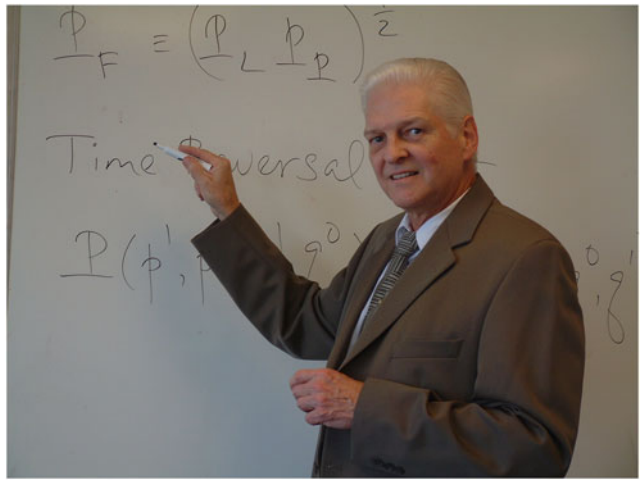
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THE ET INTERVIEW: PROFESSOR W. ERWIN DIEWERT[1]

Interviewed by Kevin J. Fox[2]
UNSW Sydney



Erwin Diewert has an exceptionally distinguished career as one of the world's most respected economists. His remarkable publication record speaks for itself. He is in his fifth decade of publishing in the leading journals of the economics profession, continuing to make significant influential contributions. While he has made his mark on an uncommonly diverse range of fields in economics, he is perhaps best known for his contributions to duality theory, index number theory, user cost of capital, functional form specification, international comparisons, international trade, and revealed preference theory.

Besides his major impact on the academic literature, he also has valuable engagement with national statistical offices and organisations such as the IMF and World Bank, in particular through contributing significantly to a series of manuals that are used to guide statistical agency practice in both developed and developing countries.

Note: A longer version of this interview is available as Discussion paper 16-02, Vancouver School of Economics, University of British Columbia.

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He has received many prestigious honours, including the following:

- Distinguished Fellow, American Economic Association
- Distinguished Fellow, Canadian Economics Association
- Fellow, Econometric Society
- Member, Royal Society of Canada
- Fellow, Academy of the Social Sciences in Australia
- Fellow, Society for Economic Measurement
- Research Associate, National Bureau of Economic Research

Many concepts that are now standard in economics derive from his contributions. His influence is pervasive. A few examples are sufficient to illustrate this: he named, generalised and popularised Shephard's Lemma (Journal of Political Economy, 1971), he defined "superlative" index numbers, he defined the class of "flexible" functional forms (which include the popular translog and AIDS functional forms), he introduced (with Caves and Christensen) the Malmquist productivity index, and developed and promoted the concept of the user cost of capital.

Shephard's Lemma is covered in standard microeconomics text books, the theoretical foundations he provided for index numbers led major statistical agencies to change their index formulae (at both aggregate and elementary levels), flexible functional forms are used in a huge variety of empirical contexts in academia and policy circles, the Malmquist productivity index is used extensively in literatures on efficiency and productivity analysis, as well as in operations research and management science, and the user cost of capital is used by leading statistical agencies in calculating capital services for official productivity statistics.

The interview covers many of these contributions. While much of his work has been highly technical, his ability to write and otherwise communicate clearly, as I believe is evidenced by his expositions in this interview, has assisted in much of his work having a broad impact beyond the academic literature.

I will make brief comments on two other aspects of his professional life which may not be obvious from the interview. First, as a co-author, he is generous, enthusiastic, patient, and much fun to work with. He enjoys interacting and discussing ideas, with a notable preference for doing so in convivial surroundings. There is a distinct lack of ego in these interactions, with the focus always on the quality of the ideas and points being made. Second, he is a dedicated educator; he never shirks teaching responsibilities, prepares and updates extensive class notes, and is a considerate, engaged, and inspirational thesis advisor. He often emphasizes the importance of educating future generations of public and professional economists, as well as academic researchers.

After decades of remarkable and innovative work, he continues to energetically conduct research on diverse topics, engage with government and international agencies, and undertake teaching and supervisory commitments. He modestly claims that his goal has simply been to be a useful member of society. I hope that this interview adequately communicates the extent to which he has most certainly achieved this admirable goal.

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How did you come to be interested in economics?

It is a bit of a long story. In high school in Vancouver, I was very interested in history and had in mind either becoming a High School teacher or an archaeologist. Fortunately, I had a very good math teacher in grades 10 and 11 who taught us Euclidean geometry and at this point, I saw the beauty and power of mathematics to solve problems. I took quite a few science courses in high school and of course, having a strong math background helped me to get quite good marks in science subjects. In our final year Provincial high school exams, I got the third highest mark in physics in the Province and so I went to the University of British Columbia in 1959 and entered the honours physics degree program. I soon learned that physics was a very mathematical subject; the teachers were using math concepts that we had not yet studied in my companion math courses! Thus in my second year at UBC, I dropped out of the physics program and went into the honours math program, with the idea that once I learned more math, I could go back to physics. However, a close friend of mine was in my carpool and as we drove out to the University, he talked about an introductory economics course that he was taking. It sounded interesting and so I sat in one of his classes and found the material quite fascinating. In fact, there seemed to be some sort of mathematical structure to economics. In the following two years, as I continued on with my math degree, I took all of my outside courses in economics. In addition to the usual micro and macro courses, I took advanced micro theory and industrial organization from Milton Moore, development economics from Ibrihim Poroy, international finance from Gordon Munroe, econometrics from Gideon Rosenbluth, and mathematical economics from Gideon and Rodrigo Restrepo. (It turns out that Rodrigo was a student of Samuel Karlin and I adopted the mathematical notation used by Karlin and Restrepo for the rest of my life). I graduated with an honours math degree from UBC in 1963 but I really did not know what to do with my life at that point. I decided to postpone any serious decision about what direction to take by enrolling for a Master's degree in mathematics at UBC as I still enjoyed learning about the different branches of mathematics. I should mention that in my MA thesis ("Analysis of Variance Estimators for the Seasonal Adjustment of Economic Time Series") I tried to devise a method for determining whether the seasonal adjustment factors were additive or multiplicative to the trend. This is a difficult topic and I returned to it periodically over the years. In any case, I continued to take outside units in economics during my MA year in 1963–1964. Milton Moore was very influential at this point; he urged me to apply for graduate school in economics and so I applied to the University of California at Berkeley and did my Ph.D. in economics there during the years 1964–1968. That was the start of my career as an economist. Looking back, I was very fortunate in having many great math teachers during my early years.

Who influenced you during your student days at Berkeley?

Again, I was lucky enough to have many great teachers during my years at Berkeley, including Gerard Debreu (mathematical economics), Sidney Winter and

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Daniel McFadden (microeconomics), Amartya Sen (macroeconomics), Richard van Slyke (linear programming), Olvi Mangasarian (nonlinear programming) and Roger Wets (stochastic programming) in Industrial Engineering and Edward Barankin (stochastic processes) and Erich Lehmann (hypothesis testing and nonparametric methods). Lehmann had a unique teaching style: every class, he would give us a couple of problems to solve and hand in at the beginning of the next class. I very much liked this approach (there was no need to cram for exams; one learned the material as the course progressed) and so I eventually adopted his style in my own classes.

The two most influential teachers I had at Berkeley were Dale Jorgenson and Dan McFadden. Dale was very active in the Econometrics Workshop at Berkeley and I enrolled in this course during my first year. Somehow Dale took an interest in my education. We would have meetings in his office every month or so. Initially, we talked about capital theory and applied general equilibrium models that could be estimated econometrically. He would direct me to various articles and books to read (e.g., he noted that Walras had developed user cost theory way back in 1874). Then at the next meeting, we would discuss what I had read. I was super impressed with Dale's ability to pick up the conversation one month later exactly at the same point where the last conversation left off. Dale also spoke very quickly. I was a bit of a country bumpkin and found it difficult initially to keep up with him but after a while, I got better at following his arguments. In any case, Dale has had a profound influence on me. Basically, Dale uses economic theory and econometric methods to solve important applied economics problems. Thus Dale has made important contributions to capital theory, applied general equilibrium modeling, consumer theory, production theory, and tax policy to name a few. Throughout my career, I have tried to follow in his footsteps.

