Macroeconomic Dynamics, **3**, 1999, 571–601. Printed in the United States of America.

MD INTERVIEW

AN INTERVIEW WITH ELHANAN HELPMAN

Interviewed by Daniel Trefler
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February 1998

Elhanan Helpman is on a quest for knowledge. He is a researcher in transition, constantly redefining his fields of interest by altering the questions asked and the techniques used. Like many of the great economists, Elhanan has worked in many areas: open economy macro, trade under uncertainty, the new international economics, growth theory, and political economy, to list the most famous. However, this interview is not intended as a review of his contributions. Rather, it is about what drives Elhanan's creative process: his wide-ranging reading, his characteristic stubbornness in tackling problems, the conceptualization of his larger research agenda, and his recollections of how he initiated transitions between fields. It is a very personal interview for those in search of creative inspiration.

Keywords: Helpman, International Trade, Growth, Political Economy

Trefler: The boy from Dzalabad, Russia, born in 1946. Tell us about your early education and influences.

Helpman: There isn't much to tell. I studied in a Jewish school in Poland for a number of years, before we left for Israel. In Israel, I completed elementary school and high school.

Trefler: You were how old?

Helpman: Eleven. In high school I learned a profession because I was not sure that I would be able to afford to study in a university, and my tendency was anyway toward engineering. Then, when I was doing my military service, I discovered economics, and I made up my mind to study economics. It was a strikingly simple decision. Suddenly I knew that this is what I really wanted to study.

Trefler: Did something happen in the army that made you think about economics?

Helpman: There was a woman that served with me, and she studied economics. She used to go to evening classes, and she used to carry this thick book by

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 1365-1005/99 $9.50

Figure 1. Elhanan Helpman.

Samuelson. I started to read it, and simply could not stop. At the end of the book I knew that this is what interested me most.

Trefler: And you married her.

Helpman: No, I did not!

Trefler: Your first appointment was at the University of Tel Aviv. I'm curious what it was like when you first got there.

Helpman: When I joined the faculty in 1974, members of the economics department were very young, and I knew most of them. The department started in

https://doi.org/10.1017/S1365100599013061 Published online by Cambridge University Press

1965, and I started to study in it as an undergraduate in 1966. At that time the classes were very small—they had just started to develop the program—and we knew personally our teachers. When I graduated in 1969 I joined the first class of MA students. In fact, I am the first Masters student who graduated there. So when I came back, the department was still very young. I don't think there was anybody there above 40.

Trefler: Wow. Was there an intellectual leader?

Helpman: Well, there were two people who made most of the decisions at the time: Eitan Berglas, who was a public finance expert, and Chaim Ben-Shachar, who was doing finance. And there were also two others: David Pines, an urban economist, and Chaim Lubin. They were the senior faculty members.

Trefler: You wrote with Pines a lot.

Helpman: I wrote with him a number of papers in urban economics. When I arrived, there were also some very young people: Assaf Razin, Yoram Weiss, Elisha Pazner, and David Schmeidler.

Trefler: Quite a remarkable group.

Helpman: Yes indeed. And there were some others who left the university. It was a very young and very active group. People were focused on doing research and debating economics.

Trefler: It sounds like quite a remarkable department for its size. These were a lot of amazing people, in retrospect.

Helpman: It was a very strong group, and getting stronger. When I came back, Efraim Sadka came back a year later. He graduated from MIT in 1974 and went for one year to Wisconsin. We knew each other from college; he too was an undergraduate in Tel Aviv. In the same year, Arye Hillman joined the faculty, but he moved later to Bar-Ilan University.

Trefler: The decision to stay in Tel Aviv rather than an American university, what were the factors?

Helpman: The fact is that it never occurred to me at the time to seek a job outside Israel. I mean, it wasn't something that we even considered. In 1971, I went to study at Harvard. I completed my dissertation in 1974. During those years in the United States, it was obvious to my wife and to me that we would go back to Israel as soon as possible. I didn't even look into the U.S. job market.

METHODOLOGY FOR THEORETICAL WORK

Trefler: Periodically, I hear some very terse methodological statements from you. At least for theory papers, what constitutes a good model?

Helpman: A model is good if it is able to address some interesting questions and to provide nontrivial answers. This is in general terms. Beyond that, it is hard to say. When building models, we have to make very careful choices of what to put in. We always leave out many things. There is the danger that these choices bias the results in certain ways. So, we have to make sure that we have incorporated at least some elements that are judged to be important for the issues we seek to address. I find this stage of my work most painful. It involves delicate decisions that impact in a major way on how successful the subsequent efforts will be. Although it is possible to move back and change these decisions later on, after gaining some experience with the analysis, it is often not easy to do. And in most cases, these initial choices have a strong effect on how one thinks about the problem also in later stages of research.

Trefler: Could you give me an example?

Helpman: Sure. When, in 1982, I started to work on multinational corporations, I spent several months reading the literature. It included academic papers and books, publications of international organizations, business literature, and the like. My purpose was to form an opinion about what features to include in the model in order to derive interesting insights. On the other hand, I wanted to build a theory of multinationals that could be integrated into trade theory, in a way that would shed light on the volume of trade, the share of intraindustry trade, the share of intrafirm trade, and the like.

But it was apparent that if I opted for a complicated model of the multinationals and tried to integrate it into the already complicated trade theory, nothing much would come out of it. So, it took me a long time just to make up my mind on what sort of elements to keep in the model and what to leave out. I was after answers that would hopefully be interesting and that would capture something essential about multinationals. Eventually, the model was quite simple. Yes, it ended up being quite simple, maybe too simple in fact. At the time, however, I felt that it helped me to think about these problems. I felt that it provided some good answers to central questions.

Trefler: I have heard you criticize papers for using overly complicated machinery to make a point that comes more naturally from other models.

Helpman: Yes. I think that this is a general point. Suppose, for example, that we try to explain some phenomenon. And we end up using a complicated model for this purpose. But in order to achieve this aim, we need to rely on a particular configuration of parameters or assume specific processes, because the model is so complicated that it also can yield different results. When this happens, we end up, in fact, with an example of something that can happen. But how likely is this phenomenon, for whose sake we have built the model in the first place? If we cannot argue that this is a very likely outcome, I find it hard to rely on such a model. Especially so if I can think about an alternative framework in which the phenomenon we are trying to explain is almost an inherent feature, and this alternative framework makes sense on other grounds as well. It seems to me that under these circumstances the first line of argument is less appealing than the second line. This was the case I had in mind.

It is not necessarily the case that there is anything wrong with the first type of argument. The results may very well be correct. But I would not choose to use a model of this type when I know that the results that interest me most rely on very special circumstances. And I know at the same time that in another model the results are rather natural.

Trefler: So, an important model evaluation criterion is the reasonableness of the framework, and not just what it's predicting.

