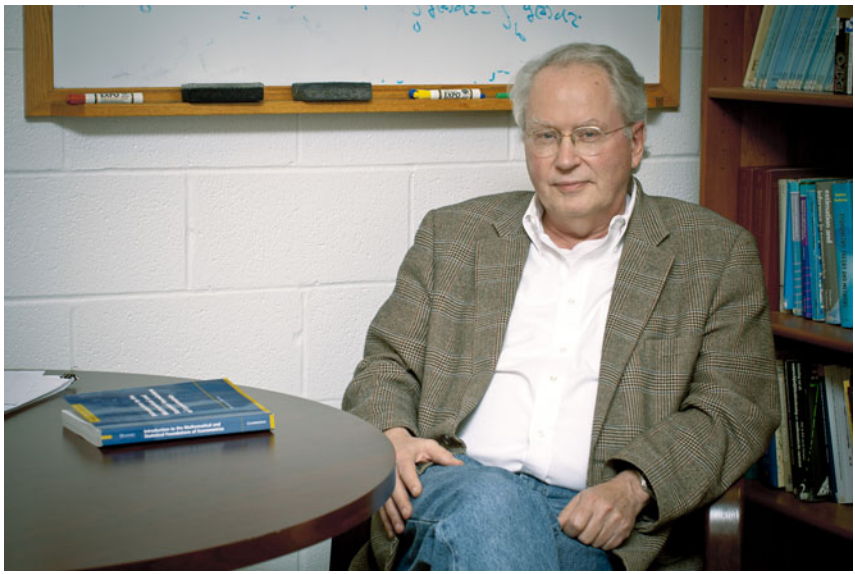
Econometric Theory, **29**, 2013, 590–608.
doi:10.1017/S0266466612000497

THE ET INTERVIEW: HERMAN BIERENS

Interviewed by Joris Pinkse
The Pennsylvania State University



Herman Bierens

The academic career of Herman Bierens spans 32 years, the latter half of which he spent at the Pennsylvania State University (Penn State). He has made several important contributions to various parts of the literature, including misspecification testing, semi-nonparametric estimation, unit roots, cointegration, and duration models. He has produced three monographs, including a rigorous first-year graduate textbook on probability and statistics, a software package, has served as the primary thesis adviser of nine Ph.D. students, was an associate editor of *Econometrica* and the *Journal of Econometrics* (*JoE*), served on two other editorial boards, is a fellow of both the Econometric Society and the *JoE*, and is a recipient of the *Econometric Theory* (ET) Plura Scripsit award and the *JoE* associate editor award. The full curriculum vitae of Herman Bierens is available at http://econ.la.psu.edu/∼hbierens/CV.PDF.

© Cambridge University Press 2012

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

Herman was born on January 7, 1943, in Apeldoorn, the Netherlands, where he lived until he moved to Amsterdam. There, he married and had two sons (Rowin and Laurens). He was trained to be a high school mathematics teacher before receiving his B.A. in econometrics at the University of Amsterdam in 1969 and his M.A. in 1972. He received his Ph.D. at the University of Amsterdam (UvA) in 1980 under the supervision of Chris Sims.

He worked for a while in the microeconomics department at the UvA before joining the Free University of Amsterdam (VU), then Southern Methodist University (SMU), and ultimately Penn State, where he retired in the summer of 2012.

Your career trajectory is quite unusual. You received your Ph.D. when you were 38 and your undergraduate degree in econometrics when you were 26. What did you do before you received your undergraduate degree?

It is a long story. Let me start with my high school. It took me 7 years instead of the normal 5 years to complete high school. I had to repeat two classes because I was really lazy. After high school I had to do my military service for 18 months. I never had a rank higher than private because I was unsuitable for the military. When I came out, I didn't know what to do except that I didn't want to live with my parents in Apeldoorn. So I moved to Amsterdam to work as a bank clerk. There I met a beautiful Euro Asian girl, Cherie. We fell in love, she got pregnant, and so we got married. I was now a bank employee with a family and an income of 300 guilders a month, about $125 in 1965 dollars, and we could not live on that.

Therefore, I decided to get schooling for a better job. I couldn't do a regular university degree, because that took 6 years; 3 years to get a "kandidaats" (B.A.) degree and another 3 years to get a "doctoral" (M.A.) degree. That was too long. So I had to choose something that was short and allowed me to start working quite quickly. I chose mathematics to become a high school mathematics teacher. The program was 4 years; 2 years to teach in junior high school (A certification) and another 2 years to teach in senior high school (B certification).

My parents gave me an allowance to help me do that, but for the rest both my wife and I worked. I was a janitor at a department store but was fired after two weeks because I was wiping the floors too slowly while thinking about mathematics. I had a night job in a wallpaper factory cleaning machines. My colleagues worked very hard so that they could sleep on the job for a few hours. When they went to sleep at midnight I would take out my lecture notes and study calculus until the morning. I did that for a couple of weeks but had to quit because I didn't get any sleep. I also worked for a while in a mail distribution center sorting mail. After that job my wife worked for a while in a hair restoration salon. Then I got a job as a civil servant at the bureau of land management of the city of Amsterdam punching punch cards