Dan McFadden has a similar thrust to his research and he too profoundly influenced me. During the summer of 1967, I worked in Ottawa for the Department of Manpower and Immigration on the problem of the demand for different types of labour for an industry. The approach that was being used at that time was very simple and was called the *manpower* (nowadays we would say *personpower*) *requirements approach*. I had taken production theory from McFadden and Winter at Berkeley and we learned about factor substitution in their course. Thus I thought that the approach should be generalized to allow for factor substitution. During the summer of 1967, I estimated a small model and in the fall of 1967, I presented my results to the Econometrics Workshop at Berkeley. Dan McFadden was in the audience and commented on my presentation as follows: "But Erwin, your demand system is not integrable." I thought to myself, "What the heck is integrability?" Needless to say, after the seminar, Dan explained the concept: if the producer's cost function is differentiable with respect to its input price components, then consistency of the model requires that the first order partial derivative of the cost function with respect to the ith input price must equal the ith input demand function. This is Shephard's Lemma. Dan directed me to Ronald Shephard's (1953) book and to his recent Berkeley working paper,

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McFadden (1966), to read up on this problem. It is then that I discovered duality theory; if producers or consumers behaved as price takers, then their technologies and preferences (with some regularity conditions) could be perfectly described by dual cost or profit functions (for producers) and dual expenditure or indirect utility functions (for consumers). A large portion of my early research revolved around duality theory and its applications. But back to my cost function problem. I had to accept that Dan's criticism of my suggested demand system was valid so I was a bit disappointed that my simple system was not going to be the answer to getting factor substitution into producer demand systems. However, one day as I was sitting in a class and my mind wandered, I thought: what if I insert a square root sign into my suggested demand system and impose some symmetry conditions? This new demand system passed the integrability test and moreover, I was able to show that the resulting cost function was a *flexible functional form*; i.e., it could approximate an arbitrary differentiable cost function that was dual to a constant returns to scale convex technology to the second order around any point. These results became a part of my Ph.D. thesis (McFadden became my thesis advisor) and led to the Generalized Leontief Cost function and my first published paper, Diewert (1971). Thus it can be seen that I owe a lot to Dan McFadden.

How would you describe the content of your Ph.D. thesis?

The title of my thesis was "Functional Form in the Theory of Production and Consumer Demand". Basically, what I was trying to do is to come up with new methods for deriving systems of consumer demand and producer supply and demand functions which were consistent with optimizing behavior on the part of consumers and producers, where the unknown parameters which characterize preferences and technology could be estimated using basically linear regression techniques. At the same time, I wanted the preferences or production functions to be able to provide second order approximations to arbitrary twice continuously differentiable preferences or technologies; i.e., I wanted the functional forms to be *flexible*. Thus my thesis came up with flexible functional forms for single output production technologies, for a multiple output but single input technologies and for general multiple output and input technologies. These papers were later published as Diewert (1971, 1973, 1974a).

Flexible functional forms, such as the translog, are now commonly used in empirical work, but more restrictive Cobb-Douglas and Constant Elasticity of Substitution (CES) functional forms are still the "workhorses" for many economists. From your perspective, what is the importance of the flexibility concept?

If a functional form can provide a second order approximation to a utility or production function or to its dual cost function, then the resulting consumer or input demand functions can provide a first order approximation to arbitrary demand systems and the resulting pattern of demand elasticities can be completely

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arbitrary, consistent with the restrictions imposed by cost minimizing behavior. The problem with traditional functional forms like the Cobb–Douglas or CES is that the elasticities of demand that these functions generate are severely restricted *a priori*. Thus if these functions are used for policy purposes, there is a good chance that the results will be seriously biased due to the inflexibility of these functional forms.

What attracted you to duality theory? Specifically, what is the advantage of using duality theory to generate systems of derived demand and supply functions?

Suppose we start with a flexible functional form for a single output production function and generate the system of input demand functions that correspond to the given functional form by solving the associated constrained cost minimization problem. The resulting demand functions are typically highly nonlinear in the unknown parameters and in some cases it is not even possible to find explicit expressions for the demand functions. Econometric estimation of such demand systems is not straightforward. Contrast this direct approach to the generation of input demand functions with the dual approach, which starts with a strategically chosen functional form for the dual unit cost function. If we choose the functional form for the unit cost function to be a quadratic form in the square roots of input prices as in the Generalized Leontief Cost function mentioned earlier, using Shephard's Lemma we get a system of input demand functions that are *linear* in the unknown parameters that characterize the unit cost function. This facilitates econometric estimation. The cross equation symmetry conditions can either be imposed or one can test for their validity. McFadden (1966; 13) basically noted this advantage of duality theory (as a simple way of obtaining derived demand functions) but I think my contribution was to work out specific examples of how his idea could be implemented with functional forms which were also flexible (and linear or almost linear in the unknown parameters). Diewert (1974b, 1993a) were survey papers on these applications of duality theory to producer and consumer theory. The counterpart to Shephard's Lemma in the multiple output and input case was first worked out by Hotelling (1932; 597) and applications of Hotelling's Lemma to generate systems of derived input demand and output supply equations using flexible functional forms for variable profit functions can also be found in Diewert (1973, 1974b) and McFadden (1978).

Is it true that you introduced the terms flexible functional form, Shephard's Lemma and Hotelling's Lemma?

Yes. It is always nice to introduce terms into the literature that catch on.

Who were the students at Berkeley with whom you interacted?

I had a great peer group at Berkeley. The economics students whom I talked to the most while there were Michael Denny, Melvyn Fuss, and Lawrence (Larry)

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Lau and to a lesser extent, Charles (Chuck) Hulten and Laurits (Lau) Christensen. I continued to see these folks in later years and even eventually collaborated with Hulten and Christensen. All of these students did their thesis work with either Dale Jorgenson or Dan McFadden and their theses were on either capital theory or applications of duality theory or both. Larry played a big role in the development of the translog functional form. After I had come up with the Generalized Leontief cost function, Dale realized that rather than taking a quadratic form in square roots of prices to form a unit cost function, one could take a quadratic form in the logarithms of prices and set the resulting functional form equal to the logarithm of the unit cost function. Similarly, one could take a quadratic form in the logarithms of input quantities and set the resulting functional form equal to the logarithm of the production function. Dale showed me the resulting translog functional forms in one of our monthly meetings. I was skeptical about the functional form for the unit cost function and pointed out that the unit cost function had to be a linearly homogeneous function in input prices. But Larry figured out exactly the restrictions on the parameters of the translog functional form that would ensure that it was linearly homogeneous, without destroying the flexibility of the resulting functional form; see Christensen, Jorgenson, and Lau (1971). This was a very useful accomplishment: the translog functional form is one of the most frequently estimated flexible functional forms in the applied economics literature.

Let's go back to your last year at Berkeley. How did things go for you on the job market?

I went to the annual American Economic Association meetings in January of 1968. At the time, I did not have a complete thesis yet but I had written up my Generalized Leontief paper and my thesis supervisor thought that I was ready to go on the job market. I remember interviewing for jobs at the Commerce Department at the University of British Columbia (UBC) and at the Economics Departments at the Universities of Western Ontario, Chicago and MIT. I was surprised to get job offers from all four departments. I wanted to go back to UBC but I did not quite see how my research interests in basic measurement problems would be relevant in a business school setting. Paul Samuelson and Robert Solow, two of my economic heroes, were at MIT and I was not sure that I was quite good enough to be a fellow professor with those giants of the profession. So I was leaning towards going to Chicago, where I thought I might fit in better. I did not know at the time that there were many giants at Chicago as well! I remember meeting Zvi Griliches at my seminar there and I knew something about him since the classic paper by Jorgenson and Griliches (1967) on measuring Total Factor Productivity (TFP) growth had just appeared. Hirofumi Uzawa was also listed on the faculty at the time and he had written a great paper on the duality between cost and production functions (Uzawa, 1964). In any case, there was another complication on the horizon. I was planning to get married to my wife, Virginia, during the summer of 1968. She was a dentist who worked for Vancouver General Hospital in Vancouver, but she was interested in getting a Master's degree in Orthodontics.

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Al Harberger, who was a Professor at Chicago at the time helped us out by putting in a good word with a Dentistry Professor he knew at Northwestern University and so it transpired that Virginia was admitted to the Master's program at Northwestern. So this cinched the deal; we went to Chicago in the fall of 1968. I had not finished my thesis at that point but I managed to write it up during the academic year 1968–1969 and I got my Ph.D. from Berkeley (signed by Ronald Reagan, who was then the Governor of California) in 1969.

What was it like teaching at the University of Chicago?

It was a great experience. I taught a graduate course in Mathematical Economics and I had some excellent students, including William Barnett, Vernon Henderson, Rachel McCulloch, Mike Mussa, and Douglas Purvis. Doug also influenced my teaching style. He noticed that I had very detailed written notes which I dutifully transcribed onto the blackboard so he suggested: why not just distribute these notes to the class? I thought that this was a pretty reasonable request so ever since then, when I teach a course, I give the students a copy of my lecture notes.