Helpman: Yes. And whether the phenomenon is generic to the framework. For example, in the context of international trade there were a number of papers that started by saying: "Let's assume that there is no product differentiation" and proceeded to build a framework of analysis that yielded a result that had been derived in a framework with product differentiation. Moreover, whereas some of these results, such as intraindustry trade, are quite natural in economies with product differentiation, in these alternative models, special assumptions are needed to obtain them. So, one can argue that it is intellectually interesting to see how far one can push a set of assumptions. If this were the only purpose of the analysis, I would accept it. But it is often argued that it is preferable on empirical grounds to stick to models with homogeneous products. And here I disagree, because I see product differentiation everywhere. There is so much evidence of product differentiation that, it appears to me, we rather need to justify the use of models with homogeneous products. My view is that models with homogeneous products were used because they were considered to be simpler. And as long as they did a good job, there was no reason to complicate them. But once they require unreasonable specificity in order to explain a set of phenomena, while models with product differentiation explain them rather naturally, I see no reason to stick to the assumption of homogeneous products. Therefore, I see no point in trying to develop theories that insist on homogeneous products in explaining such phenomena, just in order to be able to say at the end of the day, "Well you see, I can produce similar results even if there is no product differentiation." Because after all, we know that there is product differentiation and we know how to explain these phenomena very simply with product differentiation.

Trefler: Does this conflict with Avinash Dixit's comment to me that a good model is a model that can simultaneously explain many different phenomena? In that comment, Dixit seems less concerned with realism of the modeling framework and more concerned about the number of facts that can be simultaneously explained.

Helpman: I don't see that it conflicts. When we want to focus on a particular phenomenon, we try to devise a system that incorporates elements that seem to be particularly relevant to this phenomenon and then show how to explain it. But obviously, we do it only because we think that by using a more complicated framework we shall not be able to explain the desired link between cause and consequence as clearly as with the simple framework; things will be just much too complicated for the purpose at hand. But if we develop a framework or a model to explain a particular phenomenon, we always hope that it will also be able to explain other relevant phenomena, perhaps with minor modifications.

Most models that we have predict a variety of phenomena, and we hope that all of them are consistent with what we observe. When some predictions are not consistent, we worry about them. We try to understand the sources of inconsistency and to find ways in which to modify the assumptions, the framework of analysis, in order to get rid of these inconsistencies.

Trefler: Is there one paper that you are particularly proud of, judged on these terms, that you would recommend say to graduate students—how to do theoretical modeling?

Helpman: Every paper I wrote appears to me now as something I could have done better. Nevertheless, I like some of them better than others. I like very much my 1981 paper on product differentiation and international trade. I also like the 1984 paper on multinational corporations, the 1991 paper on quality ladders and growth, and the 1994 paper on protection for sale, which is very different from the others. I also like some of my papers that are not known as widely as those I have already mentioned.

For example, I have a number of papers with Alan Drazen on inflation and stabilization policies that I like very much. One paper in particular, published in 1990, grew out of a public debate in Israel concerning the causes of inflation in the early 1980s. There was, at the time, a group of economists who argued that inflation in Israel was not driven by budget deficits and that it was just a bubble. As a result, they argued, all that was needed was to burst the bubble. You did not need to do anything drastic, like asking the government to reduce the budget deficit from 12% of GDP to a manageable level. The evidence brought to bear on this point was a lack of correlation between the rate of inflation and the budget deficit. There was indeed little correlation between them in the Israeli data. Others, me included, argued that it would be impossible to reduce inflation from several hundred percent per annum without reducing the huge budget deficit.

This debate started me off on the project with Alan. We produced a paper that I like. In the paper, we built a model of inflation, with a constant budget deficit, in which the deficit is not sustainable in the long run. As a result, people expect that eventually there will be some stabilization effort. The government can close the budget deficit by cutting spending, raising taxes, or doing something else. What we showed was that, uncertainty about what policies the government will use to end this process, or the timing of these policies, generates an inflationary process that is not correlated with the budget deficit.

METHODOLOGY FOR EMPIRICAL WORK

Trefler: Let me turn to methodology for empirical work, if I may. This is close to my heart.

Helpman: This is close to *your* heart, not necessarily the strongest part of *my* work.

Trefler: You read very widely—a part that I want to come back to, but you know a lot about empirical work, even if you don't do that much yourself. What is good empirical work?

Helpman: There are many different types of empirical work that I find useful, and they are very different from each other. Some of the descriptive work is a case in point, for example, a description of what happened in a country, with data gathered in a revealing way in tables and graphs. In fact, I wish there were more publications of this sort. Just telling an intelligent story with numbers.

https://doi.org/10.1017/S1365100599013061 Published online by Cambridge University Press

Trefler: One of the questions I had at this Canadian Institute for Advanced Research here was, after hearing Clyde Hertzman and Janet Curie each present papers—one very reduced-form, one very structural—it struck me that we spend too much time on structural estimation, only to find that it really isn't all that structural. I was thinking that we needed a *Journal of Simple Correlations*.

Helpman: Yes, I would like to see a journal of simple descriptive articles. Correlations are part of it. When we see a correlation, we often do not know what is causing what. But just to know that there is a comovement can be very important. So, there are things of this sort that I like to read. I like the descriptive material in publications of international organizations, such as the IMF, the World Bank, UNCTAD, or the WTO. They contain very interesting information, even when there is little economic analysis of the data.

Trefler: I agree 100%.

Helpman: Then there are papers of an empirical nature that I find interesting. Some report estimates of various parameters. This type of work goes beyond data description. It provides a number, such as an elasticity of substitution, whether there is complementarity or substitutability, for example. Just to know a fact like this is sometimes important. We may want to know how accurate and robust the estimate is. Whether it is the same across different data sets. Take, for example, the interest semielasticity of money demand. People have estimated it for many data sets. When the various estimates are close to each other, we gain confidence that the number is right. This sort of confidence is valuable because, even when we think about theory, it often helps to have a number in mind in order to form a judgment as to what is reasonable. There is plenty of work of this nature that is useful. When it is done well, I appreciate the effort.

And then there are papers that test hypotheses. Is there inflationary inertia? Is the Phillips curve vertical? In many cases, the hypotheses are interesting and I would like to know the answer. Sometimes the formulation of the test is very ingenious and this is interesting in its own right.

Finally, there is structural estimation. This type of work is often very different from the others. Some of the studies in this tradition are very complicated, like the work of Berry, Levinsohn, and Pakes. But I enjoy this line of work too, especially when it is done well. After all, we should strive to build more accurate models and this type of work helps to achieve this end.

What I don't like is sloppy work. One can produce a careful estimate of a parameter. One can produce a careful test of a hypothesis. One can construct a careful estimate of a structural model. And if the question is interesting—and there are many interesting questions—then in each one of these instances we learn something useful.