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

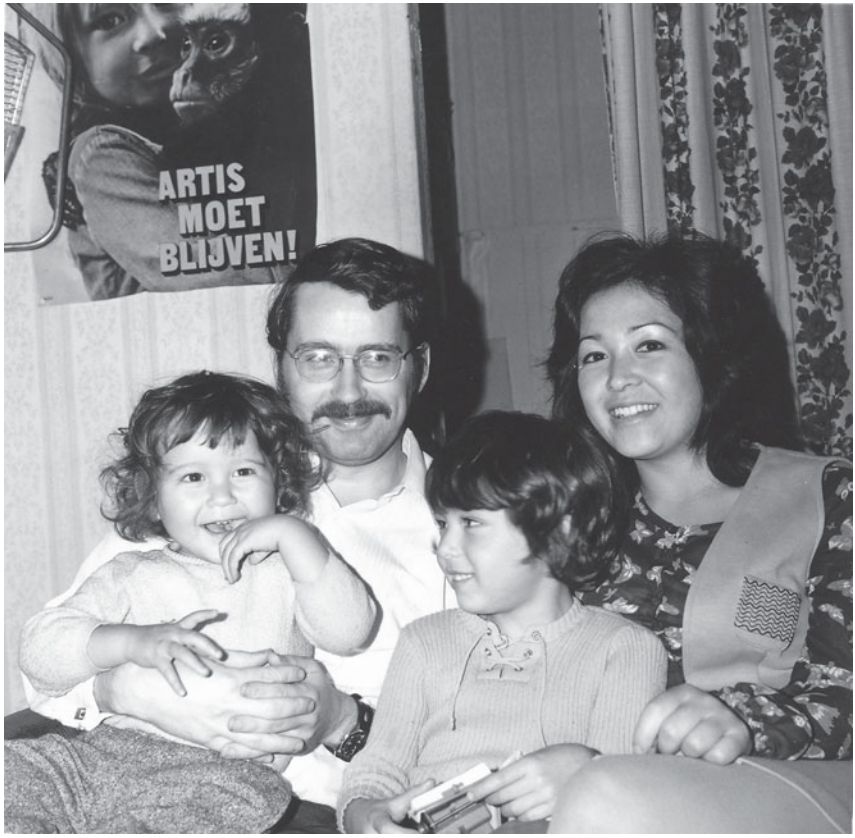


FIGURE 1. Herman Bierens with his wife, Cherie Landegent, and his sons, Rowin and Laurens. The poster says "Artis (the Amsterdam zoo) should stay!"

At the time I was finishing my A certification. A job became available in a high school as a substitute teacher in mathematics. I took the job and did it for 20 months. I was good at teaching high school. I was also quite good at keeping order; that's important. I initially liked teaching, but then I realized that I liked mathematics more than the kids, which is not the best attitude for a high school teacher. So I decided to pursue another line of work.

In Amsterdam there was an opportunity to study econometrics, which was taught by instructors from the mathematics faculty, the economics faculty, and the interfaculty where econometrics, operations research, and mathematical economics were concentrated. The reason for choosing econometrics was that I could transfer my mathematics credits to the econometrics program. So I only had to do the economics and statistics courses. The only problem was that I was still teaching 32 hours per week so I could not attend classes. However, all the economics courses had textbooks, and these textbooks had mathematical appendices where

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

the whole theory was spelled out in about 20 pages, so I only read the appendices without going to class. The statistics professor did not use a textbook; you had to take class notes. Therefore I asked him if I could study a textbook by myself instead of taking classes and then take an oral exam. In those days that was feasible. It took me 13 months to get my B.A.

At that point I got an assistantship in the interfaculty, which paid me enough to support my family, and so that I didn't need to do jobs on the side. I did the M.A. in 2 years instead of the standard 3 years. After graduation I needed another job. SEO (Foundation for Economic Research) offered me a position as a researcher doing contract research.

My first project was called "Transportation to the construction site." My supervisor told me this was a failed project and that I had to finish it in such a way that SEO could be paid. So I took the data that I had and fitted my first regression, a Cobb–Douglas production function, by hand using a mechanical calculator. This made me the econometrics expert at SEO. My later projects were mostly in the area of local governments. I did a project, for example, to predict the financial consequences of building a new town near big cities, in particular Capelle aan de IJssel, a suburb of Rotterdam. They had big budget deficits. They claimed it was because of their specific situation as a new town. So we collected data on similarly sized cities, ran regressions excluding Cappelle aan de IJssel, and then predicted what other cities would spend if they were in the same situation. On the basis of that they got extra money, so it was quite successful. Other projects came up in the area of local government.

Later I became a project leader, and SEO also gave me some time for independent research. I wanted to work something out that could result in a Ph.D. thesis. My interest at the time, in the early '70s, was on stagflation. I built a model in which stagflation was explained by inventories. The model was pretty good, but I had no idea how to estimate it in a proper way. Instead, I was calibrating it. That led me to look at the theory of nonlinear estimation, which became my main research project. I worked on it in my spare time without Ph.D. supervision. I finished the first draft, entitled "Robust methods and asymptotic theory in nonlinear econometrics," in the late '70s. I managed to apply some of this work in paid projects for the city of Amsterdam. However, in the last project that I did for them it was explicitly stated that they did not want to see any robust methods anymore.

You received your Ph.D. [2] from UvA, yet your thesis adviser was Chris Sims, who was employed by the University of Minnesota. How did that happen?

UvA had a deal with the Erasmus University of Rotterdam to send their working papers around the world. One of my papers landed on the desk of Chris Sims, and he wrote me a nice letter with comments. I went to Mars Cramer, who was my econometrics professor at UvA, and asked him to be my thesis adviser. He looked at what I had and said it was way over his head, but this guy in Minnesota wrote

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press



FIGURE 2. Chris Sims (standing on left) addressing Herman Bierens, who is standing between his "supporter" René Stofberg and his then wife Cherie Landegent. Seated next to Landegent is Herman's father.

you this letter and he seems to know what you are doing, so why don't we ask him? Cramer contacted Chris Sims and said that nobody here knows what this guy is doing, would you be willing to be his adviser? Chris agreed to do that. I sent him my draft thesis and got it back 8 months later, with his scribbles in the margins.

I met Chris for the first time at a conference in Warwick a few months before my graduation and had a 1-hour conversation with him. UvA flew Chris Sims and his wife over for the thesis defense and graduation ceremony; that was the second time I met Chris.

