. To present a new interest theory—probably the 25th or the 39th—is not an enviable task. I did not look for it, it forced itself onto me.

I would have more to say in this preface of this book, for example I could write about the significance of the allocation problem; about something that I have called “variation method.” These dry topics can only be of interest to the theorists of our field who actually might find them wanting. But it lies in the nature of the thing that in our science it is very difficult to satisfy both the theorist and those practical economists who nevertheless are interested in theoretical problems on the one hand, and on the other hand not to be too abstruse and too incorrect and at the same time trying to apply the scientific rigor of the exact sciences and not betray the peculiarities of our field. Probably the last part of this book will be of general interest.

One of my goals is to familiarize the German audience with a few things—terminology, axioms, and viewpoints. In Germany, the development of the theory has not been followed very closely; the German

economist only knows vaguely what the “pure” theorist actually does. We want to teach to the German scientists the theories of other countries.

One of the most important points in this context is the question of the “mathematical method.” Some readers might have not heard of it at all, others might have just heard general reasons in favor or against it. It would be unnecessary to discuss the general reasons, which tend to become ever more important in the English literature because of the rapid development of this approach. Furthermore, we do not want to present long mathematical deductions, because there is a lack of foundation among the readers and it would only scare them away. But we think if one wants to deal with theory, one has to do it as exactly as possible and that our way of thinking will totally absorb the thinking of higher-order mathematics. But we still are not saying that mathematics is absolutely necessary because our terminology is of quantitative nature or that complicated problems could only be solved through mathematical correctness. We are content with discussing the nature of exact reasoning in our field and to present a few points when our thinking becomes mathematical, whether we want it or not, and describe what happens in such a situation; what this process means and what can come of it. The reader then has to decide himself whether it is worth his effort to go into more detail and what to think of the criticism against this process. We will never go to a point where mathematical knowledge would be required. That would be detrimental to the goal of this book. And we hope to support tendencies and win people over instead of discussing generalities and criticizing the reader for not having enough background knowledge.

I am not a polemicist and I do not feel bitter. I have trust in the future of our discipline and do not regret anything about the past. With equal distance from authoritarianism and clinging to the old dogmas on the one hand and inconsiderate destructiveness on the other, from melancholic or complacent skepticism on the one hand and gushing hopes on the other, I calmly anticipate a new scientific day, which is about to dawn.

Cairo, March 2, 1908  
J. Schumpeter





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## **Acknowledgments**

I would like to thank the Kauffman Foundation and especially Carl Schramm and Robert Strom for the generous financial support and encouragement in my efforts to complete the English translation of this book. I would like to thank Dr. Peter Kastner and Franziska Wilcox for their many efforts in the actual German to English translation. Also, I would most importantly like to thank my mentor, Professor J. Ron Stanfield, for continuing to encourage me to read the works of Joseph A. Schumpeter during and after my doctoral studies.



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## Introduction to the English Edition

At the young age of twenty-five, Joseph A. Schumpeter published his first book in 1908, *The Nature and Essence of Economic Theory*. Published in German, this book is little known and has not been acknowledged as a major accomplishment in that era in the field of economics. Given the detail, complexity and depth of this text, such an undertaking would not be attempted today by any but the most seasoned professional. Yet Schumpeter, in this book, undertook the overwhelming task of producing a coherent, formal, and complete analysis of market capitalism. At this early age, Schumpeter attempted to strongly differentiate between static and dynamic economic theory. An example of this dynamic analysis includes his attempt to show innovation as an ongoing integral part of economics and place the entrepreneur at the center of market capitalism. This book set the stage for his myriad of later works. More importantly most of his contributions, including his work on entrepreneurship and innovation as well as his concept of creative destruction can be traced to this original text.

Entrepreneurship and its activity, innovation in the form of new products and new production methods, according to this first Schumpeter book, would disturb the conditions of static economics and could only be fully understood and analyzed through the development of dynamic economic reasoning.

Schumpeter's work on general equilibrium analysis expands much of the early works of Leon Walras and is combined with his studies at the University of Vienna. In addition, through his travels to England and his personal discussions with Alfred Marshall, Schumpeter built his analysis on the base of classical and neoclassical economics but expanded his views with the incorporation of a vast array of works from economists and sociologists from the 1700s as well as the 1800s. These formulations in the book, comparing, contrasting and at times refuting a wide variety of economic and social thought from over a century ago, makes this

original text from Schumpeter a most compelling read for professionals from a wide variety of fields.

As economists, sociologists, social scientists and other professionals look for solutions to the many social and economic problems facing society today, they are increasingly drawn back to the classical writers to search for forgotten or missed ideas from the past. One such source would appear to be Joseph A. Schumpeter's first text written in 1908. Since this book was originally published in German, this is the first opportunity for professionals to explore the original Schumpeter in the English language. This was Schumpeter's first attempt at a complete, comprehensive, integrated analysis of how economics becomes part of the social, political, and institutional structure of a nation. Schumpeter forms the foundations for much of his extensive social and economic thought in this first work. His later analysis of how society accomplishes economic growth, material advancement and income and wealth distribution and redistribution find their origins in this book. Schumpeter may be most recognized for his concept of creative destruction and although the phrase is not used in his original book, the ground work and conceptual frameworks for this concept is developed in this 1908 text.

As he attempts to develop a comprehensive general equilibrium analysis, he integrates much of the classical and neoclassical economic thought of that era. The book is at times tedious with lengthy and complex discussions and developments but remains an important contribution to the early attempts at understanding the discipline of economics and its relevance to society. He attempts to add to and further develop the works of economists such as Marshall, Pareto, Walras, and many others. In this process, he attempts to explain the many facets of economic analysis including moral, ethical, scientific, and theoretical as well as delineate the differences in each area. For example, he explains that entrepreneurial wage has "social justification" and therefore is not part of "scientific theory" in the economic sense.