Who were the faculty members at Chicago that you interacted with?

The senior faculty members that I interacted most with were Zvi Griliches, Marc Nerlove, and Arnold Harberger. Nerlove was very interested in cost function estimation and Griliches was interested in all aspects of economic measurement. He had a profound influence on me that was similar to the influence of Jorgenson and McFadden. Arnold Harberger and I talked about methods for measuring economic welfare and the fundamentals of cost benefit analysis. These conversations stayed with me for a long time and eventually led to a number of papers on the measurement of individual and social welfare (e.g., Diewert (1992)) and on cost benefit analysis (Diewert, 1983a). I attended the econometrics workshop regularly and that was always interesting. The senior faculty members attending the workshop were Griliches, Nerlove, Hans Theil and Arnold Zellner. Theil and Zellner did not get along very well. It was more or less normal that there were vigorous discussions at Chicago and it took me a while to adjust to this somewhat confrontational style. I remember giving a presentation of a chapter out of my Ph.D. thesis on estimating a Generalized Leontief cost function using US aggregate data. Zvi Griliches was not impressed. He told me after the seminar that there were too many parameters in the function and the data did not support the estimation of so many parameters. I was totally crushed by this negative assessment of what I thought was a great idea; i.e., the estimation of a flexible functional form using US data. But after a while, I dismissed his criticism: after all, how could official US data be unreliable? I will come back to this point later in this interview when we discuss my work in the 1980s.

Other senior faculty members at the University of Chicago who I talked to occasionally were Robert Fogel and Robert Mundell. I also shared an office for

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one year with J. Richard Zecher and Stanley Fischer, so I got to know these younger faculty members quite well. Other junior faculty members that I interacted with were Robert Gordon and Dierdre McCloskey.

I understand that you were asked to referee a paper by Sydney Afriat for the Journal of Political Economy, which eventually led to an influential publication on your part. Can you tell us about this?

Afriat's paper that I was asked to referee was a very interesting and innovative one and it was eventually published as Afriat (1973). The problem was that the paper referred to an earlier published paper, Afriat (1967). This earlier paper showed how a finite set of price and quantity data pertaining to a household could be tested to see whether the data were consistent with utility maximizing behavior. If Afriat's test passed, then he showed how the household's preferences could be represented by a concave utility function and he showed how to construct this function. His approach was entirely nonparametric; i.e., it was not necessary to make parametric assumptions about the functional form of the utility function. Thus Afriat (1967) was a very fundamental paper, but it was extremely difficult to read. I remember spending two weeks trying to figure out what was going on with the two Afriat papers and I finally succeeded. I figured out how to represent Afriat's 1967 testing procedure by setting up a simple linear programming problem involving the observed data. If the optimized objective function for this program turned out to be zero, then the data were consistent with utility maximizing behavior and the concave utility function which rationalizes the data could readily be constructed. I also noted that it was necessary to add a couple of restrictions on the class of utility functions to ensure that Afriat's Theorem would be true, and I provided a much simpler proof of his result. I wrote all this up in a very detailed referee report and asked the author to use this material to make his new paper more readable. Sydney refused to make any changes so the JPE rejected his paper. A couple of years later, I thought about Sydney's 1967 paper and how it was too bad that hardly anyone understood his test and so I decided to dig up my old referee report and I turned it into a paper, "Afriat and Revealed Preference Theory" which was eventually published; see Diewert (1973). As people came to appreciate Afriat's results, there was a great flowering of papers in this area, starting with Varian (1982) who developed a more efficient method for checking the Afriat conditions.

Why did you leave the University of Chicago in 1970? It has been quoted that, in response to your decision to return to Canada, Arnold Harberger said "Erwin has a great production function but a lousy utility function."

I really enjoyed the intellectual atmosphere at Chicago but unfortunately, I found it hard to adjust to the climate having lived on the West Coast of North America all my life. At the time, Illinois power plants burned coal that had very

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high sulfur content and this affected my health somewhat. When my wife's 18 month Master's degree program at Northwestern ended, I looked to go back to Vancouver and I managed to get a job at the UBC Economics Department, starting in September of 1970.

I believe that you had a few months before taking up the job at UBC. What did you do with your time?

I left Chicago at the end of March 1970 and commenced at UBC in the fall of 1970. Zvi Griliches was only at the University of Chicago during my first academic year at Chicago, 1968–1969, after which he took up an appointment at Harvard. I was very fortunate that he took an interest in my research during our year together at Chicago. When I quit Chicago, he invited me to visit Harvard until I started teaching at UBC. Zvi arranged office space for me; I had a large closet that was attached to Giora Hanoch's office, who was another visitor that Zvi supported. I gave a talk on my referee report on the Afriat paper while at Harvard and Giora attended my seminar and realized that the same nonparametric approach could be applied to production theory. This was eventually published as Hanoch and Rothschild (1972). A couple of Ph.D. students of mine made further contributions which eventually led to a couple of published papers; Diewert and Parkan (1983) and Diewert and Mendoza (2007). Hal Varian also made important contributions; see Varian (1984).

What research topics did you work on when you arrived at UBC?

Initially, a lot of my time was devoted to publishing the discussion papers that I produced while at Berkeley and Chicago. It was amazing that I was hired as an Associate Professor in 1970 and I did not have a single published paper until 1971, but I had quite a few papers in the pipeline. I also embarked on some new research projects in the early 1970s.

I regard economics as the study of choice under constraint. We have two main constrained maximization problems that we use to model the economy: (i) consumers maximizing utility subject to a budget constraint and (ii) producers maximizing profits subject to their production function or more generally, their technology constraints. These two constrained maximization problems generate household and producer demand and supply equations which interact to produce equilibrium prices. Governments enter the picture by introducing tax wedges and using tax revenues to produce various goods and services as well as monetary transfers to certain households.

Hicks (1946) and Samuelson (1947) were the pioneers in establishing the mathematical properties of these derived demand and supply functions and more generally in working out the implications of maximizing behavior. The two sides of the market are brought together in the study of general equilibrium theory. But the temporary equilibrium theory of Hicks where producers and consumers only form expectations about future prices is a much more realistic framework than the pure

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futures equilibrium which was dismissed by Hicks as being a poor approximation to reality. In the early 1970s, I attempted to provide practical models of the temporary equilibrium that could perhaps be econometrically implemented and then used for policy purposes; see Diewert (1974c, 1977). This was my first attempt to deal with the accounting problems that arise when producers use durable inputs in their one period production functions. Both of these papers derived the user cost of capital in a much simpler way than had been done in the current literature.

What else did you work on in the 1970s?

One paper was "A Note on Aggregation and Elasticities of Substitution", Diewert (1974d). This note explains why elasticities of substitution in single output production function studies tend to be small in magnitude if the number of inputs is small but these elasticities tend to grow in magnitude as there are more inputs in the model. The reason for this is as follows: if there are only two inputs, the two inputs must be substitutes so the elasticity of substitution must be positive or zero. But as we disaggregate, complementarity becomes more common. If we aggregate to a two input model, the positive and negative elasticities largely cancel each other out, leading to a relatively small aggregate elasticity of substitution between the two aggregate inputs.

A much more substantial paper was "Optimal Tax Perturbations", Diewert (1978a). This paper was intended to be a supplement to the optimal tax literature which was developed around this time. I introduced a method for determining whether a Pareto improving direction of tax change could be implemented. This method relied on Motzkin's Theorem of the Alternative which I learned in my course in nonlinear programming at Berkeley taught by Mangasarian. This technique proved to be very useful and I used it in a number of joint papers with Alan Woodland who was a colleague of mine during the 1970s and with Arja Turunen who was a Ph.D. student of mine in the 1980s; see for example Diewert, Turunen-Red, and Woodland (1989).

I also continued my research on application of flexible functional forms during this period. Ernst Berndt was a colleague at UBC during the 1970s and we wrote a paper together, along with my first Ph.D. thesis student, Masako Darrough, which integrated income distribution information with the estimation of a translog demand system; see Berndt, Darrough, and Diewert (1977).

You are very well known for your influential research on index numbers, yet there has been no mention of this work so far. When and how did you get into index number research?

During my time at Chicago, I started to get interested in index number theory. I realized that it would not be possible to estimate flexible functional forms if the number of commodities in the model was large. How exactly should we aggregate the number of commodities into a manageable number so that a complete matrix of price elasticities of demand (or supply) could be estimated?