I also hold the view, which applies to empirical and theoretical economics alike, that people should be more open-minded toward what they consider to be reasonable approaches to a problem. After all, we don't have the ultimate truths on any major issue. The phenomena we are dealing with are so complex, and our tools so limited, that it is hard to believe that one single approach can provide all the right answers. So, I think that it is only reasonable to try different approaches, different ways of dealing with the problems, and learn from each one as much as we can. This point applies with equal force to estimation methods and theoretical constructs. As long as the analysis is done well, we learn something useful from each such attempt, and it helps us also to sort out what is a more reasonable approach. I don't believe that there exists at the moment a clear single superior way of doing economics.

Trefler: Has there been a debate in international trade that was more methodological in nature that you thought was more smoke than light?

Helpman: I do not recall big professional debates in international trade, such as the switching-of-techniques debate in capital theory, but there were smaller debates. For example (and I am, of course, biased), when the first models of product differentiation came out, there were some people, such as John Chipman, who argued that there is no point in doing this work. Because intraindustry trade is a statistical artifact, we find intraindustry trade because the degree of disaggregation is too low. I did not agree. Chipman's argument was, of course, true to some extent, but product differentiation is so prevalent that it could not have been only a matter of aggregation. This is true notwithstanding the fact that the calculated shares of intraindustry trade fall as the level of disaggregation rises, but I did not see much use for a regression of the index of intraindustry trade on the level of disaggregation. The conclusion from a regression of this sort, that intraindustry trade is expected to be zero in a calculation that uses a 20-digit disaggregation level, is not informative. Moreover, it is misleading. I found this argument to be rather silly. In any case, this debate was not of major importance.

Trefler: Growth theory sounds more prone to methodological debates.

Helpman: I'm not sure even about growth. I mean, in growth, there are people who believe that certain things are important, and they believe that some other things are not important. Or they believe that certain things behave in a particular way. But I do not see big methodological debates even in this area. It is more a contest of beliefs than a debate.

Take, for example, someone like Mankiw who says: "Solow's model is enough. Why? Because in a sample of countries we can explain a large fraction of the cross-sectional variance in the growth rate with variables suggested by Solow. For this reason we should not be interested in endogenous technological change, or spend much effort in studying how R&D affects productivity." Others, like Romer or Grossman and myself, say: "It is important to understand what drives technological change and this should be thought of as part of the growth process." This does not look to me like a major debate, because I cannot understand why someone would choose to give up the opportunity to understand the sources of technological change, how they are affected by the economy, and how they affect the economy. There is so much evidence, historical and otherwise, that technological change has played a major role in raising the standard of living across the globe that it is hard for me to see why an economist would suggest that it should not be studied as an economic phenomenon. For suppose that you explain 80% of the variance in the growth rate for a cross section of countries. Does it mean that the remaining 20% is not important? Twenty percent is a large number. Many econometric studies claim victory when they explain a smaller fraction of the variation. And what if this 20% contains the margin that is easiest to exploit? That yields the highest social rate of return? There are many reasons not to give up on such a margin.

THE HELPMAN "EMPIRICAL TOUCH"

Trefler: There's a very interesting "Helpman touch" in empirical work. I'm thinking of your empirical papers on estimating variety models—the 14-countries paper (1987)—and your more recent work with Coe (1995). These papers have become phenomena of their own. They're probably some of the most widely cited empirical papers in international trade.

Helpman: I was surprised that they received so much attention.

Trefler: What's going on?

Helpman: I don't know. The histories of both these papers are somewhat odd. I did the theoretical work that was used in the first paper after returning to Tel Aviv from Rochester. I was on leave in Rochester in 1977–1979. This was my first sabbatical. In 1979, I went back to Tel Aviv. Already in Rochester I was thinking about expanding trade theory to take account of intraindustry trade. When I completed the theoretical paper, which was published in 1981 (interestingly, Chipman was the editor who handled the paper for the JIE, and he, in fact, asked me to submit it), I was very curious to see how its predictions would compare to the empirical evidence. I read, as usual in such circumstances, the literature, trying to find suitable evidence. There were some pieces that I found useful at the time, but nothing particularly convincing. One of the problems was that much of the work in this area was not rooted in theory. There were, for example, papers that ran regressions of the share of intraindustry trade on a variety of variables, but they were very unstructured. I was interested in linking the theoretical developments to empirical evidence. So, I decided to do this empirical study myself. I knew that this sort of work was not my comparative advantage. I did know econometrics because I was a statistics major in college, and I did quite a bit of econometrics before I went to study at Harvard, and I learned some more at Harvard. We used to write programs in FORTRAN for estimation purposes and I worked with Eitan Berglas, as his assistant, on a macroeconomic model of the Israeli economy. So I knew enough to do this type of work, although I was not particularly eager to get involved in it, because I was not confident that I could do it well. After much soul searching, and as I saw no one else doing it, I eventually decided to go ahead and do it.

It turned out to be a very painful project. To begin with, there were no suitable data. Then I discovered a data set in the OECD. Remember that we did not have PCs then. We worked with mainframes. The OECD wanted a lot of money for the data and I did not have the funds. I tried to get the data through DRI, but they did not want to give it to me. It is a long story. In the end, I happened to talk about it to my Swedish friend Lars Svensson, who suggested doing the research through the Institute for International Economics in Stockholm, which is his home institution.

I was grateful. Lars found the funding for the data and for a research assistant and I went ahead with the project. It took a long time to complete. First, we needed to acquire the data. Then I worked with an assistant who was in Stockholm while I was in Tel Aviv and later in Boston. The assistant would run a regression and send me a letter with the results. A few weeks later I would write him back, telling him to do something else, and so on, repeatedly. It is hard to think today that this is how we worked once. In the end I wrote the paper.

Trefler: I think you've answered my original question about the "Helpman touch." You choose questions that you think are very important and haven't been answered. So in some sense you're trying to spearhead a new field. That's what creates interest.

Helpman: The fact is that the degree of curiosity needed to induce me to start a research project is larger for empirical work than for theoretical work. And as much as I like to know facts and understand real-world phenomena, I feel that my comparative advantage is in applied theory. For these reasons, I much prefer to have a co-author who is well versed in empirical methods, when doing applied research. You mentioned the project on international productivity. It has an interesting history, too.

When Gene Grossman and I completed our book on trade and growth, it became apparent from the theory that it makes a great deal of difference whether there are international knowledge spillovers, and, if they exist, how large they are and how fast they spread. Our analysis suggested, for example, that these sorts of international links are key in determining whether countries converge or diverge over time in terms of sectoral structures and productivity. Gene and I discussed the need to estimate this and we tried to figure out how it could be done, but the obstacles looked formidable at the time. I tried to find out what we knew about this issue by reading various publications and by talking to people, but as time went by, I became convinced that we do not know nearly enough. One purpose of my discussion with various experts in productivity analysis, such as Zvi Griliches and Dale Jorgenson, was to interest them in this topic. Frankly speaking, I came out quite discouraged from these conversations. I got the sense that the chances of obtaining reliable estimates were very slim.