Schumpeter viewed market capitalism as one of the few social structures that allowed social advancement through increases in income and wealth. He explained that while systems such as the aristocratic or the cast structure prohibits such advancement, market capitalism offered both economic as well as social advancement. Part of this upward mobility was defined by Schumpeter to be the result of the entrepreneurial efforts of members of society. Schumpeter proposed the concept that the negligence and complacency of the economically and socially powerful would allow an opportunity for the entrepreneur to profit and redirect

income away from the more elite in society. He stated that this “upper-class” did not want change but the “less advantaged” were often if not always looking for entrepreneurial opportunity to better themselves. These opportunities would offer economic advancement first and then because of the individual freedoms of market capitalism, social advancement would follow.

The book is divided into five sections covering the methods and theoretical foundations of economic analysis. Schumpeter starts the book with a lengthy analysis of value and prices. Schumpeter writes the text in a style that constantly refers to previous explanations or suggests that a more complete discussion will occur later in the book. At times this proves quite difficult for the reader but this style was quite common one hundred years ago.

The second section develops the concept of equilibrium and Schumpeter states that this concept is essential for economic theory. The third section focuses on production and distribution and includes an analysis of rent, wage, and profit theories. Schumpeter goes to great length here to discuss and contrast static and dynamic economic analysis. The fourth section discusses the analysis of comparative statics. The fifth section attempts a long discussion of examples and methods to analyze the concepts set forth in the first four sections. Parts of this last section review and repeat earlier parts of the book. At the end of the book, in this last section, Schumpeter explains through many different examples how dynamic interactions between society, institutions, and government actions such as taxation work to bring about social as well as economic change continuously alters society. These interactions and the results are reasons that Schumpeter argues that economics is and must be a dynamic analysis and therefore cannot be fully explained with the static models of classical and neoclassical economics. According to Schumpeter, this process of dynamic interaction is an integral part of income and wealth creation as well as income and wealth distribution and redistribution.

The reader must be aware and recognize that over the past one hundred years, many economic concepts and terms have changed meanings. For example, Schumpeter in his original text uses the term redistribution of income to mean a redirection of income streams from the economically elite to the economically less advantaged. Today the term redistribution is usually associated with the government taking income from one group and awarding that income to another group. Schumpeter’s use of the term redistribution referred to changes such as entrepreneurial activities to allow a redirection of future income flows away from one group to another

group. He did not use the term, as it is used today, to represent a taking of existing incomes from one group and awarding that existing income to another group. Today this change in future income flows that Schumpeter discusses would be referred to as a redirection of future income.

This book offers much insight into the thought and economic depth of the young Schumpeter. This text was the academic start to an economic career that spanned over four decades, several countries, and many different appointments at both private and public institutions. Now in the English edition, hopefully the book will rekindle the interest in exploring the works of Joseph Schumpeter that are well deserved.

# **Part I**





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# 1

## Introduction

### I

Even those people who know our discipline just superficially, know about the multitude of approaches and the quarrels between their disciples. It might confuse, even deter, the beginner or the layman but it is neither special nor disturbing. Of course it is irritating that one cannot name *one* book that is widely recognized and that could summarize the position of the science so that one could recommend it to the public as general information about our field. The reason is not that the dichotomies were insurmountable; actually, I hope to be able to show the opposite in this book. Also, we cannot say that this dichotomy does not exist in other sciences as we will see shortly. The reason for this unpleasant situation is thank God a superficial one: The bitter quarrel is a rather recent phenomenon because each economist, instead of stressing what we have in common, seems to reject all positions that are not his and tries to pull the beginner to his side and make him into a warrior for his own position. So, these beginners are pulled into the controversy too early, way before they understand the problems. They often have an opinion before they have even started to work independently. Political or other tendencies that are outside the field are also to blame.

But, as I said before, the field of economics is not in any worse situation than other sciences. It would be wrong to conclude that the field needs useful methods, assured results, or even a clearly-delineated field of study, as understandable as it is that some people—tired of the constant infighting—may think that way. Since this declaration seems paradoxical considering the high noise level of our discussion about methods, we would like to briefly explain. We could name philosophy, political science, and other fields that can hardly be described as exact. In the field

of psychology, for example, there are many different directions and we can safely state that in our field there is no difference bigger than between introspective and experimental psychology. Scientists coming out of totally different disciplines work in these subfields, of which the one tends towards philosophy, the other one towards psychology. Methods and findings of these two are hardly related and psychologists spend at least as much energy on the defense of their basic viewpoints as economists do. In the field of logic it is not any different: There are worlds between “categorical” and “modern” and these two areas have not been found together. But please do not confuse this dichotomy with controversies about single problems: Not a single question but rather whole approaches, whole systems with all their basic assumptions are at stake here.

It is even more significant that we also find this phenomenon in the exact sciences, which the laymen see as the quintessence of certainty and unity. The best example is chemistry: The exact and the experimental are represented by different people and they have little in common concerning their methods and their goals. They go their separate ways and only meet to quarrel.

And the same is true for the most exact of them all, the mechanics. That is especially peculiar since all the workers in this old traditional field have a remarkably similar training, career path, and opinions phenomena in their field. In addition, they pretty much agree on all concrete findings. But all of this still cannot create unity when it comes to methods and basic principles. Not only is the difference between classic and modern mechanics considerable—which is the natural result of scientific development – but there are also clearly distinct parties within modern mechanics that fight about more or less *everything*, the complete interpretation of nature and the values of the discipline. And as in our field, the practitioners do not seem to be interested in anything but what is of practical interest. Scanning the battlefield, it becomes apparent that today the foundation of this proud building is shaken and that there is a general feeling of dissatisfaction. Doesn't this show us that all sciences go through these quarrels, not just economics; that all systems have to make room for new ideas? And wouldn't it be wrong to be in despair over the fall of classic economics? One could actually interpret it as a sign of its capacity for development.