After my graduation I applied for a travel grant from the Dutch government to spend some time in Minnesota in the fall of 1982. I didn't work with Chris, but spending time in a different environment was inspiring. In the late fall everything became frozen tight in Minneapolis so I could not get out of my apartment. To kill time I started to work on econometrics problems. My thesis was on robust estimation, robustness to outliers, but I realized that outliers could also be due to misspecification. How do you test for that? There was nothing available at the time. I started to think about that and realized that conditional expectations are uniquely related to their Fourier transforms. In particular, a conditional expectation is zero if and only if its Fourier transform is zero. I realized that you can characterize this deviation from zero in terms of sets. If the conditional expectation is zero then the set is the whole euclidean space, and if the probability that the conditional expectation is zero is less than one then the corresponding set of nuisance parameters is almost empty; it has Lebesgue measure zero. This was the basis for my first paper on consistent model specification testing. I showed Chris my handwritten draft, and his first response was that this is not possible;

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

you spread the power so thinly over all alternatives that there is no power left. Well, that proved to be wrong.

At the end of my term as a postdoc in Minnesota, Chris Sims arranged some seminars. I went to the University of California, San Diego (UCSD), and met Hal White and Ian Domowitz for the first time. Ian was at that time finishing his Ph.D. with Hal White. I stayed at Hal's house because UCSD had no money to put me up in a hotel. I went to Berkeley and stayed at Paul Ruud's house, because Berkeley also didn't have any money to put me up in a hotel. Then I went to the East Coast to Harvard, MIT, and Yale. When Chris Sims wrote to Yale he may have given the impression, I think, that I was on the market because when I stepped out of the taxi at the Cowles Foundation the first thing that I had to do was to talk to the dean. The dean told me that I was too old for an assistant professorship, but what they could offer me was an associate professorship without tenure. He asked me, "What do you want to earn?" I was not prepared for that question, so I made a quick calculation using my Dutch salary, expenses, and the exchange rate and said I needed $35,000 a year. That was the end of the conversation, because the dean said, "We never pay our associates more than $25,000." Then he asked me if I knew somebody. I just came from Berkeley, and I had attended Don Andrews' thesis presentation, which impressed me. So I suggested that Yale should try him. That worked out quite well.

When I came back to SEO, to Amsterdam, I was no longer interested in contract research. There were always constraints of time and the budget and my projects were always over budget, so I wanted a job in my area of interest, econometrics. A position for an assistant professorship at the interfaculty became available, but I was passed over because I was considered too theoretical. Actually, this was a blessing in disguise because an associate professorship became available in another part of the faculty of economics, in microeconomics. I had no idea about microeconomics, so during the summer break I took the textbook with me on my boat, sailing, and while I parked in harbors and on the northern sands, I read the books. At the end of the 2-week sailing holiday, I was up to date. I wrote two papers [22, 21] with Joop Hartog, who was the labor economist there. One was published in the *JoE*, the other one in another journal. I learned a lot, and my micro knowledge came in handy later on at SMU.

After a few years as an associate professor of microeconomics I finally got an appointment as full professor of econometrics, at VU.

During the first half of your academic career you had stints at UvA, VU, and SMU, but the second half you spent entirely at Penn State. What made you find a home at Penn State?

In 1987 I got a visiting professorship at UCSD to teach two courses. I enjoyed that a lot, so when I came back to Amsterdam I started working on another visiting appointment. At a conference in Munich I met Essie Maasoumi, who arranged for me to visit SMU. In 3 weeks I had an offer in Dallas at SMU. There was no

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

teaching, just being there doing research for 5 months, at an annual salary that was much more than I earned in Amsterdam. So I spent 5 months at SMU in the spring of 1990. They treated me very well. They were trying to recruit me, but I was reluctant. They told me they could give me a part-time position, an endowed chair, for half the year taking unpaid leave from Amsterdam. I arranged that, came back the next year. At that point my marriage broke up, and I decided to stay in Dallas. So I ended up at SMU.

Initially it went very well. The dean was very supportive. He was a well-known statistician. He provided extra funds for visitors. My research budget was the same as the one I shared with 25 other people at the Free University. I had my own secretary. It was quite luxurious. But after a couple of years things went downhill at SMU. SMU got expelled from the football league for making side payments to the players. Enrollments went down, and endowments dried up. Then I realized how important the football program was for universities in the U.S., especially private universities.

Then the environment became more macro- and calibration-oriented and less friendly for econometrics. I ended up being lent out to the statistics department to teach Billingsley's *Probability and Measure* [29]. At the time I wasn't very happy about it because it was a lot of work, but I'm grateful for that now since it was the basis for my textbook.

I got a phone call from Bob Marshall, who said: "Herman, you don't know me. I'm the head of the econ department at Penn State. Would you be willing to be considered for a position?" I went over to give a job seminar at Penn State, got a nice offer, so I decided to take that job. This was the beginning of the buildup of the econ department at Penn State. The first external hire was Eric Ghysels. He was my colleague. The people already there were very nice, welcoming; I felt appreciated. It was the first time I didn't feel I needed to watch my back. It was a refreshing situation. I also got involved in recruiting. We recruited quite a few senior people after that. Joris was of course a relief because at that time I was the only econometrician left. Bob was quite worried, and so was I. At Penn State people always put the department first, not just look out for their own fields. That's the reason I've stayed here for more than 15 years. I've never had the urge to look for outside offers. I've never liked the system in which you have to pretend to be interested in outside offers.

Probably your best-known work relates to model specification testing, starting with your 1982 paper in the *JoE* [4], including two papers [8, 24] in *Econometrica* in 1990 and 1997 (the latter with Werner Ploberger), and continuing until the present day with a joint paper [27] with Li Wang in ET. What do you see as the reason for the success you have had with this work?