Then, one day, after a talk I gave in the research department of the IMF, which I was visiting for the summer, David Coe expressed an interest in the subject. David and I had known each other for a while. He had studied productivity in France and I knew him to be a first-rate researcher. And what was very important, he was very enthusiastic and had faith in the enterprise. So, we decided to go ahead with the project.

Trefler: A lot of data went into that, and a lot of data cleaning.

Helpman: Yes indeed. The data were not readily available at the time, but David, who worked in the OECD in the past, was able to get it. It took a long time to prepare the data, to check its reliability and to fill in missing cells. The IMF's research department was very good about it. David had access to research assistants who were very professional in handling data, and this helped a lot. Well,

https://doi.org/10.1017/S1365100599013061 Published online by Cambridge University Press

you yourself obviously know how much effort is involved in this type of work. Then we needed to decide how to estimate these effects so as to have confidence in the findings. We proceeded to estimate the international spillovers using different approaches and specifications. All this took a long time, and only after we were convinced that the answers were not too sensitive to specification and estimation method, we wrote up the paper.

After the circulation of the first draft, we received many comments, in response to which we checked the sensitivity of these estimates. At the end, we remained convinced that there were significant spillovers. We then decided to examine less developed countries and to see how much they gain from R&D performed by the industrial countries. This too entailed the construction of a new data set. Alex Hoffmaister came aboard this project and the three of us—Alex, David, and me—ended up working together. It was a major effort, but it was a pleasure to work with them. In retrospect, it is clear that I was lucky to find such knowledgeable and reliable collaborators; I would not have been able to do this work without them.

Trefler: What hasn't been done empirically that should have been done?

Helpman: The nature and quality of empirical work is often related to available theory, and most of the recent interesting empirical work in international trade is intertwined with theoretical developments. At the beginning there were a lot of empirical studies that were too rough, basically searching for correlations. Then a new line of work developed, which I attribute to Ed Leamer, which linked up much better with theory. Leontief should also be credited for this development to some extent, although the systematic way of building data sets and examining them in a well-defined theoretical framework starts with Leamer's 1984 book. This was an exciting development that breathed new life into the field. Not only did it produce new insights, it also triggered a large number of new studies, yours included, which have been methodologically much more satisfying. We learned a lot from this line of research, which also produced an active interaction between the empirical side and the theoretical side of the field.

Nevertheless, this work has also some drawbacks. It has, for example, concentrated too much on broad levels of aggregation, for both industries and factors. We have neglected, to a large extent, within-industry heterogeneity, such as the size distribution of firms. Is the level of concentration important? How does it impact on trade? Can finer definitions of factor inputs help in explaining some of the problematic features of trade patterns? Some of the microstudies look at export performance of individual firms. Can this view be incorporated into the existing general equilibrium trade models? Are the intersectoral relationships important for exports of individual firms? I would like to see more work on the role of heterogeneity.

Trefler: I'm surprised to hear you say that. In large part, it's exactly my thinking and frustration with my own work. The big payoffs will go to people like Andy Bernard.

Helpman: Yes. We need more work along these lines, and we need to rework the theory in order to help to better formulate the empirical studies.

Trefler: It's critical. Critical. This is an area that has hampered me empirically. So, if you have some solutions which you think are natural in a trade context ... .

Helpman: I don't have solutions. We have limited time and we make choices, all the time, about what to work on. My experience is that I can work only on a small fraction of the problems I consider to be interesting and important.

Another area that needs more work, and at this stage especially on the theoretical side, is the link between trade and foreign direct investment. When you look at what has happened over the years, it is not possible to fail to see how important direct investment has become. Multinational companies are not only large foreign investors, they also play a major direct role in foreign trade. I started to wonder whether we don't miss essential parts of the story of foreign trade by just focusing on trade issues and abstracting from the activities of multinational corporations. The most important empirical studies of trade in over a decade have relied on trade models without multinationals. I feel that this shortcoming needs to be addressed.

THE MAKING OF RESEARCH (GROWTH THEORY, FOR EXAMPLE)

Trefler: As I was reading through your CV, I couldn't help but laugh.

Helpman: Okay, let me laugh too. What were you laughing about?

Trefler: You have more publications in the best journals than I think the entire Department of Economics at the University of Toronto. And I guess ... .

Helpman: This is, of course, not true.

Trefler: But it seems awfully close, and I was thinking that my laughter must be some sort of self-defense mechanism, that I'm envious. I'm just curious about this. How prolific you are.

Helpman: Frankly speaking, I don't think that it matters how much a person writes. What matters is how many important contributions one has made, and there is no necessary relationship between the length of the publication list and the importance of contributions. Some of the giants of our profession attained this status with rather short publication lists.

I live with a constant sense of urgency, that I do not spend enough time on my work. Nevertheless, for some time now, I have not been concerned with how much I write. I did care about it in the past, but not now.

Already as a student at Harvard, I had started to write. There were at the time requirements to write a term paper for every area course. So when I took the trade course from Houthakker during my first year, I wrote a paper. We also wrote papers for seminars, such as Jerry Green's Theory seminar. This was a very good experience for me. I am not a natural writer. It takes me a long time to write and it is quite painful. So, the term papers at Harvard provided a very helpful experience, especially for people like myself for whom English is not the primary language. It also gave me an opportunity to write with some very good people, from whom I learned a lot.

Trefler: Anybody in particular?

Helpman: I wrote a paper with George Feiger, who was a student in my class. He works now for McKinsey, after teaching at Stanford for several years. He wrote beautifully. I also wrote a paper with Bob Cooter, who is a Professor of Law and Economics in Berkeley. Bob was different from George, and I learned from him different things. Then I wrote a paper with Jean-Jacques Laffont, who is a professor in Toulouse, France. He is an outstanding theorist from whom I learned how to write theoretical arguments. So I had many good teachers, including my professors who commented on the papers: Hank Houthakker, Ken Arrow, Richard Caves, Jerry Green, and Richard Musgrave. They were all extremely helpful.

When I went back to Tel Aviv, I already had writing experience. This was very valuable, but I felt that I had not completed my education, that I really did not know enough. Although I spent a lot of time at Harvard attending courses in different fields, I felt that the best way to learn a subject was by trying to write on it a paper. Therefore, for some time, I deliberately worked on different topics, writing papers, as part of an attempt to broaden my understanding of economics. This is how I came to write papers on public economics, urban economics, economic theory, exchange rates, and the like.