One could reply that the discussion about methods in our field is characterized by such a high level of intolerance towards the viewpoint of the opponent, that people do not even know their approaches and findings. But that is the case everywhere: The introspective psycholo-

gist feels misunderstood by the experimental, and the latter is inclined to deny the former any right to his way of thinking. The mathematical and exact chemist often disdains the experimental one and the latter, as I have experienced, sometimes does not even know the first one exists or what he does. And today the field of mechanics is in a similar situation, and even pure mathematics suffers from it.

We actually should not be too surprised since not only is the knowledge so vast but the methods also so varied that only a few people have a handle on *one* whole discipline with all its subfields. He chooses what is closest to him and his subfield is often such a large part of his personality that is as hard to shed as the moral character—especially for the best in their fields. The result is often the incapability to compromise. But compromises are a necessary result of scientific development and fighting them is as futile as trying to unify the world religions.

So, we do not take the inner strife of economics too seriously. We are pretty sure that the state of our field today will not lead to a destruction of our discipline in the future. We are supported in this by the observation that things have turned around recently and that a healthy “communis opinio” is developing. At least, that is a result of our discussion. On the other hand, we cannot and we do not want to deny that the situation is not completely satisfactory and that we will state here what we think the reason is.

Let me take the fight between the representatives of the abstract theory and of the historical school: Usually, both are right in their general assertions. But we do not look at their limitation that they often deal with different problems. Each method has its own field of application and fighting over generalizability does not get us anywhere. We will always stress that a discussion about methodological questions only makes sense in the framework of practical scientific research. Our viewpoint is that there is no contradiction between the historical and the abstract approach—their only difference is their interest in different problems. The pure price theory, for example, cannot be dealt with historically, the problem of the organization of an economy cannot be dealt with in an abstract way. And our discussions would not be as controversial, if we would have always kept that in mind (and some other things we will discuss later). People realize that more and more, although not all of them.

## II

Here we want to give a short overview over the present important approaches. We follow a long tradition. Almost all economics books,

especially the systematic ones, start with such an overview. This way the reader learns about the philosophy of the author and gets an introduction to the literature. We also have to do that, even if in this case the reader is expected to have prior knowledge of the topics we will discuss here. We have an additional reason: We see it as our task to contribute to a deeper appreciation of each approach, to demarcate them better, and hopefully put them into a more precise relationship to each other. To this end, several of them will be named here without going into any type of critiquing at this point but rather discuss them pretty generally. The reader will only be able to develop a yardstick out of the entirety of the discussion; we do not want to come to a judgment at the threshold of our discussion, as it so often happens. In this general information we will stress the points to which we ourselves can contribute. The general historic development of our science as well as what is usually written in this context—all this we expect the reader to already know. The intentions of our discussions are the excuse for their incompleteness and we mention names only in examples and as sparingly as possible, and only when non-mentioning of names could lead to confusion among the readers.

We will start out with the systems of classic authors, especially A. Smith, Ricardo, and their immediate successors, without analyzing how much they are dependent on previous authors. We have to do that for two reasons. First, they are the origin of most approaches and we have to discuss them in order to understand these approaches. Secondly, they are still alive and powerful, insofar as some economists still represent them. Let us now look at the first point, which presents a window to the development of our science.

*Natura non facit saltum*—Marshall prefaced his work with this sentence and it really captures its character very well. But I do want to contradict him: The development of human culture, and here especially that of knowledge, happens in spurts. Colossal rushes alternate with periods of stagnation, effusive hope alternates with bitter disappointments, and even if the new is based on the old, development is not a steady one. Our science can tell stories about that.

The works of the classic authors seem very fresh, still. That a cornucopia of facts and findings, of points of departure of which even today not all have been utilized, is offered by the “Wealth of Nations”! Scientists pushed forward without checking the reliability of the direction and exploited the break with the past. These new ideas broke through into wider circles—often misrepresented and *always* over-generalized. Disillusionment followed fast and we had a situation like an economic

crisis: Exhaustion followed a period of productivity, exaggerated mistrust followed unconditional trust. What is characteristic in this situation is not the attitude of wider circles towards economics but rather its internal condition. Suddenly we have a standstill in its development, it looked as if the field had been exhausted and nothing could be extracted any more—and that in spite of obvious flaws that invited further work: No workers could be found. Only half built and already half dilapidated was the building of economics, when powerful opponents appeared. I cannot explain this strange standstill, these Hippocratic elements in our literature between 1830 and 1870. Most people with knowledge of the literature would probably support me if I said: The classic system was not destroyed by outside enemies but rather by internal paralysis (as the demise of societies cannot sufficiently be explained by the existence of outside enemies). The historical school stormed a fortress and only found a bunch of invalids in it. The works of the epigones would be of little value, even if a historic approach had never existed. We do not want to deny that *something* was achieved during that time: Almost every author contributed this or that detail. But the creative power was exhausted. That is especially true for J. St. Mill, as embarrassing as it is to judge an individual this quickly. There are also some beginnings that indicated promise but it is characteristic for the paralysis of the field of economics that they did not get any attention.

The impression I got from the literature at that time cannot be described any better than through the term “being stuck.” Perhaps even Smith and Ricardo would not have known where to go next. That is definitely true for their successors. Their approach came to the end of its productivity, and there was no replacement. It makes sense that people generalized what was only true for one approach and thought that the whole field of economics would not have a shining future. Some thought the system was perfect and complete—something that is always a dubious symptom—others had a feeling of uneasiness without having a solution.