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Around 1972, I started reading papers on index number theory. I found two papers, by Robert Pollak (1983) (the discussion paper version of this paper was published by the Bureau of Labor Statistics in 1971) and Sydney Afriat (1972) in particular, which were very interesting: they related functional forms for a consumer's utility function to functional forms for bilateral index number formulae, like the Laspeyres, Paasche, and Fisher indexes. In particular, Afriat (1972; 45) noted a result first derived by the Russian mathematician Buscheguennce, or Byushgens (1925); the first spelling is how someone translated the original Russian into French, whereas the second is my spelling, which is an accurate translation from the original Russian. The result is that if a consumer maximized a homogeneous quadratic utility function, then the utility ratio between periods was *exactly* equal to the Fisher (1922) ideal quantity index (the geometric mean of the Laspeyres and Paasche quantity indexes) and the true cost of living index was *exactly* equal to the Fisher ideal price index (the geometric mean of the Laspeyres and Paasche price indexes).

This seemed to me to be a very important result since I was able to prove that the homogeneous quadratic function was a *flexible functional form* and hence could approximate any differentiable homothetic preference function to the second order. Using the Byushgens result, it is possible to construct aggregate prices and quantities that are consistent with utility maximizing (or cost minimizing) behavior and will closely approximate the "truth" without having to undertake any econometric estimation. Although the underlying aggregator function had to be linearly homogeneous, the number of commodities in the aggregate could be arbitrarily large. Thus there was a connection between flexible functional forms and certain index number formulae.

How were you able to find this remarkable paper by Byushgens?

During the 1970s and 1980s, Larry Lau, my old classmate from Berkeley, was able to invite me to visit Stanford during the summer to participate in the Mathematical Economics Workshop run by Mordecai Kurz. Thus I had access to Stanford's Hoover Library which had a huge collection of post-World War I Russian journals and books. Fortunately, during my undergrad years at UBC, I took three years of Russian and so I was able to find Byushgens (1925).

I also discovered a great paper by Konüs and Byushgens (1926) which had not been noticed in the literature. It had other exact index number results in it; for example, they showed that the share weighted Jevons quantity index was exact for a Cobb–Douglas utility function. I was able to modify the proofs to cover additional classes of preferences such as the quadratic mean of order r and translog aggregator functions and find exact index number formulae which corresponded to these flexible functional forms. I also clarified the conditions that needed to be imposed on the matrix of coefficients in the homogeneous quadratic utility function.

I gave a seminar on my results at Stanford in 1973 and the paper was eventually published as Diewert (1976); it was rejected by five major economics journals but

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Dennis Aigner was brave enough to publish it in the *Journal of Econometrics*. In addition to deriving results for the case of linearly homogeneous aggregator functions (utility or production functions), I also showed that the Törnqvist price index was exact for a general translog (nonhomothetic) cost function and provided a similar exactness result for nonhomothetic translog distance function. It seems that not many researchers know about these nonhomothetic translog exactness results. I also made my first attempt to measure Total Factor Productivity (TFP) growth using exact index numbers and making translog assumptions about the technology. I was attempting to justify the Divisia measures of TFP growth that were used by Jorgenson and Griliches (1967). My results on TFP measurement in this paper were not entirely satisfactory; I made some separability assumptions between inputs and outputs and so the results were not general enough. Later, Diewert and Morrison (1986) and Diewert and Fox (2010) made less restrictive assumptions and derived much more satisfactory exact index number measures of TFP growth. It turns out that the translog functional form is well suited to exact index number applications.

The term "superlative index" is now commonly used to describe indexes such as the Fisher ideal and Törnqvist indexes. Did you introduce this term into the economics literature?

Not quite but I gave the term a more precise meaning. Irving Fisher (1922) in his famous book on the axiomatic approach to index number theory introduced the term "superlative index". However, he did not really give a proper definition for this term. He classified an index number formula as being "superlative" if it was numerically close to his Fisher ideal index. In my 1976 index number paper, I termed an index number formula to be "superlative" if it was *exact* for either a linearly homogeneous aggregator function or its dual unit cost function, where either the aggregator function or the unit cost function was a *flexible functional form* in the class of linearly homogeneous functions. Thus the Fisher ideal index and the Törnqvist indexes are both superlative indexes. Superlative indexes have caught on and are increasingly used by statistical agencies.

I believe you wrote an additional paper with further related results following this.

Yes, Diewert (1978b) was a follow up paper on my 1976 paper. There were a number of interesting results in this paper. First, I was able to show that all known superlative index number formulae approximated each other numerically to accuracy of a second order Taylor series approximation if the derivatives were evaluated at a point where the two price vectors were equal and the two quantity vectors being compared were equal. Second, I considered the two stage aggregation of superlative indexes and compared the two stage index with its single stage superlative counterpart. I was able to show that superlative indexes, while not exactly consistent in aggregation, were approximately consistent in aggregation.

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Finally, I used a numerical example and showed that chaining the indexes reduced the spread between the Paasche and Laspeyres formulae and also reduced the spread between commonly used superlative indexes. From this exercise, I concluded that using chained indexes was probably more appropriate than using fixed base indexes, at least for annual data.

These papers seem to have been influential in changing the practice of some national statistical offices.

Yes, I believe that they helped provide a justification for the US Bureau of Economic Analysis to move away from the fixed price indexes they were using to measure GDP growth prior to 1996 and to implement the chained Fisher index methodology that they are currently using.

What were some of the other areas of economic research during the 1970s?

I was still working on the Hicks and Samuelson research agenda which attempted to determine the empirical implications of (competitive) optimizing behavior. The first paper along these lines was Diewert and Woodland (1977) followed by, for example, Diewert (1985). There was also a related paper, Blackorby and Diewert (1979), where we showed that a local second order approximation to a utility function also provided a local second order approximation to its dual expenditure function. Charles Blackorby was another colleague of mine at UBC and we spent a lot of time over beers (with Chris Archibald, David Donaldson, and William Schworm) discussing the finer points of separability, duality theory and the measurement of social welfare.

You also followed up your initial research efforts on the user cost of capital in the latter half of the 1970s.

Yes. Around that time, I started to attend the meetings of the National Bureau of Economic Research (NBER) and in particular, the meetings of the Conference for Research in Income and Wealth (CRIW). Dan Usher organized a CRIW conference on the measurement of capital held in Toronto in 1976 and I contributed a paper on the problems associated with the measurement and aggregation of capital: Diewert (1980). Around this time, Dale Jorgenson and Zvi Griliches (1972) got into a controversy with Edward Denison on how exactly should capital services be aggregated. They demonstrated that the method of aggregation matters empirically. I sided with Jorgenson and Griliches in this dispute.

But I also got into a bit of a dispute with Dale that has persisted to the present day. The user cost of a particular capital stock component used in production comprises the sum of interest rate, depreciation rate and tax rate terms less the *expected or actual ex post* rate of asset price appreciation over the accounting period times the beginning of the period asset price. Jorgenson has always maintained that the *actual ex post rate* of asset price inflation is the appropriate term

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to insert into the user cost formula whereas I maintained that the *expected rate* of asset price inflation should be used. The use of ex post inflation rates will generally lead to *negative user costs*, which does not make a great deal of sense. In the following decades, I returned to the topic of capital measurement repeatedly; see for example Diewert (2010a) and Diewert and Fox (2016a).

Let us turn to your research interests in the early 1980s. One of these seems to be the measurement of waste.

Debreu (1951; 285) distinguished three types of waste in an economy: (i) underemployment of existing resources (i.e., unemployment), (ii) technical inefficiency, and (iii) inefficiency due to the imperfection of economic organization. But how exactly can we measure these types of waste quantitatively? I tried to answer this question in a series of papers; see Diewert (1981a, 1983b, 1984, 1985).

I made another contribution to the measurement of inefficiency in the production sector of an economy. Farrell (1957) showed how technical and allocative inefficiency in production could be measured if one had estimates of the best practice technology production possibility sets in hand. In Kopp and Diewert (1982), we showed how Farrell's methodology could be applied if instead of a direct measure of the efficient production possibilities set, only an indirect representation was available in the form of a best practice cost function. This paper is widely cited so it served a useful purpose.

Another research interest in the early 1980s seems to have been the study of generalized concavity.

This was a fun area for me. Obviously, concavity and quasiconcavity arise naturally in economics and so I was well aware of the importance of generalizations of concavity to economics. Mangasarian introduced me to the concept of pseudoconcavity in the differentiable case while I was a student at Berkeley. Pseudoconcave functions have the property that the first order necessary conditions for maximizing a differentiable function are also sufficient for a maximum and so these functions are also useful in economic applications. In the early 1980s, I came into contact with a couple of Israeli industrial engineers, Mordecai Avriel and Israel Zang, and we interacted to produce the paper, "Nine Kinds of Quasiconcavity and Concavity"; see Diewert, Avriel, and Zang (1981).