Trefler: That's extraordinary.

Helpman: This explains to some extent why I wrote so many papers in those days, and on different topics. It also became a challenge to publish them. It is, of course, possible to write papers and not publish them, which I did occasionally. But the temptation to publish was huge; it was a confidence-building measure more than anything else. Later, I just kept working, project after project. I was hard driven and I enjoyed working immensely. I could work for weeks with little sleep. In fact, I often could not sleep, because I would wake up thinking about a solution to a problem that haunted me. At the same time, we started a family and the evenings were not available for work. It was very exciting, and it is registered in my memory as a wonderful period.

Trefler: How do you deal with your research disappointments?

Helpman: The hard way. It is unavoidable to encounter problems that cannot be satisfactorily resolved. When all efforts fail, I try to leave them for a while and come back to them at a later stage. It is very difficult to do, but it is sometimes the only way to keep going.

One of my typical responses to situations like this is to read as much as I can on related topics. Often, it is frantic reading. This helps occasionally. I mean, it is always pleasant to read and learn something new, but my experience is that, when I think about a problem, I often cannot "get it out of my head." Without a resolution in sight, this can be extremely frustrating and I find it very difficult to focus on something else. So the reading is like a mental therapy. This way I do not get rid of the problem, but the situation becomes less painful. While reading, I am very alert to the relationship between the reading material and the unresolved problem. In what ways does the reading help me to think about it? Is there a useful hint somewhere? Can I see a new twist that changes my perception of the problem? My ways of thinking about it?

https://doi.org/10.1017/S1365100599013061 Published online by Cambridge University Press

And occasionally something happens, not necessarily because of a specific passage or argument in a paper or book, but rather through a process I do not understand. The interplay of reading and thinking produces a "click" and, magically, various pieces fall into place.

Trefler: It's an amazing response. Many people find destructive responses to disappointment.

Helpman: Well, this response provides no guarantees. It works for me because it calms me down and from time to time it also produces results, but it also happens that a long period of reading leads nowhere. Then I put away the topic, which means that I will pick it up again later, sometime after several years.

For example, when I finished the 1981 paper on product differentiation and trade, I started to think about trade dynamics. I tried to develop a dynamic trade model that would trace out shifts in comparative advantage over time. I wrote a bunch of models, but they turned out to be too complicated and I could not characterize their dynamic trajectories. It was a very frustrating experience.

I was still thinking about these models when I was writing the first book with Paul (Krugman), in 1983–1984. When I visited Harvard in 1982, I tried to convince Lars Svensson, who was in Cambridge at the time, to think with me about these problem. He had experience in dynamic general equilibrium theory, but he was occupied with his work on exchange rates and nothing came out of these talks.

Trefler: He has gems that have gone unnoticed.

Helpman: Lars is a very serious scholar and his contributions are now widely recognized. In 1982, we were both young. In any case, I made a number of attempts to tackle this problem, with no results.

A number of years later, in the fall of 1986, Gene Grossman visited Tel Aviv. He was interested in this problem too. We talked about it and decided to combine efforts. We spent considerable time struggling with it, but it remained as stubborn as before. The only difference was that I enjoyed working with Gene and therefore our collaboration lessened the frustration.

Trefler: I had to read some of those papers a number of times. I had no idea what was going on. They were subtle.

Helpman: This was a tough project. The first paper we wrote was published in 1989. It describes a dynamic model of trade and growth, but growth peters out because there is no accumulation of knowledge beyond the blueprints of new products. It is, in a way, a straightforward extension of a static model of trade with differentiated products, to which we added an R&D activity. In our model, firms have to hire workers to invent new products and they cover these R&D costs with future profits. We worked on this paper in Tel Aviv and later in Cambridge; I visited MIT in 1987–1988. We were able to characterize the evolution of trade in this model after many failed efforts, and we had to make compromises, such as assuming fixed production coefficients. But when we finished working on it, we felt that we had accumulated enough experience to move on to a more ambitious project: the link between trade and long-run growth.

Trefler: Why growth?

Helpman: We felt that there was little understanding of the relationship between trade and growth and we saw an opportunity to develop a framework that would clarify this important link. If one is only interested in the evolution of the pattern of trade over time, then much insight can be gained with models that do not exhibit long-run growth. But, for the study of the relationship between trade and growth, this option is not a good one. The reason is that the alternative is to focus on transitional dynamics, which is, in a sense, more relevant. Unfortunately, it seemed hopeless to develop a good characterization of transitional dynamics in a model with two trading countries that invest in R&D in order to develop new products. We had two reasons for choosing models with long-run growth: (1) we wanted to study the relationship between trade and growth; and (2) our experience suggested that the characterization of transitional dynamics would be much too complicated for this purpose. In our 1989 paper, we worked out transitional dynamics for a special case, which was painful enough, and so, we moved on to develop models that exhibit long-run growth.

Trefler: I've often been frustrated with the analysis on the balanced growth path.

Helpman: It would be much nicer and more satisfying to have results on the link between trade and growth out of steady state, but the choice we had at the time was to either compromise and focus on steady states or give up on the analysis altogether. We chose to compromise. We did introduce a little out-of-steady-state dynamics in our 1990 paper, and a little more in our 1991 book, but most of what we did focused on steady states, for technical reasons. My sense is that our results are not that much off mark compared to out-of-steady-state results.

Trefler: What do you see as your strengths and weaknesses?

Helpman: My biggest strength is that I am a big "akshn" ("stubborn" in Yiddish). What helped me a lot over the years is that I can become interested in a problem so intensely that it preoccupies me almost completely, for long periods of time. In such situations one gains tremendous strength and concentration. It helps not to be deterred by difficulties, including routine work such as calculations of various sorts. If you have to calculate for hours a day, you just do it. It is not a major hindrance as long as you maintain a high level of interest in the problem.

Still, sometimes I wondered in retrospect whether I did the right choices, the right cost-benefit analysis. It is hard to be completely rational when the emotions are so high, and without these emotions there is little fun left. How else can we spend endless hours on pieces of analysis that end up in the garbage can? So, the intensity of interest and the personal commitment are important for good research. They may be not so good for other things, such as family life, but they help to deal with difficult research topics.

Trefler: How about weaknesses?

Helpman: One of my major weaknesses is that I am slow. It takes me a long time to achieve satisfactory results.

Trefler: I know that feeling. This morning, I finally finished something that is not going to be more than 10 lines long, something that I was working on for a month.