In the '82 paper on the integrated conditional moment (ICM) test I had worked out the test itself, but I couldn't figure out its null distribution. Therefore, upper

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

bounds for the critical values were based on the Chebyshev inequality. But it was the first consistent model specification test. The statistics literature in that area started 15 years later, with a paper by Stute [43], who used a similar device but instead of Fourier transforms he used a weighting scheme of the residuals that was based on indicator functions.

In econometrics, the first papers in that area came after my 1990 paper [8]. They were based on a comparison of nonparametric regression estimates with the parametric ones. The problem with these tests is that the rate of convergence of the test statistic is lower than the standard (parametric) rate because of the bandwidth. In nonparametric regression the bandwidth is going to be dominant. In these tests everything depends on the bandwidth, and the higher the dimension, the lower the rate of convergence. That becomes much clearer in the case of local alternatives. The rate of convergence to the null is much slower for tests based on the comparison of parametric and nonparametric regression models.

In the 1990 paper I figured out what the limiting distribution was of my test. After the 1982 paper I was still working on that problem, and then I came across a little book by Billingsley [28], and there it was: you had to prove tightness and prove that the finite distributions converge, and that's it. Still, I didn't know how to derive critical values because the test is not pivotal; the asymptotic null distribution depends on the distribution of the covariates. But at least I had a proof of tightness and that under the null the empirical process involved converges to a zero mean Gaussian process.

Then I talked to Werner (Ploberger) and he told me that he could prove that the ICM test, my integrated conditional moment test, was admissible, i.e., there is no uniformly more powerful test, and also that the local power was $\sqrt{n}$. It was not a conclusion that was accepted in the community. Werner told me he'd been told it was impossible. We started working on that while I was at SMU. Werner was working on the admissibility. My part of the project was to write everything down and to come up with upper bounds of the critical values that are independent of the model and the covariates. The paper is to a large extent inspired by the ideas of Werner.

I'm not sure whether this is really popular at the moment, because the line of research on consistent model specification testing based on kernels went further and further. My papers were mentioned, but mostly as an alternative rather than building upon. There are only a few people who have built upon my test. In particular, Stinchcombe and White [41] have shown that the approach in my 1990 paper carries over to a large class of nonpolynomial weight functions, and (Juan Carlos) Escanciano [31] has combined the ideas of Stute and mine. Another is my student Li Wang. I suggested to her that the ICM principle could also be used to test the correctness of parametric conditional distribution models, and that if she wanted to write a Ph.D. thesis on this topic I could help her with that. That's what Li did. In the process my interest in model specification testing woke up, so it became a joint project and publication in *ET*. This test works really well. We applied the test to a conditional Poisson model on the basis of health economics

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

data. The test strongly rejects the conditional Poisson model, a popular model for that type of data.

Your work spans quite a broad array of topics, including semi-nonparametric estimation, forecasting, autoregressive moving average (ARMA) models, cointegration, duration models, and even auctions. What led you to diversify so much?

After writing a couple of papers—I had two papers on model misspecification testing in the *JoE*, one for the independent and identically distributed case and one for time series [4, 6]—I got bored, so I wanted to move on to something else. I started to work on nonparametric estimation, on kernel regression. The way I work is that I don't worry very much about what other people have written. As a result, I usually have to throw away half of what I have done and what is left over is my new stuff. Initially I wrote a paper on kernel regression, reinventing the wheel, but I also extended it to time series, and that part got published in *JASA* [5]. The rest had already been done. Then I got an invitation to speak at the World Congress in 1985 in Boston. That invitation was based on my thesis, so they were expecting a talk on robust estimation. But I was done with that, so I presented a paper [7] on nonparametric estimation where I used jackknifing and higher order kernels to remove the bias and to speed up the rate of convergence to a rate arbitrarily close to $\sqrt{n}$. That got some interest, because it allowed other people to do things. The paper by Powell, Stock, and Stoker [40] on average derivatives uses this device to get a sufficient speed of nonparametric convergence.

After that I lost interest in nonparametric regression. I got bored, so I moved on to another topic, and that was time series. I had a student at the Free University, my first Ph.D. student. I put him on a project on autoregressiving moving average with exogenous inputs (ARMAX) modeling with applications, and that paper [18] got published in *Econometric Reviews* in 1993. Then I got interested in unit roots because that was fashionable at the time because of the work of Peter Phillips [37, 38, 39], so I wrote a couple of papers [9, 11, 12] on unit roots and cointegration. But at the end of the '90s I lost my interest in time series.

One of my last papers [13] on time series was a weird one; it was about complex unit roots. There was very little literature on that. I was interested in whether business cycles could be explained by complex unit roots, because complex unit roots give you a regular cycle. I wrote a paper on that. I was also concerned at the time about the direction of macro, where real business cycles (RBC) models were calibrated. They're not business cycle models at all. Therefore, the title of that paper, "Complex unit roots and business cycles: are they real?," had an intentional double meaning.

I got interested in RBC models and especially whether or not to calibrate. I was invited to write a paper for a conference on the interface of economic theory and econometrics with a particular focus on these theoretical models and why people don't estimate them. I started to think about the difference between a theoretical

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

economic model and an econometric model. Theorists use the ceteris paribus condition, holding everything else constant, while econometricians integrate things out. If you estimate these models econometrically then you get misspecification because it's a different way of conditioning. The paper involved, joint with Norman Swanson, was published in *JoE* [26].

I was disturbed by this calibration business. For instance, I interviewed people for junior jobs, not here but at SMU, and there was a guy from Rochester. I asked him how he had estimated his model. He said he didn't estimate, but calibrated. I asked him how many parameters he had calibrated; he said 50. I then asked him how long his time series was. He said 50. He didn't understand that there was something wrong with this. People in theory calibrate the model in such a way that the number of parameters is the same as the number of moment conditions so there is no way to verify whether the model is correct or not, because you don't have overidentifying restrictions. I was thinking about ways of estimating these models keeping the basic idea of calibration, which is that all models are wrong and therefore there is no reason to estimate them, which is their excuse for not doing econometrics.