I went on to produce two more papers on this topic, including Diewert (1981b). This paper is a nice one for me; in it, I was able to prove a Generalized Mean Value Theorem without making any differentiability assumptions. This is probably my one and only theorem in the mathematics literature! In this paper I also generalized the concept of pseudoconcavity to the nondifferentiable case. I joined up with Avriel, Zang, and another industrial engineer from Germany, Siegfried Schaible, to produce the book, *Generalized Concavity*, which was published in 1988. What is remarkable is that in 2010, the Society for Industrial and Applied

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Mathematics reprinted this book in their series, *Classics in Applied Mathematics*; see Avriel, Diewert, Schaible, and Zang (2010).

In addition to your work on the measurement of waste and on generalized concavity, you seemed to continue your work on index number theory.

Yes, I had four papers on index number theory appear in 1981–1982 and two of these papers turned out to be quite influential. The first paper was Diewert (1981c), which gave a comprehensive review of the economic approach to index number theory. The second paper was joint work with Robert Allen, who was an economic historian at UBC during this period. He was attempting to measure the productivity of US steel mills between 1889 and 1909, where productivity growth is measured as an output index divided by an input index. Bob collected data on the prices and quantities of outputs produced and inputs used by US steel mills over this period but the question was: should we form price indexes for outputs and inputs using our favorite price index formula and then calculate quantity indexes residually by deflating output and input values by their corresponding price indexes, or should we form quantity indexes for outputs and inputs *directly* using our favorite quantity index formula? Using the direct strategy means that the price indexes are calculated residually by deflating output and input values by their corresponding quantity indexes.

Allen and Diewert (1981; 433) showed that the choice of aggregation strategy could matter empirically. If the variation in prices is more proportional than the variation in quantities, then Allen and Diewert thought it best to aggregate prices first and generate the corresponding quantities residually and if the variation in quantities is more proportional than the variation in prices, it is best to aggregate quantities directly.

But how can we decide whether prices vary more proportionally than quantities? Allen and Diewert (1981) suggested a procedure. Many years later, I returned to the problems associated with measuring the degree of proportionality (or the amount of similarity) between two positive vectors of the same dimension. It turns out that these questions play an important role in making comparisons of prices and quantities across countries; see Diewert (2009a).

The third and fourth papers on index number theory in the early 1980s were joint work with Laurits (Lau) Christensen and Douglas Caves. In Caves, Christensen, and Diewert (1982a), we defined output and input indexes for very general multiple output, multiple input technologies using Malmquist and Shephard distance functions. A feature of these definitions was that these output and input indexes did not depend on output or input prices and thus these definitions appealed to industrial engineers and operations researchers. However, in order to calculate these indexes without knowledge of prices one had to know the underlying technology, information which is usually not available. We showed that if the output distance functions for a production unit could be represented by certain translog functional forms for two time periods and the economic agent

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engaged in revenue maximizing behavior, then a certain Malmquist output index was exactly equal to the Törnqvist output quantity index. Similarly, we showed that if the input distance functions for a production unit could be represented by certain translog functional forms for two time periods and the economic agent engaged in cost minimizing behavior, then a certain Malmquist input index was exactly equal to the Törnqvist input quantity index. These results are fine. The paper also provided a definition of productivity growth using Malmquist indexes and Theorem 3 in Caves et al. (1982a; 1404) attempted to derive an exact index number formula for a measure of TFP growth, assuming that the two technology sets can be described by translog output distance functions.

In order to derive this result, we assumed competitive revenue maximizing behavior conditional on input quantities and competitive cost minimizing behavior conditional on output quantities. These assumptions are satisfactory provided that the underlying technology sets are subject to either constant returns or decreasing returns to scale, and producers take prices as given. But it is well known that competitive revenue maximizing behavior is not consistent with increasing returns. Moreover, our definition of productivity growth was really a definition of technical progress; i.e., of a shift in the technology set over time or space. Thus the paper was not entirely satisfactory on the topic of measuring TFP growth when there are increasing returns to scale. Diewert and Fox (2010; 89) provided a theoretically sound relationship between the Törnqvist output and input indexes and measures of technical progress and returns to scale by introducing monopolistic markups into the translog distance function framework. A specialization of the Diewert and Fox results to the competitive case with constant returns to scale led to the measure of TFP growth used by Jorgenson and Griliches (1967). In a related paper, Diewert and Fox (2008; 177–178) used translog cost functions, monopolistic markups and exact index number techniques to derive relatively simple relationships between output and input growth rates, returns to scale and measures of technical progress.

My second paper with Caves and Christensen was my first paper on making multilateral index number comparisons. When constructing indexes to compare production units across space, there is no natural ordering of the data. For international comparisons, we could take one country as the base country and then construct fixed base indexes of output and input across the production units in our sample using our favorite bilateral index number formula. However, it turns out that the resulting indexes are not invariant to the choice of the base country. Gini (1931) provided a simple solution to this lack of invariance problem: he constructed fixed base Fisher indexes using each country in turn as the base country and then he took the geometric mean of these base country specific sequences of indexes. His method for making international price or quantity comparisons is known as the GEKS (Gini, Eltetö, Köves, and Szulc) method.

Caves, Christensen, and Diewert (1982b) showed how Gini's methodology could be used to construct indexes of output, input and productivity for cross sectional or panel data sets, which consisted of prices and quantities for the

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inputs used and outputs produced for the production units. But instead of using the Fisher formula to do the aggregation of outputs and inputs, they drew on the results of Caves et al. (1982a) and used Törnqvist output and input indexes as their bilateral index number formula. The major advantage of this adaptation of Gini's approach is that we were able to give a strong production theory justification for the use of the Törnqvist formula. Although this methodology has been widely used, there are two problems with it that are sometimes troublesome: (i) all output and input quantities have to be positive for every observation in the panel and (ii) the methodology cannot be used to make comparisons of value added (or GDP) across production units; only gross outputs can be compared using this methodology. The problem is that the CCD methodology relies on output and input distance functions and output distance functions do not exist in general if there are negative outputs, i.e., intermediate inputs, in the output aggregate. This second difficulty has recently been addressed by Inklaar and Diewert (2016), where we made functional form assumptions on the value added or GDP functions that characterize the production units. This new approach allows us to have value added or GDP as the output concept and to make TFP comparisons across production units.

Around this time, you also got involved with giving advice to Statistics Canada. How did this come about?

Martin Wilk became the Chief Statistician of Statistics Canada at the end of 1980. He was a professional statistician and he had some definite ideas on how to improve Statistics Canada. He decided that his staff should interact more with the public and the academic community and so he organized a conference on price measurement that was held in Ottawa in November, 1982. Since I had written a fair number of papers on index number theory by that time, I was asked to help organize the conference and I and Claude Montmarquette (a Ph.D. student of mine) edited the conference proceedings which appeared in 1983. Another improvement that was initiated by Martin Wilk was to set up technical advisory committees for the different divisions of Statistics Canada. The members of these committees were academics, business economists, knowledgeable users, and staff members from other statistical agencies. This was a good idea and it has been copied widely elsewhere, in the United States and Australia for example. I was the chair of the first of these Statistics Canada advisory committees, the Prices Advisory Committee, starting in 1983 and continuing to the present. Wilk's decision to make me chair of the Prices Advisory Committee had some important implications for me later in life, as we shall see. Martin retired as Chief Statistician in 1985 but he continued to serve on various advisory committees such as the Services and the Science and Technology Committees, where I was also a member. He had very strong views on almost everything and we had some vigorous discussions about many issues. But he was basically a nice guy and I enjoyed arguing with him over the years.

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What else caused you to realize that official data were not always reliable?

On June 10, 1985, while I was visiting Stanford, I read an article in the San Francisco Chronicle on the growth of sales of those huge earth satellite dishes that were becoming popular around that time. Over the 5 years 1980–1985, the article gave the US sales of these dishes which were (in thousands), 4, 20, 60, 225, and 450. The article also gave the average prices of a dish for those years which were (in thousands of dollars), 40, 20, 10, 5, and 2. When I saw these figures, I thought to myself: I wonder if earth satellite dishes are in the US CPI? Of course, the answer was no and then I thought: I wonder how many other new goods either did not enter the CPI basket of products at all or until the price of the new product had dropped dramatically?

It was then that I realized that it was not an easy matter to calculate price indexes or to measure real output accurately. I recalled the comment by Zvi Griliches on my flexible functional form paper that I gave at Chicago in 1969; that the data did not support the estimation of all of the parameters in a flexible functional form. I realized that Zvi was right and from that time on, I devoted most of my research effort to improving economic measurement. I gave a talk on the new goods problem at Zvi's NBER Productivity Workshop on July 14, 1986 based on the Chronicle article. I produced several papers on this topic; see e.g., Diewert (1980, 1996, 1998).