Helpman: I am not surprised. It takes sometimes a day to draft a footnote. I have noticed recently that many students do not have patience for this sort of work. I don't know if this was always the case or if it is a recent phenomenon. When I propose they do some work in order to improve an argument, to relate it to an existing argument in the literature, or to make the references more complete, I have the sense that they are saying to themselves: "This will take too long. Why should I do it? It is only one paragraph or a footnote." I feel that this is the wrong attitude to research, because if you really want to do research, you should be ready to do it right. And to do it right means that you have to be clear on every point, you have to know what was known before and what is your value added. Sometimes it takes an hour, sometimes a month, and sometimes it takes a year. But you still have to do it, because if you do research without paying attention to details, you end up writing silly papers, getting wrong answers, or you do not pay enough attention to whether your answers are sensible.

Trefler: There are some themes that are appearing frequently, themes that I share but I sometimes think are my biggest problem. I put in an enormous effort trying to make sure that the data are as good as I can make them, recognizing that they will never be good, but I get absolutely no kudos for it. Nobody cares because nobody can assess it. Nobody can look and say that this was better than what other people have done. And I recommend to my students, "Don't bother. This is my obsession. But don't let it become infectious. You do what everybody else is doing. Don't get caught up in my overly meticulous style."

Helpman: Why? I think we should do the best we can.

Trefler: Yeah, but it doesn't give you the right cost-benefit, I don't think.

Helpman: I'm not sure. You wouldn't feel comfortable writing up a paper when you are not convinced that the data you have is the best you can have, or that you have estimated it in the right way. It is easiest to convince others when you feel comfortable with what you have done. And there is the reputation effect. When you do a good job time and again, this is appreciated, including the finer points you describe. They trust your data. They trust your results. They trust your evaluation, your judgment. I think this is how it works.

I knew a guy whom somebody told once: "I found a mistake in your paper." And he replied: "This paper was already accepted for publication. So let them publish it. After publication you can write a comment and I will write a reply." This is appalling. A person that does not care about the content of his writings, just their visibility, cannot do good research. We are not involved in a game. Research and publications are serious stuff. This sort of attitude is a reflection of something rooted in one's personality. Some people have the right personality for this work, which is very lonely and demanding. Others do not.

Trefler: Makes the wife unhappy.

Helpman: It often does, and the reward usually arrives years later. Therefore you need to have an internal mechanism for self-satisfaction. You need to enjoy the doing of research, not only the results. Most people do not have this personality.

https://doi.org/10.1017/S1365100599013061 Published online by Cambridge University Press

Trefler: It's a topic that we could talk about for a long time, I think. A very long time. I think it is central to academic research.

Helpman: Yes. It applies to areas in which people work alone. It is probably different in areas where people work in teams. In a team the others can reinforce every member. We, however, work alone most of the time. Even when we have coauthors, we do not spend with them an enormous amount of time.

ON READING

Trefler: You read an enormous amount. Any comments for graduate students?

Helpman: They should read as much as they can. I think that everybody should read as much as they can. Many economists do not read enough. I find out too often that people do not know what they need to know, even about things that are related to their own work.

My own reading is driven to some extent by a sense of inadequacy, a strong feeling that I don't know enough. It started during my student days and it has remained. So I do not know if this is a positive reason for reading, but this feeling has driven me for a long time. And there is, of course, the genuine sense of curiosity. At some point I decided to devote one day a week to reading. In Israel it was easiest to choose Friday. So, every Friday I used to spend reading the journals, papers that I prepared during the week, and books. This is not to say that I didn't read on other days, but Friday was all for reading. It became a routine that lasted for many years.

I made sure to educate myself in a number of fields, which was easier before the explosion of publications. I did not feel satisfied being up to date only in international trade. Broader reading was also helpful for my own research. After all, we need some broader perspective on what we are doing. Even just to know how our work relates to other fields is satisfying. But we can learn a lot from the ways people in other fields think about problems. This is helpful for our own work.

TRANSITIONS

Trefler: You've gone through so many different fields: public economics, location theory, open economy macro, international finance, exchange rates, uncertainty, the new international economics, growth, political economy, and now you're doing detailed work on Innovation. How did these transitions come about? What happens at these junctures?

Helpman: There is some random element in these decisions. Most of the time it was simply because I became interested in a particular problem. There are different triggers. I told you, for example, about how the debate about inflation in Israel started me on a project that lasted for several years. In other cases the trigger was different. The main reason I worked on urban economics, for example, was David Pines, who is a member of my department in Tel Aviv. (He also was my first teacher of economics.)

When I joined the faculty in Tel Aviv I did not sense any obstacles to switching fields. In some North American schools, it may have been different, but in Tel Aviv

https://doi.org/10.1017/S1365100599013061 Published online by Cambridge University Press

it was quite acceptable. To begin with, we talked a lot to each other. I do not mean David and me only. It was a small group, and we all talked a lot to each other. Not only did we eat lunch together everyday, but if somebody had a problem, it was quite natural to knock on somebody's door, walk in, and start talking to them about it. People felt comfortable to ask for help. Or if somebody proved a nice result, it would be quite natural for him to go around and tell his friends about it. There was a very congenial environment and it was very easy to interact with other members of the department.

So it was quite natural that when David came to talk to me about problems in urban economics, we ended up working together. This was also in line with my efforts to educate myself. I interacted with a number of other members of the faculty on different topics. But during all those years, while working on various topics, I maintained an interest in two broad research agendas. First was international trade. I have had an interest in the subject since I was an undergraduate student. I had this vision of building an elaborate trade theory that would be rich in its descriptive power and useful for policy analysis, but I got distracted time and again from this line of work. My other interest was in macro. In Israel, you could not think about macro without thinking about open economy macro. So for us, macro was open economy macro. We always thought in these terms. Investment was linked to the balance of payments, and so was consumption. And everything was linked to the exchange rate. So I had this desire to develop a macroeconomic theory for open economies that would neatly link to microeconomics.

I started to do some work in this area, both on balance-of-payments problems and on exchange rates. I worked on them for a number of years, and then, I had this idea—that open economy macro and trade had to be integrated, that it was silly to think independently of trade and macro. My initial work with Assaf Razin on trade under uncertainty was designed to lead to this sort of integration. The idea was to think about what linked trade in assets and trade in commodities. The hope was that, at a later stage, we would be able to integrate money into this system, but when we started to work on monetary problems, we started by disregarding the links between trade in assets and trade in commodities. We tried to compare different exchange rate regimes and to develop welfare evaluations. All this was part of a broader effort to integrate micro and macro. I had this vision that eventually everything would come together, which has not happened. Quite disappointing.

Trefler: It's a large number of topics.

Helpman: Well, there was a central theme to what I tried to achieve, even if things did not seem to fit in together. In the process I became also interested in additional topics, which led to diversions.

Trefler: Still on this topic of transitions, it sounds as if you actually sit down and think "where is the field going? What needs to be done?" Rather than, "here is a small problem. Let's try to work on it."