I was thinking about the literature on indirect inference. There you assume that the theoretical model is correct, and you generate artificial data from it. Then you use an artificial model to compare the parameters of that model using the real data and the generated data and you match them, getting them as close as possible. I thought what if we reversed that order and used an unrestricted vector autoregression (VAR) as a representation of the data generating process? If you do indirect inference then you can match the structural parameters, the deep parameters, to the parameters of the VAR. Then on the basis of the delta method you can endow these parameters with confidence intervals. That was my next project in that area, which took 5 years to get published in the *JoE* [15].

I took a textbook RBC model and I matched the deep parameters with an unrestricted VAR model. The result was quite interesting. All the dynamics disappeared; the depreciation rate became really close to one, and its confidence interval included one. All the dynamics were gone. So the data suggested that the best model for the U.S. data was a sort of Robinson Crusoe type of economy, where Robinson has to decide how many potatoes to plant and how many to eat. That showed how ridiculous these models are.

After that paper I was done with RBC models, except that I suggested to one of my students, Hung-Pin Lai, that this might be a good topic for her research. You could use the same technique if the process was a cointegrated error correction model, which is more common than an unrestricted VAR. She wrote a thesis on that. It didn't get published, but she has some other papers that are related to that.

In the early 2000s there is a gap in my CV, and the reason is that I wrote that textbook, *Introduction to the Mathematical and Statistical Foundations of Econometrics* [14], based on lectures notes that I used in a Ph.D. course at Penn State. It took me 4 years to complete that. I didn't really write any papers, except that the last RBC paper was conceived in the early 2000s.

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

Then one day in 2001 José Carvalho came into my office. He had been working with an assistant professor in the department who was leaving. He had no supervisor anymore, so I took him on. José had two data sets on crime, but no paper, nothing else. He had lost a full year. José was funded by the Brazilian government, and his funding was going to expire in a year. I put him on a short leash. The data were duration data, and given the time constraint we had to do everything parametric, standard duration stuff. He had a data set on two types of crimes: misdemeanors and felonies. It was censored of course. This was something Bo Honoré had written about with Jim Heckman [36]. It looked like we had a competing risk model, and we did everything simple, with Weibull baseline hazards and a gamma distribution for the unobserved heterogeneity. The other data set also involved durations and recidivism, but it involved treatment type effects. They were data on a program that provided extra help after people were released from prison for people in the treatment group. All was conditional on finding a job.

José programmed everything in EasyReg, which is programmable, although it's very inconvenient to do so, but it works. We got the first results, and they looked pretty good. For the other paper he did the same, treating job search and recidivism as competing risks, which with hindsight proved to be not quite correct. José succeeded in graduating on time. We sent the competing risks paper to the *Journal of Applied Econometrics* (JAE) and received favorable referee reports. However, the co-editor wrote to us that the journal now also required an econometric innovation. We were allowed to revise and resubmit, but we had to do something more fancy than a purely parametric modeling exercise. What can you do more? I knew from the paper of Honoré and Heckman that the model was nonparametrically identified. So instead of using the gamma distribution I wanted to allow any distribution. I started to work on that and got intrigued by applications of Hilbert space theory to functional approximations. I started working on that topic by myself using a different approach than the standard Hermite expansions advocated by Gallant, Nychka, and Tauchen [33, 34], using Legendre polynomials on the unit interval. Any distribution function can be converted to a distribution function on the unit interval by linking them via an a priori chosen distribution function, which has a dual role. It is not only used for the transformation, but it also gives the unknown distribution function some backbone. It's like a prior. Using distribution functions on the unit interval opened up more possibilities for series expansions of density functions and distribution functions.

I was writing that paper for the interval censored case (corresponding to the other paper [20] that José and I had). It was published in *ET* [16]. We used that technique in the application for the *JAE*, and I also managed to squeeze in an ICM test for the correctness of the Weibull baseline hazards. It became a nice paper [19], an empirical application with theoretical innovations as required by the *JAE*. That is how I got interested in semi-nonparametric econometrics.

I could only derive consistency results for sieve estimators at the time. I was aware of some asymptotic normality results, but I was under the impression that most results were assumed rather than proved because the conditions were geared

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

toward the results. I started thinking about that, which resulted in my last paper on asymptotic normality of sieve estimators under weak and verifiable conditions. In that paper [17] I derive the asymptotic normality in a completely different way.

Meanwhile, I had a little interlude of cointegration, and that came about as follows. Luis Martins wrote a thesis on time-varying cointegration. We sent the joint paper to *ET*, which got good reviews but one of the referees didn't understand a certain proof. In going over the proofs I noticed that the proof of the main result was wrong. I felt guilty because I didn't check the proof carefully. Therefore, I took it upon myself to fix the problem. I had to postpone all my research and go back to the books to refresh my knowledge on cointegration. I did that by writing 60 pages of lecture notes on the theory of cointegration, and then I spent another $1\frac{1}{2}$ years fixing the proof. The main result appeared to be correct: only the proof was wrong. That resulted in a joint paper [23] in *ET*.

Because I had to postpone my research on my real interest, semi-nonparametric econometrics, for almost 2 years, I got really frustrated with time series, so I decided that this was my last project on time series ever.

I usually get bored after writing two or three papers about a topic. However, changing topics many times is like writing a Ph.D. thesis over and over again. This is of course not good for getting a good quantity of publications because you start from scratch every time. You don't have the benefit of the previous experience of working on an area, but I think it's better than working on your Ph.D. thesis for your whole academic life. Some people do that, but you have to go beyond that. Don't work too long in the same area as your Ph.D. thesis; move on.