It seems that in the middle to late 1980s your attention once again turned to the issue of finding flexible functional forms with nice properties.

Yes. It was possible to impose concavity on the Generalized Leontief and translog cost functions but the methods suggested in the literature for doing this were not satisfactory in that the flexibility of the functional form was destroyed. Terrence Wales and I addressed these problems in a series of papers; for example, Diewert and Wales (1985, 1995).

You have mentioned some joint work with Cathy Morrison, a UBC Ph.D. student of yours. Perhaps you could expand on this?

I regard Diewert and Morrison (1986) as a very important paper on how to measure Total Factor Productivity in a production theory context. Total Factor Productivity growth between two points in time for a production unit is generally measured as an output quantity index divided by the corresponding input quantity index. But this definition is not immediately connected to production theory and leaves open exactly how to choose the bilateral index number formula that measures aggregate output and input growth for the production unit. Thus Caves et al. (1982a, 1982b) addressed this problem in a satisfactory manner for constant returns to scale technologies using translog distance functions to describe the technologies at the two points in time and then finding an appropriate exact

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index number formula to describe productivity growth. However, as I mentioned earlier, this methodology cannot be applied to situations where the output aggregate includes intermediate inputs. In particular, if the output aggregate is the value added for an industry or the GDP for an economy, the CCD methodology cannot be applied since the output distance function is not well defined in this situation.

Diewert and Morrison solved this problem by making general translog assumptions on the value added or GDP functions for the production units at the two points in time. The end result is that we provided a strong justification, based on production theory, for measuring TFP growth as the implicit Törnqvist output index divided by the direct Törnqvist input index. We also showed how the overall Törnqvist input index could be decomposed exactly into a product of terms involving the input growth of each individual input and we developed a similar decomposition of the Törnqvist output price index into the product of terms involving the rate of price change for each individual price in the output aggregate. Diewert and Morrison (1986; 668) went on to apply this price change analysis to determine the effects on the GDP of an economy of changes in export and import prices.

We realized that an increase in export prices or a decrease in import prices, holding all else constant, should lead to an increase in "welfare" that is similar to a productivity improvement. I should note that Ulrich Kohli (1990) independently worked out the Diewert and Morrison translog measure of TFP growth but he took our analysis one step further: he rearranged the TFP growth measure that we both derived and he obtained a decomposition of nominal GDP growth of the production unit between the two periods into a product of explanatory factors, including output price inflation, productivity growth, and real input growth. Kohli (1978) also was one of first economists to assume that all exports and imports flowed through the production sector of an economy. Using this assumption, a large portion of trade theory could be analyzed in a production theory context instead of in a much more complicated general equilibrium context. Diewert and Morrison used this idea and I have used it many times in other papers; e.g., see Diewert (1983b, 1983c). I should mention that Ulrich was my second Ph.D. student.

Turning now to the 1990s, did you address any new measurement problems?

Yes, I ventured into a new area of research for me and that is to come up with tractable functional forms for consumer preferences over states of nature that are uncertain. I was inspired by a paper by Blackorby, Donaldson, and Davidson (1977) where they derived the class of preferences that are implied by the expected utility theorem by using separability arguments. In Diewert (1993b), I extended their separability approach to more general classes of preferences that could be characterized by implicitly separable preferences over states of nature. I applied this more general model to problems in modeling insurance, gambling, and investing. I followed up this paper with a more specific model that could be applied to insurance and gambling, Diewert (1995a).

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I believe that you first became acquainted with Marshall Reinsdorf in the early 1990s?

I first met Marshall on January 7, 1993 when I discussed his paper, "Seller Substitution Bias in the US Consumer Price Index" at the Anaheim meetings of the American Economic Association. Marshall compared US CPI components for gasoline and food products with corresponding average price series compiled by the Bureau of Labor Statistics (BLS) over the years 1980–1992. He found that the BLS CPI component series grew somewhere between 1.1% and 1.4% per year faster than their corresponding average price series over this period. These results indicated the possibility of a considerable amount of upward bias in the official US CPI, which is used for a wide variety of indexation purposes. In my discussion of the paper, I remarked that "this paper is the measurement paper of the decade". Eventually published as Reinsdorf (1998), it had a very large influence on the measurement community. It started a detailed study of how CPIs were constructed in practice and led to the Boskin Commission Report on bias in the US CPI in 1996; see Boskin, Dulberger, Gordon, Griliches, and Jorgenson (1996). The Boskin Report had implications around the world.

Marshall attributed his results to problems with the BLS procedures for aggregating specific price quotes at the first stage of aggregation where information on quantities is not available. A bilateral price index that does not make use of quantity information is called an elementary index. After discussing Marshall's paper in Anaheim, I got interested in elementary indexes and I soon wrote a discussion paper on the topic in January of 1995 which was published as Diewert (1995b). I followed up this paper with a couple of papers on bias in the CPI, Diewert (1996, 1998). In the first paper, I basically defended the Boskin Commission's estimates of the upward bias in the US CPI as being in the range 1.3–1.7% per year and in the second paper, I tried to provide an analytic framework for the numerical estimation of the various biases that might exist in consumer price indexes.

I would also like to praise Paul Armknecht, who was in charge of the Prices Division of the BLS at the time Marshall produced his research. Paul was brave enough to allow Marshall to present his results at the Anaheim meetings even though he knew there would be some substantial fallout from this decision. My discussion of Marshall's paper in 1993 led to a friendship with him.

The Ottawa Group on Price Indices got started around this time.

This is an interesting story. The United Nations Economic Commission for Europe and the International Labour Organization team up every two years and hold the Meeting of the Group of Experts on Consumer Price Indices in Geneva in May. These experts were members of national and international statistical agencies. During the 1994 meeting of this Group, three participants got together after a day of listening to country reports for a beer or two and they lamented the fact that the meetings did not have a very high research component at that time. These participants were Paul Armknecht, head of the Prices Division at the BLS,

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Bert Balk, a very well-known index number theorist at the Dutch Central Bureau of Statistics, and Bohdan Schultz, head of price research at Statistics Canada, and a founding father for the GEKS method for making international comparisons. Around this time, UN City Groups were forming. These City Groups were set up by national and international statistical agencies to have periodic meetings which would present research papers on an area of economic statistics. The idea was that national statistical agencies face similar measurement problems but have limited research resources, and these City Group meetings offer an efficient way of transmitting advances in economic measurement across a wider audience. Essentially, the research efforts of individual statistical agencies could be pooled by these meetings. The first of these UN sponsored City Groups was the Voorburg Group on Services Statistics which met in Voorburg for the first time in 1987. In any case, Armknecht, Balk, and Schultz thought that it would be a good idea to form a UN City Group on Price Indices. Bohdan went back to Statistics Canada after the Geneva meeting and convinced Jacob Ryten, the Deputy Chief Statistician, to form the Ottawa Group, or more formally, the UN International Working Group on Price Indices). The first meeting of this Group was held in Ottawa, October 31 to November 4, 1994. Because I was the Chair of the Statistics Canada Prices Advisory Committee, I was invited to this inaugural meeting. I was able to circulate Diewert (1995b) at this meeting and I believe this paper had some influence on John Astin, who attended the meeting and was the father of Eurostat's Harmonized Index of Consumer Prices (HICP).

The Ottawa Group will have its 15th meeting in 2017 and I have had the privilege of attending every meeting. Since 2007, the Ottawa Group has met every second year, alternating with meetings of the ILO/UNECE Experts on Consumer Price Indices, which I also attend). These two Groups have been tremendously influential in transmitting improvements in the measurement of prices across a much wider audience. Very few academics attend these meetings so I am honoured to be able to participate in these meetings and be accepted as a useful contributor. The decision of Martin Wilk to form a Prices Advisory Committee for Statistics Canada had far reaching consequences for my research.

Evidently, your participation in these groups led to your participation in a series of international price measurement manuals.

Yes, the ILO and UNECE realized that it was time to revise the existing international price index manuals in the light of new developments. The members of the Ottawa Group and other statistical agency experts were called upon to help draft new manuals on price indexes; see ILO/IMF/OECD/UNECE/Eurostat/The World Bank (2004a, 2004b, 2009). The *Consumer Price Index Manual: Theory and Practice* was the first of these Manuals (edited by Peter Hill) followed by the corresponding *Producer Price Index Manual* (edited by Paul Armknecht) and the *Export and Import Price Index Manual* (edited by Mick Silver). I wrote a large proportion of the theoretical parts of these manuals, with the help of the editors and Bert Balk, David Fenwick, Carsten Hansen, and others.