Helpman: I did both; they are complementary to each other. I like to always have a project that is really long term, with a remote goal in mind. A goal like this will most likely never be achieved. At the same time I would pick something more concrete to work on, aiming at getting closer to this unachievable goal. And when the going gets rough, hop to a diversion, a well-defined problem in which you are interested. Some of the latter problems pop out of the reading. When you read, you are often not satisfied with something. It looks quite interesting to see how to do it in a different way, or a way you think more suitable. Then you just go and do it. On other occasions, it is just whims. Something gets stuck in your mind. You see that you cannot get rid of it. It is then better to sit down and work it out and proceed to the main topic. In any case, I think that it is helpful to have both—a long-term goal and less ambitious problems to work on from time to time to keep you going when progress is slow on the main research project.

THE NEW INTERNATIONAL ECONOMICS

Trefler: Which do you consider your most important projects?

Helpman: I am not sure that I know how to carve them up in retrospect. Probably the trade project; this one has been very long.

Trefler: In looking at your CV, the thing that struck me the most was in the period of the late 1970s and early 1980s when there's this massive amount of work being done, and here you are, one of the most prolific people I've ever met, yet you wrote very few papers on the topic. In fact, the 1981 paper—which is a landmark paper—is your first paper on the topic.

Helpman: I would not say that there was massive work on this subject in the late seventies; Krugman and Lancaster were the exceptions rather than the rule. I was doing trade under uncertainty for a number of years. Assaf and I published the book in 1978, and I continued to work on macro topics as well. Most of my time was then devoted to the study of exchange rate regimes.

I started to work on trade with monopolistic competition in 1979, at the end of my stay in Rochester, but most of my time was occupied with macro problems. The integration of micro and macro elements was still on my agenda, and I was obsessed with exchange rate issues, especially with the question: "What is a good exchange rate regime?" I am afraid that even now we do not have a satisfactory answer to this question. In Rochester I wrote a paper that compared fixed with flexible exchange rate regimes for economies with cash-in-advance constraints (published in 1981). This paper took a lot of time to work out, and I waited to finish it first, before switching to trade.

Trefler: In your own mind, how original was the landmark 1981 paper?

Helpman: To me, it was quite original. When I started to work on it, I did not know about Krugman's work. It is my fault because I did not pay enough attention to his 1979 paper, which I did not view as a proper trade paper.

Trefler: My students have trouble understanding that this is a trade paper.

Helpman: Lancaster's book *Variety, Equity, and Efficiency* was also published in 1979. I read it immediately because I was eager to learn more about monopolistic competition. In Chapter 10, he discussed the implications of his theory of monopolistic competition for intraindustry trade, but like Krugman, he too used a one-sector model, so there was no intersectoral trade. For this reason I did not pay enough attention to his contribution as well. I was familiar with Balassa's story of why there is intraindustry trade, and my sense was that the one-sector formulations did not provide that much insight beyond his story. What I did not know at the time, however, was Krugman's subsequent work that was published in 1981, which was much closer to my way of thinking about the problem. Neither was I familiar with the book that Dixit and Norman were writing. I became familiar with these efforts in 1980 in Warwick, at a workshop organized by Avinash Dixit. The Dixit–Norman book was hotly debated in that workshop, but not so much for its coverage of trade in differentiated products as for the use of duality theory.

Trefler: It's one of the first books in international trade I ever read.

Helpman: To me, the biggest revelation in Warwick was that there were all these first-rate people working on trade with monopolistic competition. This was quite a shock. It was very useful to meet them and to talk to them about these problems. It was an important event.

Trefler: How about the collaboration with Paul?

Helpman: In 1982, I went on leave, to Harvard. This was after the war in Lebanon and I was eager to spend some time in a more peaceful environment. I also completed my term as Chairman of the Department in Tel Aviv. So off I went.

The first project I started to work on was the integration of multinational corporations into trade theory. I was also thinking about writing a book that would integrate everything.

Trefler: What did "everything" mean?

Helpman: Everything meant traditional trade, trade with product differentiation, multinational corporations, imperfect competition, and increasing returns. On some of these topics I worked for my *Handbook of International Economics* chapter—the first *Handbook* that was published in 1984.

Trefler: I was going to ask you about that.

Helpman: Jones and Kenen asked me to write for the *Handbook* a chapter that deals with economies of scale and imperfect competition in trade theory. I spent several months reading the literature, going back to the nineteenth century. I also went to the Widener Library at Harvard and read dissertations of Chamberlain's students who wrote on the subject. While working on this chapter, I started to see that there were many things in the literature that could be put together into a unified framework. At the same time, I was doing my research on multinationals and it struck me that this too could be made to fit this unified framework. It then occurred to me that I should write a book.

In 1983, I moved from Harvard to MIT, for the second year away from Tel Aviv. I used to go on sabbaticals two years at a time. We thought that it was better for the children this way, to stay a longer time in one place rather than take them back and forth more frequently. Paul then came back to MIT from the Council of Economic Advisors, where he had spent one year. We shared the trade course in the fall of 1993 (and I also shared a topics course with Rudi Dornboush in that year).

Trefler: One of my favorite personalities.

Helpman: So it was quite natural for Paul and me to talk about our common research interests, and we also discussed the idea of a book. It did not take long to decide to collaborate and write the book together. So we did.

Trefler: This is a very personal question, personal from my side. What was the point of that book? Is there a way of summarizing it? Maybe you already have. If you already have, then just pass on the question.

Helpman: At the beginning, the motivation was to integrate various parts of trade theory into a single theoretical framework, as I have just explained, and to present it in a systematic way. But once we started to work on the book, we realized that we could do better than this, that there were important gaps to be filled in. We felt that we could use this opportunity to also make an original contribution. Under different circumstances, we might have written a number of papers. Instead, we incorporated the new material into the book. Given the big changes that were taking place in trade theory and our involvement in these changes, we felt that we should use this opportunity to write a book that is as complete as we could make it. We never intended it to be a textbook, but this is what it turned out to be, because most people use it in their graduate classes. If we were thinking about a textbook, we would have most likely written it in a more user-friendly style.

Trefler: But it's good for graduate students. The fact that it isn't a textbook is a plus. I know my own sense of surprise when it struck me—when I was trying to find a unified Heckscher–Ohlin–Vanek prediction. I was very surprised to find out that was a major theme of your book. Even though I knew your book very well, it was only after I'd done my own work that I realized you'd already done this. This is a major theme.

Helpman: It turned out to be a major theme. It was not planned this way in the beginnings.

Trefler: I was quite surprised. The depth of the book—that I could be extremely well acquainted with the book, and still years later say, "Oh! I missed that." And how pervasive a theme it was.

PUBLIC POLICY

Trefler: I have the sense that you've avoided public policy ... . I'm finding that's just not true from this interview, but you seem to have avoided some controversy in the context of the strategic trade policy literature. You don't figure prominently in that. You're not in the ... that Krugman strategic trade volume, which was a set of very opinionated papers. You avoided that. Why is that?