If you were a graduate student now, what area of econometrics would you write your thesis on?

On semi-nonparametric econometrics. I have enough material now to write a 150–200 page thesis. Thus, I'm already done with a thesis in that area.

What do you view as your greatest academic accomplishment?

I consider my last paper on semi-nonparametric econometrics to be one of my best papers. However, I am best known for my work on model misspecification testing. The recent paper with Li Wang is different from the previous work in that it tests the complete conditional distribution function instead of a regression model. There are only a few papers that do this, including Andrews' conditional Kolmogorov test [1], and Zheng's test [45] based on kernel density estimation. What Andrews did is compare theoretical empirical distribution functions with empirical ones. Since conditional distribution functions are completely determined by their conditional characteristic function we thought we can do the same. I suggested that idea to Li; she worked it out. That became a nice paper [27] in *ET*.

In the paper with Werner (Ploberger) [24] we worked out the limiting distribution of the ICM test statistic for real-valued weight functions. It turned out to

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

be a weighted sum of χ_1^2's. In the paper with Li we found out that because with complex numbers you have a real and an imaginary part, the results carry over but that now the limiting distribution is a weighted sum of χ_2^2's. Since the same idea was used in my 1982 paper [4], I now know what the limiting distribution of the test proposed in that paper looks like. But somebody else has to pick it up.

You wrote a software package, EasyReg, and three monographs, including a first-year graduate probability and statistics text. What motivated you to invest your energy in these endeavors?

In the '90s I was working on nonparametric cointegration, and I saw that Søren Johansen and Katerina Juselius had written a Rats plugin called Cats. I saw all these applied people using that package without knowing the theory. I thought that if I wrote my own software package for my nonparametric cointegration test and I make it available for free then people are going to use that and that will boost my citations.

I then realized I had to do more to get people to use that. It's like if you're selling dishwasher detergent in either a colorful box or a gray bag. Even if the stuff in the gray box is the same and cheaper people will often choose the colorful box. So I had to provide something more than just my nonparametric test of cointegration. I added the Johansen approach, my own cointegration test, some simple stuff like linear regression. I wrote it for DOS and made it available. In the mid-'90s I bought Visual Basic and could now give EasyReg a Windows interface. It became a hobby to add stuff to it, so now it is a complete software package like Eviews. Of course Eviews is a professional package, and its graphics are much better than those in EasyReg. All graphics in EasyReg are homemade. On the other hand, EasyReg has options that Eviews does not have, for example, the Bierens–Martins test for time varying cointegration, the Breitung unit root test [30], and the ICM tests for conditional expectation and distribution models.

I have spent quite a lot of time developing EasyReg further. I'm going to make it open source after retirement. If I sell it my fear is that the buyer is only going to destroy it to get it off the market, and I've invested too much in it to have it destroyed. The only plan I have left is to improve the upgrading procedure, which doesn't work very well under Windows 7, and to build in my semi-nonparametric stuff. Then I'm done with EasyReg.

My first monograph was my Ph.D. thesis. After 5 years working on it I was fed up with robust estimation. I didn't want to work it out into different papers, so I offered the whole package [3] to Springer for their lecture note series.

The second book [10], *Topics in Advanced Econometrics*, has its origins in San Diego. I taught a graduate course at UCSD in the spring of '86. Hal White had just published his asymptotic theory book [44]. In that book measure theory is used, there is conditioning on a sigma algebra, so I thought let me teach that. There was no textbook available, so I made a whole bunch of handwritten lecture notes and gave copies to my students. That was the basis of my book. I included

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

a chapter on nonparametric econometrics based on my World Congress invited lecture. At the time I was also working on ARMA memory indices, which is a way of compressing the data from infinitely many dimensions to one dimension, which I also included just for fun. I had a chapter on unit roots, which I was working on at the time as well, and a brief explanation on cointegration. It was published by Cambridge University Press.

The last book was based on the first semester econometrics course in the Ph.D. program I taught during my first 5 or 6 years here at Penn State. When I was hired, Bob Marshall, the head, said he wanted me to strengthen the econometrics core and include some measure theory. That is what I did. Initially I used Ron Gallant's book [32] on theoretical econometrics. But measure theory was not sufficiently developed in that book; Borel measurability was not really treated. I therefore augmented Ron's book with my lecture notes, which grew so out of hand that in the end I had a new textbook. Cambridge University Press was interested in publishing it. But there were chapters that I had not written yet and there were appendices that had not been written yet, so it took two more years to polish everything and to add material to that book. It finally came out in 2004 [14]. It had many typos so when the hardcover sold out Cambridge allowed me to make corrections for the 2007 paperback version, which is better and cheaper than the hardcover. I hope that my colleagues are going to continue using this book.

You have always injected a degree of mathematical rigor into your instruction, even in the first-year Ph.D. econometrics courses. Why do all Ph.D. students need that level of rigor?

For various reasons. When you look at the current literature in econometrics, people are using measure theory. I think that it is important even for people who are not taking a field in econometrics to at least understand what it means. Secondly, in macro, the book on dynamic programming by Stokey, Lucas, and Prescott [42] is full of measure-theoretic concepts. Even in micro, when you're working with recursive systems and how to solve them, that's all measure theory and functional analysis. I hear from colleagues in theory that it's very good that students have been exposed to this material. It's also a good selection mechanism. If they do poorly in my course then they likely do poorly in other courses as well. So I think it is of fundamental importance for a good Ph.D. program to teach that stuff.

On April 28, 2012, a conference was held in your honor at Penn State. Virtually all your former graduate students were there, and they came from all over the world just for the conference. You visibly enjoy working with graduate students. What is the source of your enthusiasm?