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In the CPI Manual, I showed that the four major approaches to index number theory (basket approaches, axiomatic or test approaches, stochastic approaches, and economic approaches) led to the same three bilateral index number formulae: the Fisher ideal index, the Törnqvist, and the Walsh index. The material that I wrote up in the manuals on these four approaches to index number theory drew on my earlier published work.

At the first meeting of the Ottawa Group in 1994, there was a major split among the participants. The participants from North America tended to favour the economic approach while the participants from Europe tended to favour the other approaches. But based on Diewert (1978b), I argued in the manuals that it did not matter so much which approach was chosen: each approach leads to the same three indexes which will numerically approximate each other to the second order around an equal price and quantity point. Eventually, this point of view was accepted and in later meetings of the Ottawa Group, we stopped arguing about which approach to index number theory should be chosen and focused on other measurement problems.

You made the above point in all three manuals, so you obviously thought there was a need to reiterate this in the different contexts of your theoretical chapters.

Yes, there was. But there was some new material that was developed in the *Producer Price Index Manual* that is important: basically, I showed that the traditional method for forming real estimates for outputs and intermediate inputs in an input–output framework did not lead to accurate estimates. The traditional method uses the *same* price index to deflate an entire row of value estimates for commodities produced or used by industries for a particular commodity class. But each commodity in the Input–Output (I–O) tables is actually an aggregate of hundreds if not thousands of individual products and the transactions in a given commodity class across two industries will have micro product weights that are specific to the bilateral transactions in that commodity class for the two industries under consideration. The construction of accurate real I–O tables requires more information on individual transactions than will be available to statistical agencies. The lesson here is that published real I–O tables are inherently unreliable using traditional methodology and hence industry productivity estimates that rely on these tables should be regarded with some caution. Diewert (2006b) addressed additional problems with Input–Output tables that are due to the treatment of tax and transportation margins. I also developed some new material on the problems caused by transfer prices in the *Export and Import Price Index Manual*. The difficulties associated with the estimation of transfer prices is a growing problem as globalization proceeds.

You also started to do some consulting in Australia during the mid-1990s.

Yes, Denis Lawrence was a former Ph.D. student of mine, and he really trained me to be a reasonably effective consultant. Starting in 1994, I would visit

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Denis in Canberra for a month or so every summer for many years and we would work on various consulting problems in three areas: (i) measuring the marginal excess burden of taxes using flexible functional form techniques; (ii) measuring the productivity of countries using superlative index number techniques and (iii) measuring the productivity of regulated firms as an aid to more effective regulation. I also wrote a number of papers with you starting from this period in three main areas: (i) the theory of regulation; (ii) measurement errors as an explanation for the productivity slowdown that occurred around the world starting in the mid-1970s and (iii) measuring productivity.

In addition to joining the Ottawa Group on Prices Measurement in 1994, you also joined another similar UN Group in 1997, namely the Canberra Group on Capital Measurement.

Yes, there were actually two separate Canberra Groups on capital measurement. Canberra I (officially the Expert Group on Capital Stock Statistics) had meetings over the years 1997–1999 and Canberra II (the Expert Group on the Measurement of Nonfinancial Assets) ran from 2003 to 2007. I attended all of the meetings of these two groups and I wrote a number of research papers as a result of my interactions with these Groups; see for example, Diewert and Schreyer (2008) on capital theory. These areas of research can be grouped into five main areas.

The first main area was concerned with the *measurement of depreciation*. My interest in this area actually started many years ago: in Diewert (1977, 1980), I developed a very general model of depreciation where the amount of depreciation that occurred over a time period depended on the intensity of use of the asset and other inputs which could offset depreciation of the asset. I later realized that my model was pretty closely related to an accounting framework which was proposed by Hicks (1961) and the accountants Edwards and Bell (1961). I spelled out the implications for the measurement of depreciation and the construction of user costs using this framework in Diewert (2010a).

The second capital measurement problem that I addressed as a result of working with the Canberra Groups was how to measure *inventory change* and the user cost of inventories in the Hicks, Edwards, and Bell accounting framework; see Diewert (2005a). In this paper, I noted that index number theory breaks down if the value aggregate can change sign between periods and this is clearly the case if the value aggregate is inventory change (or net exports). If we take inventory stocks at the beginning and end of the accounting period, these stocks can be deflated using normal index number theory and real inventory change can be measured as the difference between the deflated stocks. I note that the Bureau of Economic Analysis now uses this method for defining real inventory change.

The third capital measurement problem that I addressed during this period was how to measure the contribution of *R&D investments* in production. The Canberra Group II recommended introducing R&D investments as a productive asset into the international System of National Accounts. Prior to the 2008 version of the SNA, investments in R&D were immediately expensed which is clearly

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inappropriate since the benefits of a successful R&D project persist for many periods. However, it is also inappropriate to treat cumulated investment expenditures on an R&D project in the same manner as we treat investments in machinery and equipment and structures. The capital stocks that correspond to these durable inputs yield capital services which are inputs into a production function and can be varied over time. However, a successful R&D project generates a recipe or blueprint for either making a new commodity or for providing a more efficient method for making an old commodity. This R&D "input" cannot be varied over time; R&D capital stocks are very different animals from traditional reproducible capital stocks and require a different accounting framework.

Basically, a successful R&D project has a cost: the cumulated investment expenditures associated with it. Going forward, this cost is a fixed cost. The benefits of the project are the discounted stream of excess cash flows that the successful blueprint generates. Depreciation of the R&D asset in an accounting period is basically the loss of the excess cash flow that the project generated. This is very difficult to measure and so rather than tackle the difficult measurement issues associated with the fixed cost nature of R&D investments, national income accountants have simply made more or less arbitrary assumptions about R&D depreciation rates and treated R&D stocks in exactly the same manner as they would treat investments in trucks. Ning Huang (another former student of mine) and myself developed a rather complicated method for estimating R&D depreciation rates and more importantly, worked out how the System of National Accounts would have to be restructured to deal with R&D capital stocks in a more satisfactory manner; see Diewert and Huang (2011a, 2011b).

My fourth area of research into capital measurement is associated with the third area above and that is determining the implications for the measurement of capital services and depreciation when we have *sunk costs*; see Diewert (2009b) for the implications of machinery sunk costs and Diewert and Fox (2016b) for the implications of sunk costs in land and structures.

A fifth contribution to capital theory that I made around this time was to provide a solution to the *negative user cost problem*. The usual user cost of land is a financial opportunity cost of using the land for an accounting period; it is equal to the beginning of the period value of the land less the discounted expected, or actual, value of the land at the end of the period. Using actual end of period values of land almost always lead to negative user costs for at least some years over the longer run and even using expected end of period land prices leads to at least occasional negative user costs for land. This user cost for land is a financial cost of postponing selling the land for one period. But there is another way of valuing the cost of using the land for the accounting period under consideration and that is what one could rent the services of the land for during the period. This opportunity cost valuation for the services of land during the accounting period will always be nonnegative and typically will be positive. In Diewert (2011a), I suggested the *opportunity cost approach* to the valuation of land services, which sets the value of land services to the *maximum* of the rental price and the user

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cost of land. The resulting valuation for the services of owned land will always be nonnegative.

In 1999, you started attending the meetings of a new group that focused on economic measurement problems.

Yes, this was the Economic Measurement Group annual workshop, usually held in December which started at the University of New South Wales in 1999 under your leadership and grew from an initial attendance of six people to well over 100 experts from all over the world. I have attended 15 of the 16 meetings of this group. One of the most influential papers to come out of these workshops was Ivancic, Diewert, and Fox (2011), which introduced the Rolling Year GEKS method for addressing the chain drift problem which arises if scanner data and a chained superlative index are used in the construction of a CPI. Jan de Haan (2008) brought this problem to our attention during the 2008 workshop. These workshops presented many other influential measurement papers over the years.

You have also worked on the problems associated with quantifying the benefits of a favourable change in a country's terms of trade.

I started thinking about this problem in the 1980s but the real breakthrough came in 2005 when I did a research paper on this topic for the Bureau of Economic Analysis, Diewert (2005). The first basic idea in this paper runs as follows. I followed the example of Kohli (1978) and assumed that exports are produced by the production sector of the economy and that all imports into the economy are either used by the production sector or have some value added to them by transportation and retailing inputs. We can use the methodology developed by Diewert and Morrison (1986) and Kohli (1990) to decompose the growth of GDP over two periods into a product of three effects: (i) growth of net output prices; (ii) growth of primary inputs and (iii) technical progress. The growth effects on nominal income of changes in import and export prices can be precisely measured using the Diewert–Morrison–Kohli (DMK) methodology. Instead of using nominal prices, we could deflate these prices by an appropriate consumer price index for each period. Then we could use the same DMK methodology to get a decomposition of the real income generated by the production sector over the two periods into changes in real net output prices, growth of primary inputs and technical progress. These effects can be precisely measured. This was the first main idea in Diewert (2005b).