This situation became very clear during the celebration of the 100-year anniversary of the “Wealth of Nations” at the Political Economy Club in London. Actually, the year 1876 is already part of the new period. But the works of the new writers were still being ignored and our discipline seemed to be shrouded in the tranquility of death. Mr. Lowe who opened the debate expressed it perfectly: “I am not sanguine as to any very large or any very startling development of political economy. I observe that the triumphs which have been gained, have been rather in demolishing that which has been found to be undoubtedly bad and erroneous, than

in establishing new truth; and imagine that, before we can attain new results, we must be furnished from without with new truths, to which our principles can be applied ... the great work has been done." In other words, it means that the field of economics was "done," that nothing new and noteworthy could come out of it anymore and that one had to go beyond its confines to learn anything interesting. The only ones who showed self-confidence and initiative and were forward-looking were the "historians," especially Cliffe Leslie. And a newspaper expressed the impression of wider circles perfectly when it described the get-together as a "wake," not as an anniversary of economics.

With the loss of its internal power, our field also lost its external influence, especially since at the time of its upswing the field had gone out on a limb by tackling the area of practical problems and had given short and general answers to questions, which were too complex to solve on the first try. As the scientific building started disintegrating brick by brick (wage fund theory, population theory, etc.), one practical finding after the other came apart. And people had heard so much about economics, and the field was so pretentious, and the misuse of this science so evident, that people turned their backs on it.

This way the historical approach showed one huge success: The theory with which one could show everything or nothing, that became paralyzed in empty phrases, was thrown overboard and the field began to concentrate on collecting facts and practical problems of social and economic politics. But it was not a complete success. The discussion of current problems was still based on classical arguments and the free-trade party and the Manchester party still did not want to let go of their beloved theories. That alone, though, would not have had a big impact on the science. But many scientific economists also stuck with the theory. For a while there was the hope that these would be swept out after a short while. But that never happened. Instead, new activity sprouted in the ruins, and the flock of theorists began to regroup to grow and go on the attack again. The historians were not aware of the fact that they were faced by different opponents—they thought they were the remnants of the successors of the classical authors. But they were *new* warriors who picked up the same old quarrel about methods. It was a mistake to fight them with the arguments used against the classical authors. But they brought it on themselves by seeing themselves as grandchildren of those authors.

The reader knows which group of economists we are talking about: Menger, Jevons, Walras, and their successors. Their situation was difficult

in the beginning. The period of being ignored was followed by being attacked and of many misunderstandings. The theory was put aside and people were not ready to accept it again. But the new approach weathered the storm, made progress, and today we can say we are on a theoretical upswing. The classical system did not benefit, though—quite the contrary: it suffered another attack, which totally shook it.

In order to fully understand this, we have to be clear on the nature and content of what we call the classic system. The first thing that is striking when one reads the classic literature in my mind is the fact that it consists of many different elements. It is astounding that this fact has basically been ignored, and we think the reason for that lack of results of the methodological discussion is the fact that these different elements were not sufficiently isolated and arguments that fit just one of them were over-generalized. The heritage of the classical literature consists of a scientific part and a political one. I do not think we go too far when saying that the big success and the devastating defeat of the classical system can be explained by the latter rather than the former. The attack of the historical and the newer socio-political approaches were directed against free *laissez faire* trade, the victorious catch words during the first half of the 19th century and against practical extremes of other theories. These circles had no interest in the real economic theory. Still, it was expected that this theory would fall if faced with those practical assertions and postulates. Those are not necessarily consequences of the pure scientific classical literature and can be separated from them. It is not difficult to prove that: For example, it is easy to see that the theoretical content in Ricardo's chapter on wage does not necessarily lead to what the author calls "poor laws." If one rejects the latter, the former can still be true. And we are only interested in the scientific heritage of the classical authors. But even that is not completely homogeneous. Yes, economics is its most important and most valuable part. But it also contains philosophies about the topic of individualism and collectivism, about the motives of people's actions, etc. That all that does not have anything to do with economics, we will show later. We can—and have to—admit that the attacks were legitimate in this point, too. But that is all; the pure economics of the classical authors, as paradoxical as it sounds, remained almost untouched. Attackers did not even get through to it and only charged it in general, in connection with everything around it. We can still observe this phenomenon in today's discussion.

It was the representatives of the new theory who investigated the classic economics. Did they destroy it and put something new in its place?



That is a question that is being answered in many different ways. We do not want to solve it here—the following discussions in their entirety will give an answer—but we do not want to hide our opinion: Yes, the system of the modern theory is mostly new and even those findings that concur with those of the classic system have been derived through a different method. Of course, we are indebted to the classical authors for the terminology and thought processes; sure, without the old one the new theory would not be possible but the former has become obsolete as the older literature in every other science. I regard this as very natural and is of equal distance to all extreme opinions we hear so often.

The preceding discussion builds the basis for a brief description of today's parties in our discipline. We already saw, and will see again soon, that we can trace back almost all directions of our field to the classical authors as their take-off point. It does not matter whether one followed their orbits and tried to substitute what was dismissed, whether adored or attacked—one always started with them. One might be inclined to deny that; each new direction tries to stand on its own feet and denies the impudence within older works; but it still exists. The historical school started from a criticism of classical findings. The classical authors provided its terminology, its systematic representation, and one finds classical thinking in this direction's work—whether it was uttered consciously or subconsciously. And that is, of course, even more true for the new theories.

So, we can see a clear development of our discipline, even if it has not been a straight, constant, and calm one. Like the branches of a river delta, the individual directions came from a common source and are closely related. One often hears that especially the German economists have lost contact with the classical authors: That is not true for the theory; as far as theory is being discussed in Germany, the classical authors are being stressed. But they are also very active outside the pure theory discussions—quietly but profoundly.