Initially when I work with Ph.D. students I give them ideas based on my own research. As I told you before I have a short attention span. After 2 or 3 years I'm moving on to a new topic. But if a student is interested in an area I've written

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press



FIGURE 3. Participants of the conference in honor of Herman Bierens, April 28, 2012, at Penn State. Front row: Guang Guo, Xiaohong Chen, Joris Pinkse, Herman Bierens, Ron Gallant, Chris Sims, Sung Jae Jun, Saroj Bhattarai, Alex Fakos, and David Jinkins. Second row: Emily de Jong, Li Wang, Yanping Liu, Moon Jung Kim, Bruno Sultanum, Evelyn Nunes, Nail Kashaev, Jim Jordan, Paul Grieco, Felix Tintelnot, Werner Ploberger, Moohua Liao, Robin Sickles, Luis Martins, Hung-Pin Lai. Back row: Robert de Jong, José Carvalho, Hosin Song, Venky Venkateswaran, Quang Vuong, Jicheng Liu, Yuanyuan Wan, Adam Slawski, Kai Li, Shengyu Li, Huihui Li, Haiqing Xu, Nianqing Liu, John Riew, and Steve Guo.

on then of course I will do that. And if they work on that, which they don't always do, then my interest in the topic will reawaken. Then I jump in and work with them on extensions, proofs, etc. So in the end it's really a joint project.

Sometimes, like in the project with Li Wang, an old topic gets a new face and I get a new interest in it. Sometimes work I've done with a Ph.D. student inspires me for my own research as is the case in my work with José Carvalho on duration models. It put me on track for a new research area, semi-nonparametric modeling. I was pretty blank in this area and got fascinated about the possibilities and the potential of these techniques.

When I was working with Li on ICM tests I was also talking to Hosin Song, who was looking for a topic. I thought that the idea of matching empirical and theoretical characteristic functions can also be used in auction models. The most commonly used nonparametric approach to first-price auctions is that of Guerre, Perrigne, and Vuong [35]. But you can do it a different way. If you model the value distribution semi-nonparametrically then you can draw from that; you can generate artificial bids. Then you can match them via empirical characteristic functions with the observed bids. It worked beautifully [25]. In the last semester of my last econometrics field course I had three students replicate the results in that paper using different series expansions, cosine sequences, and Chebyshev polynomials, and amazingly with a few parameters you can almost exactly replicate the underlying value distribution by the simulated method of moments. So it works, which is intriguing.

I've always tried to apply the new techniques to simulated or actual data. It gives me satisfaction to see that what I have figured out, together with students,

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press



FIGURE 4. Herman Bierens with his former students: Luis Martins, Hung-Pin Lai, Hosin Song, Guang Guo, Li Wang, Robert de Jong, José Carvalho, and Steve Guo.

actually works. I'm not a purely theoretical econometrician anymore. I have done a lot of empirical work both at SEO and in the labor economics department at UvA. I have learned to base assumptions and conditions on real world examples. It is easy to assume all problems away; I've never liked that. I take great care in keeping the conditions as simple and as realistic as possible.

That is also the basis of my last paper [17], on the asymptotic normality of sieve estimators. I didn't like the conditions in the statistics and some of the econometrics literature. They are very high-level conditions, which are very difficult to verify. It's like a magician; you put the rabbit in the hat and in the end you pull it out and see: "Look, there is the rabbit." But the rabbit was already there, and that is sometimes the case with all these assumptions that econometricians and statisticians make. They make the assumptions to get the result they desire. But they don't work out whether these conditions are actually realistic and verifiable. By taking a completely different route I came up with conditions for asymptotic normality of sieve estimators that I could verify.

You are retiring from Penn State this summer (2012). Presumably you are going to spend more time on other pursuits, like sailing?

I've always had a passion for sailing. As soon as I could afford it I had sailboats.

Initially, in the Netherlands when I finally could afford a dinghy, it almost killed me. I had it in my backyard behind the IJsselmeer dike near Hoorn, the

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

Netherlands. It was sitting there to be used. I took it sailing with my brother-in-law. The dinghy was sold to me as unsinkable. Fortunately, I had bought a good pair of life jackets. So we set sail and it was quite windy (6 or 7 Beaufort). We were sailing over the waves on the big lake. People in the harbor of Hoorn were looking at us because we were going so fast. Suddenly the boat capsized. I knew the theory on how to turn it right side up; you hang on the centerboard until the boat flips back. That worked well, but the boat was full of water and when we got in, it made a hissing sound and like a submarine it went down.

We were floating in the lake about a mile from shore in our life vests. People in the marina had seen us go down, so the harbor master sent the first boat that came in back out to look for us. It took a while. Meanwhile, my brother-in-law passed out from hypothermia, so I had to keep his head above water. I was very calm, but I knew we might not survive this. They picked us up. A sailboat came back from the harbor, and he threw a line overboard and was going to pull us in. But the guy had his engine running, and I had this idea of the propeller cutting off my leg. I tried to tell him to turn off his engine, but he couldn't hear me. So I let go of the line and it got stuck in his propeller and it stopped the propeller. At that point I was willing to come on board. My brother-in-law had to be dragged on because he was unconscious. At the harbor the police and an ambulance were waiting. A policeman recommended that we take a stiff drink and a warm shower, so we did. We finished a bottle of my favorite liquor, Berenburger. The only problem is the day after you realize how close you came, and the aftershock was quite severe. That was my first sailing experience.

The next boat was a 17-foot cabin cruiser, which I sailed with my family on the IJsselmeer, which was a stupid mistake. I crossed over when there was a lot of wind, and the whole family was vomiting over the side. I was the only one having fun. Since then my family didn't want to go with me anymore, and I've been sailing solo ever since.

My retirement plan is to sail to the Bahamas. First I'll sail from Baltimore to Norfolk and then via the Intracoastal Waterway to Florida. From there it is only 90 miles to the Bahamas, which can be done in 24 hours. I'm planning to do some island hopping in the winter. After that, I'll probably go back to Texas. We still have a house in Texas. Perhaps we'll move to a lake property and have a boat in my backyard.