The second main idea was related to my work on the user cost of capital. The usual user cost of capital contains a depreciation term and this is quite appropriate if we are working in a GDP framework. But gross product cannot be consumed; depreciation should be deducted from the value of gross output if we want to have an estimate of sustainable income that could be spent on consumption. Thus in the second half of Diewert (2005b), I advocated moving to a net product framework.

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In addition to your work on "regular" index number theory, you have also published papers on the difference approach to index number theory.

In Diewert (2005c), I developed the test approach to determining the "best" functional form for the indicator of price change and found that the Bennet indicator was "best"; i.e., it was the difference approach counterpart to the Fisher ideal price index which was "best" using the ratio approach to index number theory. There is also an economic approach. In Diewert (1992) and Diewert and Mizobuchi (2009), we looked for flexible functional forms for the utility function such that we could identify Hicksian measures of price and utility change using observed data on prices and quantities for the two periods under consideration.

In recent years you have provided advice to the World Bank on their International Comparison Program (ICP). What is this program concerned with?

The World Bank has taken the lead in a worldwide partnership to collect comparable price and expenditure data on the components of GDP for most of the countries in the world. National price indexes cannot be compared across countries because they have different units of measurement. Thus the ICP collects prices of individual commodities in national currencies but in the same physical units of measurement that are comparable across countries. These comparable prices are aggregated into national price indexes called PPPs (Purchasing Power Parities). The PPPs are then used to deflate the components of a country's GDP into aggregate quantities (or volumes) that are, in principle, comparable across countries. Thus the ICP enables us to compare real GDP, and components of GDP, across countries. The ICP is a fundamental building block for the Penn World Tables which are widely used to compare real output and consumption across countries over time. The actual collection of the individual commodity prices has historically only taken place once every 5 to 10 years; the last two rounds of price collection took place in 2005 and 2011. The World Bank has set up a Technical Advisory Group (TAG) to provide methodological guidance for the construction of the PPPs. Because I had written papers on the properties of multilateral price indexes during the 1980s and 1990s (see Diewert (1999b)), I was invited to join the TAG for the 2005 round and initially, I was the Chair of the TAG for the 2011 round of the ICP. It proved to be too difficult to be the Chair since I was so far away from Washington D.C. and so I resigned as the Chair and was replaced by Paul McCarthy and Fred Vogel as joint Chairs. One very interesting methodological problem that we faced for the 2005 and 2011 ICP rounds was how to adapt existing multilateral index number theory (which treated each country in a perfectly symmetric manner) to deal with situations where a subgroup of countries (e.g., European Union Countries) first undertook a comparison of real GDP for all countries in the subgroup and then entered into a worldwide comparison of GDP by country with the restriction that the worldwide comparison be consistent with the subgroup comparison. This problem is of practical importance since some EU

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subsidy programs depend on the relative magnitude of the real GDPs of countries within the EU. Thus the EU wanted the European and worldwide relative estimates of GDP for EU countries to be consistent. I developed methodologies to deal with this consistency in aggregation problem as well as other ICP problems; see Diewert (2010b, 2010c). It was a pleasure to work with the members of the TAG; in particular, I would like to express my gratitude to Bettina Aten, Angus Deaton, Bert Balk, Yuri Dikhanov, Alan Heston, Robert Hill, Francette Koechlin, Paul McCarthy, Prasada Rao, Sergey Sergeev, Mick Silver, Marcel Timmer, Kim Zieschang, and Fred Vogel for helpful discussions and comments over the years. I should add that I was not always persuasive in my recommendations to this group: I was, and still am, very much in favour of using Robert Hill's (2001) spatial linking method for making international comparisons of real output across countries but the TAG favoured using GEKS as the primary method.

It seems that another area of research that you have contributed to in recent years is how to quality adjust prices using hedonic regression techniques.

A hedonic regression is a regression of prices of a product at a point in time on the quantities of its price determining characteristics. But there are many different ways that this basic methodology can be adapted to construct constant quality price indexes over time. I initially did not want to get involved in this area; I thought that I would leave the methodological problems in this area for experts in the area like Jack Triplett and Mick Silver to solve. However, I eventually got involved. In Diewert (2003a), I tried to look at a hedonic regression from the viewpoint of traditional consumer theory and find restrictions on preferences which would justify the usual hedonic regression approach. In Diewert (2003b), I took a somewhat systematic look at many of the unresolved issues in this area. In Diewert (2006a), I looked at the axiomatic properties of a two period time dummy hedonic regression model. In Diewert, Heravi, and Silver (2009), we did a detailed comparison of the time dummy and hedonic imputation methods for running hedonic regressions. Jan de Haan (2010) did an excellent follow up paper which cast more light on these issues.

In recent years, you and your co-authors have looked at a particular application of hedonics, namely its application to the construction of property price indexes.

In the last five or six years, I tried to address the problems associated with the lack of information in the national accounts of most countries on the price and quantity of land in the economy. This information on land is needed in order to estimate the Total Factor Productivity of industries and economies as well as to estimate rates of return on assets in the economy. There are also problems with our estimates of depreciation rates on structures in the national accounts. To address these problems, I suggested a simple hedonic regression model which I called the *builder's model*; see Diewert (2011a). When I was a university student many

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years ago, I used to work for my father, Ewald Diewert, where we built a house in Vancouver each summer. The value of the newly built house was equal to the cost of the land plot that the structure sits on plus the cost of construction. This simple observation can be turned into a hedonic regression model. The builder's model is also described in some detail in a Handbook that Eurostat commissioned to act as a guide for statistical agencies to construct house price indexes; see de Haan and Diewert (2011).

Since the turn of the century, you seem to have addressed a number of problems associated with productivity measurement. Could you elaborate on your efforts in this area?

In several papers I have complained a bit about the slow progress that national statistical agencies have made in improving our estimates of national and industry TFP growth; see Diewert (2008). The biggest problem is the lack of information on land used in production. Another problem is that the System of National accounts does not present enough detail on the incidence of taxes which makes it difficult to construct accurate user costs and to construct sectoral prices that producers face.

Here is a list of other productivity issues that I, and my coauthors, have attempted to address in recent years. (i) In the nonmarket sector of an economy, there are no prices to value the outputs produced by this sector and so national income accountants have measured output growth by the corresponding input growth. Using this methodology, TFP growth is automatically zero. In Diewert (2011b, 2012), I suggested several methods for dealing with this problem in a more satisfactory manner. (ii) If estimates of an industry's best practice technology are available, then the TFP growth of a firm in that industry can be decomposed into a product of explanatory factors such as increases in the firm's technical efficiency, returns to scale and technical progress. In Diewert (2014a), I provided a similar decomposition under the assumption that an estimate of the industry's best practice cost function is available. (iii) In Diewert and Fox (2017), we provided a general framework for the decomposition of productivity growth into explanatory factors making weaker assumptions on the reference best practice technology; i.e., we assumed that the reference technology satisfied free disposability rather than the usual convexity assumption. (iv) In Diewert (2015a), I showed how economy wide TFP growth, or Labour Productivity Growth, could be decomposed into industry contribution factors. (v) In the productivity literature, it has long been known that estimated TFP growth using gross output as the output concept is much smaller than estimated TFP growth using value added as the output concept. In Diewert (2015b), I showed the exact relationship between these two ways of measuring TFP growth if Laspeyres, Paasche, or Fisher indexes are used to aggregate outputs and inputs. (vi) Finally, Diewert, and Wei (2017) suggested that geometric depreciation is not an appropriate depreciation model for computers, rather one hoss shay depreciation (or a more general model of depreciation) is more appropriate. We illustrated the difference between our suggested model and the geometric model using Australian data.

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Another research area that you have looked at recently is the measurement of financial services in the System of National Accounts.

Yes, this is a very difficult area where there is little agreement in the literature on how to proceed. Diewert, Fixler, and Zieschang (2012) took a user cost approach to the problems associated with measuring the outputs and inputs of banks and illustrated how alternative treatments of inputs and outputs led to different measures of bank output using US data on the banking sector. In Diewert (2014b), I used a similar user cost approach to financial transactions to provide a framework for integrating a firm's financial transactions with its "real" production decisions. There are still many issues to be resolved in this area.

Any final words?

I would like to thank all of my coauthors over the years (including the ones that we did not discuss in this interview): it has been a pleasure working with you over the years! I would also like to thank my wife, Virginia Diewert, for her support over all the years that we have been married.

NOTES

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