Helpman: I had nothing important to say on the subject. The Brander–Spencer 1985 paper and the Eaton–Grossman 1986 paper are the key contributions to this literature. Others have clarified various points and made valuable observations of one sort or other, but these studies are the backbone. My view was, and it has not changed, that the only sensible conclusion from this literature is that strategic trade policy is not a good idea, because it cannot achieve much, if at all.

Trefler: Using what criteria?

Helpman: As we know, in the best of circumstances, strategic trade policy works in favor of some countries at the expense of others. It is not designed to attain global efficiency, but even in the cases in which it can work in favor of a country, it requires a lot of information in order to be implemented successfully. Mistakes in the judgment of conduct, for example, as Eaton and Grossman have shown, can turn a potentially successful policy into a failure. Similarly, mistakes in the evaluation of a new entry can turn a potentially successful policy into a failure, and the list of such possible errors contains additional nontrivial items. Therefore, we need to ask ourselves whether governments are capable of executing policies of this sort, whose success depends so extremely on the availability of detailed information and fine-tuning of the policy tools? My experience suggests that they are not.

Trefler: You're not coming from the Stiglerian political economy argument?

Helpman: I would say that they are not capable of doing it even if we disregard the political economy problems. They simply cannot amass the required information, which is more refined than what is required for other policy purposes. Adding political economy considerations just makes things worse.

A large part of the public debate that relied on the Brander–Spencer paper involved people who did not appreciate the subtleties of the arguments. Some people had a view that an active trade policy was desirable, and they used Brander and Spencer to provide academic respectability to their position. Those people did not care much about limitations of the arguments and often misused the theoretical results. I had no desire to get involved in arguments with such people. Admittedly, it is an important job to convince the public what constitutes a valid economic argument and what's invalid or wrong. It is also important to limit the influence of people who do not understand economics, but who use economic arguments to serve their own purpose, and twist in the process some valid points. But I did not want to get involved in all this. I admire Paul Krugman for the time and effort he has taken to educate the public, and he has indeed done a remarkable job. As for myself, I do not like public exposure. In Israel, for example, I refuse to be interviewed, and I rarely speak in public forums.

Since this is how I feel, it was quite natural for me to shy away from the debate on strategic trade policy. I had an opinion, but not original insights for a scientific paper. So I let it go. I kept busy doing other things.

POLITICAL ECONOMY

Trefler: Let me come to this last topic, which is political economy. At first glance, a strange field because the field was dead; it was flogging a dead horse. Why did you go into that field?

Helpman: Because I thought it was "too dead." I had a similar experience with other topics as well. I think that a topic is important, and then, when I teach it, I

https://doi.org/10.1017/S1365100599013061 Published online by Cambridge University Press

find that I am very uncomfortable teaching it as it is; it just does not feel right. Namely, I feel that this is not what I want to teach, but there is no better option available in the literature. The choice is between not teaching it at all, or teaching whatever is available. For this reason I sometimes choose not to teach a topic. Sometimes I teach it and feel unhappy at the end. This is how I felt about political economy. I did not always teach it, but each time I taught it, I did not like the outcome.

On the other hand, I felt that the subject is too important to be disregarded, and not only because of its intellectual attraction, but also, and perhaps especially so, because of its importance in practice. I had in Israel a number of amazing experiences that cultivated these feelings.

Trefler: One you can talk about?

Helpman: Sure. The most significant story from my personal point of view is the following. After the stabilization program of 1985 the government introduced an excise duty on imports. This was a temporary measure whose renewal required the approval of the Knesset's (the Israeli parliament's) Finance Committee. As a result they used to hold hearings during some of the discussions preceding the annual renewal of this tax.

A member of the ultrareligious party chaired the Finance Committee. He owned a carpet company, and the tariff on carpets was a few hundred percent. His company dominated the domestic market. At the time, as today, his party was very influential, and he personally was especially influential.

One day I was asked to appear before this Committee. They held a hearing prior to the renewal of the excise duty. I prepared a lot of material about the structure of protection in Israel, its effects on the economy, how much damage it was causing, why this particular form of protection was bad, and the like. My presentation was uneventful. The members of the Committee listen to my presentation, asked only a few questions, and I left. Outside, in the men's room I met a member of the Committee who left the room with me. We started to talk.

Trefler: Back-room politics.

Helpman: And this guy said to me, "Your lecture was very fascinating and I think that we learned a lot, but I should tell you that you wasted your time because the decision to renew the excise duty had already been made." We all hear stories about political deals. Nevertheless, I was very upset hearing that my testimony was just a formality, not a real input into the decision-making process. For a number of years I refused to testify after that.

I also had other experiences. Years later, in one particular instance concerning the Buy-Israel-Act, I was persuaded by a friend on the Economic Committee of the Knesset to testify. Rabin, who was the Prime Minister, was pushing this bill, whose intention was to force government-related companies to buy Israeli products, unless they were able to purchase foreign goods for at least 15% less. On that particular occasion the room was filled with lobbyists. They listened to the entire testimony and, to my great surprise, took also the liberty to intervene and

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make comments from time to time, until the chairman called them to order. It was an unbelievable spectacle, and the bill passed.

So I had a number of experiences that exposed me to lobbying. At the same time, the literature I was teaching did not deal with what I saw, except perhaps in a very crude and indirect way.

Trefler: You mean from the general economic theory of regulation?

Helpman: Yes. Hillman applied Stigler's approach to trade policy. And it was evident that you could not use it to answer a very elementary question: Why are textiles more protected than TVs? Or more generally, what explains the cross-sectional variation in the rate of protection?

Trefler: My big complaint is about the empirical relevance.

Helpman: I had the feeling that it was not suitable for addressing natural questions of this kind. When Gene Grossman and I finished the book on growth, we discussed a number of topics for future research. The political economy of trade policy was one of them. We felt that it was an important and a neglected topic, the work of Magee and his associates notwithstanding. There was also an empirical literature, which was almost independent of the theoretical contributions. And we felt that the theory was not well suited for empirical investigations. So we decided to develop a theoretical framework that would take explicit account of lobbying activities.

Trefler: And now there's going to be ... there's a slow trickle and it might become a flood of empirical work.

Helpman: I wish there were more empirical studies. In particular, studies that are disciplined by theory. Our structure is reasonable, but it can be improved. The paper by Goldberg and Maggi is beautiful. It is the type of empirical study that makes you happy to do applied theory, but more work is required along these lines. We need a better sense of how good an approximation is provided by our equations. Once we better understand in what ways the fit is not satisfactory, it will open the door to fruitful improvements of the framework. Or perhaps to an informed search for a different, better framework.

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