The economist who in his "introduction" discusses the different directions of economics, usually differentiates the pure theory—which he, depending on his viewpoint calls "exact," speculative," or "deductive"—then especially history of economics and description of economics and tries to characterize them with some general remarks. That is not sufficient because there are so many differences within the theory that a general judgment can only be made in very general terms. We therefore want to keep the different groups separate.

In our opinion, then, the classic systems are the common birthplace of all the approaches of economists, at least when we consider the pure scientific side of it. And with that we do it justice. Not *one* of its parts can be upheld today but *everybody* has contributed to today's state of our science. But the classical authors still are a vivid power today, more so than in any other science. Quite a few mathematicians have not read Newton or Laplace. That is not possible in our area: Many people even today go back to A. Smith and Ricardo. The reason is that people in our field are not sure what our classical authors can teach us today and how they have to be interpreted, while in other sciences the valuable parts of older works do live on in the new ones. But there is another reason: Wider circles who understand theories very well, nevertheless do not understand the modern system of theory because its scientific framework is much more difficult to grasp. On the other hand, these same people do not have any problem following the classical authors and get more satisfaction out of them because they can get brief answers to burning practical questions. So, both the laymen and the expert economists turn to the classical authors rather than the modern ones. After we now have shown the significance of the classics for the development of our field and, thus, for the present situation, we have to include them among our modern approaches: They are still alive today.

It is a strange phenomenon, the group of researchers that is still based on the classical authors. We do not want to try to judge; not to explain this phenomenon. Of course, the reasons we just discussed are not sufficient because we can find researchers whose *main interest* is in the pure theory. But we have to say that we cannot think of another science in which a group of scientists finds the development of its field of the last forty years foreign. The economists we are thinking about here are, for example, Professor Sumner from the U.S., Professors Nicholson and Cannon in England, Professor Dietzel and others in Germany. In a way, A. Wagner can be included in this list and there are many lesser-known disciples. According to them, the basics of the pure theory that was laid out by the classical authors, are still of use today and should be preferred over the new systems of theory, which is not fully accepted yet due to its newness and questionable value.

On the other side, there is a group of theorists, which we can call "modern." The founders of this direction we already have mentioned: St. Jevons, C. Menger, and L., Walras. They claimed to have put the exact economics on a new footing, which was not a development of the field but rather meant the destruction of the classic system. Indeed,

their work was different in every aspect, in the demarcation of the field, the methodological means, the findings; in all these areas the classical authors stand in total contradiction to the authors of the present. We only want to mention briefly that it in itself is split: “The Austrian School” with its most prominent representatives Menger, v. Boehm-Bawerk, and von Wieser and many non-Austrian scientists – Wicksell, Pantaleoni, Smart, Pierson, for example—has a totally different character than the “American School”—J. B. Clark and his successors. In a still different group are v. Pareto and E. Barone—for a lot of different reasons.

A third group is formed by theorists around A. Marshall. St. Jevons did not have any disciples in England—as famous as he was. You can find his ideas in almost every book on theoretical economics but it does not very often receive praise without a lot of reservations. The theorists of this direction think that his criticism of the classical authors went too far and that his discussions are mere additions to prior knowledge. They think that there is not much of a difference between the two if one interprets the classical authors loyally and does not see brevity as a criminal act. They think that the new value theory is one-sided and in itself insufficient. So, this approach is characterized by a certain eclecticism; one might be of a different opinion but it has a lot support in our discipline. Marshall is the theorist who gets the most attention today, even from people who usually keep their distance from the theory. And there is really no other book from which one could learn as much as from this great work.

We can now contrast these “*theoretical*” directions with other directions. We already mentioned the economic historians. Here we have to think of the interesting phenomenon of the development of new theories based on historical materials. Probably the best example is the “Theory of Modern Capitalism” by W. Sombart. This direction seems to be on a fast upswing and will soon be written about a lot. But we cannot put it next to the “exact theory”; it actually is totally the opposite, both in its nature and its goals: It does not try to build an exact framework but poses hypotheses about concrete questions, hypotheses similar to the ones in the field of political history. They do not strive to be generally accepted but rather are related to single historical facts. There is almost never a connection between the two—extended thought processes do not occur. So, there are certain parallels to the hypotheses in biology, which also deal mostly with problems of development. The biggest difference to our static theory is that these new theories are anything but static. But perhaps theirs is the field of “dynamics”! We will see.

Another group of economists deals with the great problems of today, the development tendencies of the world economy, questions of currency politics, modern monopolies, etc. This approach differs from our theory in two areas: First, their work is predominantly practical; and secondly, it is based on concrete phenomena of reality, on collections of statistical facts. That is of course totally different from basing your work on abstract hypotheses and similar instruments of exact thinking. It also deals with the present instead of the past. Of course, we will have to make the connection between our theory and the findings of this type of work, but for us it is more important to stress that those two have to be kept separate.

Other economists have turned to social policies, some to a point that they are “economists” by name only. That is definitely the case in Germany. But of course it is not up to us to judge this direction. I do not want to give the impression that I do not recognize the validity of this direction by my musings from the standpoint of economic theory. The opposite is the case. But it has to be stressed that economic theory and social policies are totally separate fields, both in theory and content, and that the social politicians’ opinion of the political problems and the theorists’ opinion of social policy problems have to be a dichotomy.

But we find these encroachments a lot. A good example is the majority of French economists who mix different approaches. This direction, especially the academic economists, is the successor of the practical-political inheritance of the classical authors. Theory for them is not an end in itself but a platform for political tendencies. We have to appreciate their contribution to the theory but we do have to stress that we do not have anything in common with them. Among them are many economists who are interested in economic policies—not in theory—although they do not try to do without it.

And finally, we have to mention scientific socialism, which of course has its own theory. Although this theory does not go up or down with its practical postulates, it is still used by many non-socialists. But we have to be careful calling the latter “bourgeois.” The exact theory as it is represented here is not partial and does not lead to any practical postulates. Very often, we find attacks against socialism in many theoretical works on the present distribution of production profit. It actually has to, since its theory proves that free competition leads to “profit maximation.” We do not want to analyze this here. Not because we would be against socialism but because of theoretical reasons that do not have practical meaning.

## 14 The Nature and Essence of Economic Theory

This is all we have to say here. This book is purely theoretical. It tries to dissect as exactly as possible the basis, the methods, and the main findings of pure economics in order to gauge its nature, its value, and its potential for further development. This should be enough for an introduction; more remarks about the relationships of our narrow field will be found further into the book, especially in the [second part](#) and towards the end of the book.

Now we want to start our task and lay out the basics of our pure theory, as dry and plainly as possible in order to be as unassailable as possible.

## 2

# The Point of Departure of Our Theory

### I

We are faced with some problems at the threshold of our discipline, problems that seem to be based in its nature. Every discussion of economics starts out with many undoubtedly highly interesting and highly important problems. The motives for human activities, the fluctuating forces of society, the reasons for economic activities, etc.—scientists think they have to discuss all of this and much more before they can start to tackle the real problems of our science. They force upon us ready-made opinions on topics such as the types of human needs; how they can be explained and then satisfied; and how these different types should be evaluated. And everything that follows in their work seems to rise or fall with their opinions. Mostly they are general axioms, presented in a very authoritarian tone. And we do not care whether they are uttered overtly or can be read between the lines. Even the author who tries to ignore them, cannot do that if he analyzes what he actually says. A furious and confusing discussion is going on, and some scientists totally concentrate on it, to the point that some of our colleagues neither have the time nor the energy for constructive work. What is the driving force of an economy—the individual or “the society”? What guides the human being more—egotistic or altruistic motives? And it does not matter whether the one or the other is correct—are these motives mainly of economic nature or do others play a large, even decisive role, i.e., ambition, the will to dominate, love of country, etc.? Indeed, what is more natural than that these things are so important for economic questions is that they have to be answered before we can move on? One might even think that; that in itself is not even enough. Are the actions of humans simple

and regular enough to be able to describe them scientifically or do they originate in a free will, which would not allow for an exact description? Are their motives based on natural laws, on measurable “powers,” with which the natural sciences deal all the time? So, we are even slipping into the field of free will. But if we really want to tackle these problems, we have to admit that our field is in a grave position. We are forced to include all these things into our field and have to give up any clarity and independence in our discussions. We would give up clarity because the indicated problems exclude clear and precise solutions. Partly, they are in the area of metaphysics and this fact excludes any type of real exactness. Foggy metaphysics would shroud our discussions. And we would give up independence because some of those problems belong into other disciplines, like psychology, physiology, and biology. Since we are just dilettantes in these fields, we need constant help and so we lose our independence.

Indeed, in all these questions we are being attacked by enemies of our science. As a matter of fact, the sheer fact that we deal with these questions explain the mere existence of these enemies. We are embroiled in a quarrel that will not lead to any results and it will only end when everybody involved in it will just get tired. And as long as not everything is on the table, we will not come to an understanding and people will not develop trust in our discipline. But do we really have to wait until mankind has answered all these questions? In this case we would have to give up economics altogether because we will never run out of questions. So the question has to be posed, whether we really have to solve *all* problems, whether we have to blow up all hurdles instead of trying to navigate around them. Other disciplines do it, too. If the field of mechanics wanted to answer the question what “power,” movement,” and “mass” *really* are, the beautiful building that we admire today never would have been built. Wouldn’t it be possible, then, in our field to also tackle problems without having to do all that groundwork that saps all our energy?

When we look over the arsenal of our findings, we observe something that shows us the correct path: The discussions that have provided us with the most valuable findings in our discipline are virtually free of all these controversies. For example, the person who asks what the interest on capital is and what its laws are, does not really care whether economic or artistic interests have greater power over the economic subjects. The same is true for the money theory and one could find many more examples. Questions like this should only play a role in introductions and

very general discussions. In the real work environment, in the practical part of our science, they do not play a role. So, one can assume that they are a lot less important than it sometimes seems. This seems to be a path out of our quandary.

So, let us use this path! We neither want to strive for new ways to solve problems nor do we want to produce ammunition for any of the quarreling parties. I have started on a different path, one that avoids fruitless controversies, a path that does not add anything to the popularity of our field but can be recommended to anybody who is serious about scientific work, and who prefers precision and cognitive correctness instead of unclear phrases and colorful generalities.

It is the following: We look at a group of concrete findings, which are typically called pure economics, and ask ourselves how we can understand them with a minimum of background and axioms; we analyze which underpinnings we really have to know and use; and we refuse to comment on any problem unless it is necessary for our cause. In other words, we analyze which of them are necessary for our theory and, further, what economists want to do with them and what they do when they come up with these overgeneralizations. It is not important to us what these assertions generally mean or whether they are true but rather what they mean to *us* and whether they are useful for each of our cases. It becomes clear right away that the whole thing becomes clearer because it is more limited. In addition, it takes away from the aggressiveness of our quarrels because we do not try to solve the problems in general but rather in very limited cases.

We do not want to embellish the basis of our discussion but word it as boringly as possible. We want to say as little as possible about things that are not really part of our domain. Our introduction should be bland and formal but therefore as clear and correct as possible. It has to be purged of every word that is unnecessary for what follows. The less the reader has to digest the better. We especially will avoid getting too deep into questions that are not part of our domain and to try to *explain* our presuppositions: If we go into philosophical, sociological, physiological, and other underpinnings of certain phenomena, it appears that our discussions are dependent on their level of correctness, and thus could be refuted by philosophers, sociologists, physiologists, etc. I cannot stress enough, that that would be wrong.

We would like to contribute to a critique of our science in order to show what the exact content of the wordy presuppositions is that we meet at the threshold of our science and that we are given when we ask about



the foundation of theoretical economics. We gladly sacrifice the interest in our field for scientific rigor. We put dry assumptions—which might not say anything interesting but there is no doubt about their sense—in the place of the most beautiful philosophies. Everybody can embellish them the way they want. As long as he accepts them we do not care what his reason might be. We will limit ourselves to a very small area because that allows us to control it.

What is, then, the result of this form of proceeding? Does it do what it is supposed to? The reader might want to judge that for himself but we want to mention in advance that we will accomplish what we wanted, in a quite surprising fashion: A whole bunch of controversies simply disappear. If one does not deal with them with general arguments but really analyzes what they really are about, one finds out that there really are no roadblocks but one can easily avoid them. All others—and I really think *all* of them—can be rewritten in such a way that the sticky points do not have to be touched upon at all and can therefore be neutralized. All objections that I know of can be taken into consideration and anybody who appreciates the meaning of the exact discipline of human actions will be ready for a sacrifice that is necessary in every cleansing process.

In our work to purge everything unnecessary or compromising from the main questions of the exact economics, we get to the following statements which seem like bare skeletons—but that makes the delineations of our discipline so much clearer. That might seem strange to the economist—that is the reason for the preceding comments and for the fact that our discussion will be interrupted frequently for further commentaries. But everybody who is interested in the exact sciences will find in them well-known ideas. They will lead straight to the problems with which we will be dealing.

## II

If we look at any economy, we will find that each subject owns a certain quantity of certain goods. At the basis of our discipline lies the realization that all these quantities, which we want to call “economic quantities” are in a state of interdependency, in a way that if one of them gets changed all of them change. That is a simple insight that is so obvious that it does not need any further explanation. We say that those quantities form the elements of a system. Even if they altogether are arbitrary and random, the individual ones in themselves cannot be random and independent.

Now, if we find there is such a connection, that each quantity of one element or a few of them relates to only one other one, we call this system

unequivocally determined. We call this state the State of Equilibrium. The individual quantities in this state we call *normal* or *natural*.

When given any state of an economy, it is our task to deduce those changes of the quantities, which will happen in the near future if nothing unforeseeable happens. We call this deduction "explanation." We get there through a *description* of the interdependencies. We therefore define our task as the *description of our system and its tendencies of movement*. If that can be achieved unequivocally without having to use axioms of our field, then there is a *self-contained economic discipline*. Our descriptions are therefore "*economic laws*," as long as they are of high significance. In their entirety, they comprise the discipline of the "pure" or "theoretical" economy.

### III

Let us halt here to discuss what we just wrote, especially the advantages of our method. Above all, it allows us a precise definition of our topic, free of any type of vagueness. Of course, it can only be fully appreciated at the very end of our discussions. In general, it is a characteristic of a strictly scientific approach that the reader or the listener only learns during the discussion which direction the author is taking and why he chose this specific organizing principle. Although they can be found in the very beginning, the first sentences are always the result of later discussions of what is necessary in preliminary deliberations to understand what follows. Just looking at some definitions shows us what economists in the past did not do but also how inadequate their definitions are. We do not want to talk about those who see economics as the science of the best means to reach economic well-being and, thus, strip our science of its scientific character; we are beyond that. But even the more correct approaches are very unsatisfactory. Some have described economics as the science of the satisfaction of needs. The satisfaction of need is a question of physiology or, looked at from a different angle, a question of engineering or, from yet another viewpoint, a question of cultural history. Such a definition creates all kinds of expectations, which will be disappointed but not those which would be justified. This definition does not indicate at all the nature of the pure theory nor its concrete problems. In addition, it does not delineate the field either. And finally, it infuses a lot of problems and vagueness into our science by using the term "satisfying needs."

The definition of economics as the science of economic activities does not fare any better because that would necessitate a full explanation in

our discipline, which means some information on the nature of human activities (especially the economic one), how economic ideas and habits can be explained, etc. Biology covers these problems. This approach does not do the questions of pure economics justice. It does not fathom economic activities, it actually is not even related to them. Of course, prices are a result of “economic activities.” But the important point is that we deal with them on the basis of certain formal assumptions but not on what they are based and therefore we do not have to even deal with human activities. And when we do not have to, we will not do it on the basis of “scientific economics.” But that will become much clearer later on.

Another definition, the one of the “economic principle” goes too far, because this principle covers a very wide field and it is about the generalization of a logical rule.

While this definition is on the one hand too wide, on the other one it does not contain everything necessary: The economic principle by itself is not enough to solve our problems. We need more building blocks in order to erect the building of our science. At least this interpretation of economics is more correct than any other I know, especially the one that sees economics as a function of the egotism of the individual. This definition also is too wide because one can also act egotistically outside the area of economics. But aside from that, this definition creates a lot of problems for our field, which easily could be avoided because the element that it stresses the most does not even play a role in our problems.

I would like to mention yet another definition: One often calls economics the science of production, distribution, and consumption of goods. But theory does not deal with everything that is entailed in this “production,” for example the technique of production. As far as “consumption” is concerned, we only look at certain elements such as consumption delay, meaning saving the money; in general it is behind the processes we are interested in. And we also do not deal with the distribution problem as thoroughly as necessary but just one side of it. We never say which parts of the three phenomena we are discussing—what is missing is the characteristic element.

All these definitions—perhaps even *all* that have ever been written—fail because they were written *a priori*. Instead of looking at the concrete problems, theorists have always tried to explain the name of their field (here we do not want to deal with definitions that encompass more than the pure science). And this name is “economics” or some such thing. Isn’t it only natural that it is the task of an economic science to

research economic actions? But that is not necessarily the case as other sciences show. For example, the field of psychology does not deal with the nature of the soul; it does not even comment on the question of its existence. So we can say without a problem that economic theory does not have anything to do with the nature of economic activity, that it does not even have to be defined. That is not a paradox at all. And when we then keep in mind that the scientific nomenclature is an inheritance of bygone periods and that the development of the science leads to a specialization of the disciplines and to a shift of their problems, we should not be too surprised when the terminology does not meet today's needs. But it is still a good idea to stick with it even if that might lead to misunderstandings and a skewed perception among people outside of our field.

We looked at the concrete problems of the pure theory and peeled off anything unnecessary and got to the dry but exact definition, which you read above. When evaluating it one has to keep two things in mind: First, we do not want to define the whole field that today is called "national economics" or "political economics" but rather the much smaller field of "pure economics." There are other theories dealing with economic problems which do not belong to this group so that, for terminological reasons, we have to differentiate here between the areas of "theoretical economics" and "economic theory." The latter is much wider than the former. The reason why we pick a group of economic theories instead of their totality is that that group forms a self-contained system. [We use the term "system" in two different ways: as a "scientific system of theorems" and a "system of connected quantities." I hope that does not lead to any confusion.] We definitely do not want to limit the field of economics. We just want to analyze a part of the field in its true form without any impurities.

Secondly, please do not forget that we want to give an exact definition, which only contains those and only those elements that are necessary for the following discussion and which really forms the basis for the following train of thought—but not a popular one. If we wanted to show which material theorems the reader can expect, a different one might have been more useful. We just want to work out the exact content of the known definitions and cannot deny that the beginner might not be able to work with ours.

#### IV

Let us proceed: Certain interdependencies and functional relationships are what we are interested in. The fact that the economic quantities

are in such interdependencies justifies dealing with them separately, as long as they are accurate. The accuracy of a system of quantities is a very important scientific fact. It means that we—as long as certain data is known—have all necessary elements to “understand” the dimensions and movements of the quantities. In this case, a separate, independent discipline about such phenomena is possible and that is what we have to prove. Even if an equation system offers nothing but the proof of a clear-cut interdependency that is worth a lot: That is the basis of a scientific building. When we have proven its existence, we have as the first big result, that the economic quantities are not random numbers but are rather determined ones.

This axiom has often been misunderstood. But controversies about this point have almost always been about socio-political consequences. Nothing seems to be clearer as the fact that economic quantities are *not* unequivocally determined but rather determined by social power relationships, which can be manipulated. From a socio-political standpoint, this axiom has therefore been criticized continuously. Indeed it seems to judge all socio-political endeavor rather harshly. Especially the representatives of the workers always assume—and often rightly so—that there is always a political commentary behind them. Therefore I want to stress here right away, that for example the axiom that income is not determined haphazardly but in a certain sense determined by nature, does not mean anything but that it can be deduced from a certain set of data. We do not judge socio-political attempts to change the existing income distribution since nothing is being said about whether this data can be altered or not. But our sentence also seems to contradict everyday experience: For example, there can be a wage increase without any change in the financial situation of the company or without any other type of change in economic circumstances. We only mention this objection at this point—we will discuss it at a later point. There are other reasons, why one has a problem accepting terms such as “natural,” “regular,” and “normal” in this context. Partly at fault are the economists who only too often abuse these terms but partly it is of non-scientific nature. We do not want to discuss these questions at this point because it will become clear that we can avoid them altogether. What is important to us is to protect the words “natural” and “normal” against the suspicion that we in the end mean something different from what we said previously and push philosophical and political axioms. These terms only relate to the given condition of our systems of quantity of goods, which we do not want to judge. We do not care whether there are normal or abnormal, desirable

or deplorable conditions or whether they all have the same relative justification. We will see that our discussions are totally independent of any concrete condition. We do not want to claim that the economic subject's ownership of goods—or as it is sometimes called, “the distribution of goods in the area of the researched area—could not be different. We also do not claim that the distribution, which is the basis of this theory, is the best one. Certainly a violent event could change it and one cannot claim that that would be bad for an economy; what we do claim, though, is that from a given distribution plus a few more certain data we can deduce another one if nothing unforeseen happens, like a violent intervention. Each economic quantity within a system has a certain given dimension. Now, if an economic subject receives a different amount of a certain good that is abnormal from the perspective of our theory. A different reasoning would have to be found. That does not mean we would not accept such a case nor that we see it as an abnormality or a temporary phenomenon. Perhaps the term “normal” and even more so the word “natural” suggest both; in this case both terms are misleading and we want to stress that we do not want to be associated with the group of economists who say that. For practical reasons we do stick to the terms but do not want to put any different meaning into them; thus we take away their controversial character. But the price for that is high: We relinquish every material assertion and demote this terminology to a harmless but meaningless means of scientific thinking. But in our area we only use it as that. In the following discussions we will see that these controversies, which play such a large role in our literature therefore are utterly superfluous, that our tight definition pulls the rug from under it. We got them out of our path.

Let me comment on the term “balance.” It actually is not a very good term for the situation in which nothing changes as long as there are no outside interventions. The term reminds us that the field of mechanics and analogies with that field are not very popular and actually do have some problems. We again would like to stress that we do not want to draw any conclusions from such an analogy and that we want to stick with the term and we do not want to use it in any other form as the way we have defined it. The basis and the central problem of our theory is what is necessary to determine the balance of our interdependent system. Details will be discussed at a later point.

## V

The description of the interdependency of the elements of our system in order to be able to interpret the different conditions of that system is