I still have some unfinished work on semi-nonparametric econometrics, which I will continue to work on, but I can do that anywhere, even on a boat.

NOTE: All recent photos were taken by Bulat Gafarov.

REFERENCES

[1] Andrews, D. (1997) A conditional Kolmogorov test. *Econometrica* 65, 1097–1128.
[2] Bierens, H. (1980) *Robust methods and asymptotic theory in nonlinear econometrics*. Ph.D. thesis, University of Amsterdam.

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

[3] Bierens, H. (1981) *Robust Methods and Asymptotic Theory in Nonlinear Econometrics*. Springer-Verlag.
[4] Bierens, H. (1982) Consistent model specification tests. *Journal of Econometrics* 20, 105–134.
[5] Bierens, H. (1983) Uniform consistency of kernel estimators of a regression function under generalized conditions. *Journal of the American Statistical Association* 78, 699–707.
[6] Bierens, H. (1984) Model specification testing of time series regression. *Journal of Econometrics* 26, 323–353.
[7] Bierens, H. (1987) Kernel estimators of regression functions. In Truman Bewley (ed.), *Advances in Econometrics: Fifth World Congress*, vol. I, pp. 99–144.
[8] Bierens, H. (1990) A consistent conditional moment test of functional form. *Econometrica* 58, 1443–1458.
[9] Bierens, H. (1993) Higher–order sample autocorrelations and the unit root hypothesis. *Journal of Econometrics* 57, 137–160.
[10] Bierens, H. (1994) *Topics in Advanced Econometrics: Estimation, Testing, and Specification of Cross-Section and Time Series Models*. Cambridge University Press.
[11] Bierens, H. (1997) Nonparametric cointegration analysis. *Journal of Econometrics* 77, 379–404.
[12] Bierens, H. (1997) Testing the unit root with drift hypothesis against nonlinear trend stationarity, with an application to the U.S. price level and interest rate. *Journal of Econometrics* 81, 29–64.
[13] Bierens, H. (2001) Complex unit roots and business cycles: Are they real? *Econometric Theory* 17, 962–983.
[14] Bierens, H. (2004) *Introduction to the Mathematical and Statistical Foundations of Econometrics*. Cambridge University Press.
[15] Bierens, H. (2007) Econometric analysis of linearized singular dynamic stochastic general equilibrium models. *Journal of Econometrics* 236, 595–627.
[16] Bierens, H. (2008) Semi-nonparametric interval-censored mixed proportional hazard models: Identification and consistency results. *Econometric Theory* 24, 749–794.
[17] Bierens, H. (2012) Consistency and Asymptotic Normality of Sieve estimators under Weak and Verifiable Conditions. Technical report, Penn State.
[18] Bierens, H. & L. Broersma (1993) The relation between unemployment and interest rate: Some empirical evidence. *Econometric Reviews* 12, 217–256.
[19] Bierens, H. & J. Carvalho (2007) Semi-nonparametric competing risks analysis of recidivism. *Journal of Applied Econometrics* 22, 971–993.
[20] Bierens, H. & J. Carvalho (2011) Job search, conditional treatment and recidivism: The employment services for ex-offenders program reconsidered. *B.E. Journal of Economic Analysis & Policy* 11(1–5).
[21] Bierens, H. & J. Hartog (1988) Estimating a hedonic earnings function with a nonparametric method. *Empirical Economics* 13, 267–294.
[22] Bierens, H. & J. Hartog (1988) Non-linear regression with discrete explanatory variables, with an application to the earnings function. *Journal of Econometrics* 38, 269–299.
[23] Bierens, H. & L. Martins (2010) Time varying cointegration. *Econometric Theory* 26, 1453–1490.
[24] Bierens, H. & W. Ploberger (1997) Asymptotic theory of integrated conditional moment tests. *Econometrica* 65, 1129–1151.
[25] Bierens, H. & H. Song (2012) Semi-nonparametric estimation of independently and identically repeated first-price auctions via an integrated simulated moments method. *Journal of Econometrics* 168, 108–119.
[26] Bierens, H. & N. Swanson (2000) The econometric consequences of the ceteris paribus condition in economic theory. *Journal of Econometrics* 95, 225–253.
[27] Bierens, H. & L. Wang (2012) Integrated conditional moment tests for parametric conditional distributions. *Econometric Theory* 28, 328–362.
[28] Billingsley, P. (1968) *Convergence of Probability Measures*. Wiley.
[29] Billingsley, P. (2012) *Probability and Measure*. Wiley.

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press

[30] Breitung, J. (2002) Nonparametric tests for unit roots and cointegration. *Journal of Econometrics* 108, 343–363.
[31] Escanciano, J.C. (2006) A consistent diagnostic test for regression models using projections. *Econometric Theory* 22, 1030–1051.
[32] Gallant, A.R. (1997) *An Introduction to Econometric Theory: Measure-Theoretic Probability and Statistics with Applications to Economics*. Princeton University Press.
[33] Gallant, A.R. & D. Nychka (1987) Semi-nonparametric maximum likelihood estimation. *Econometrica* 55, 363–390.
[34] Gallant, A.R. & G. Tauchen (1989) Seminonparametric estimation of conditionally constrained heterogeneous processes: Asset pricing applications. *Econometrica* 57, 1091–1120.
[35] Guerre, E., I. Perrigne, & Q. Vuong (2000) Optimal nonparametric estimation of first price auctions. *Econometrica* 68, 525–574.
[36] Heckman, J. & B. Honoré (1989) The identifiability of the competing risks model. *Biometrika* 76, 325–330.
[37] Phillips, P. (1987) Time series regression with a unit root. *Econometrica* 55, 277–301.
[38] Phillips, P. & S. Durlauf (1988) Trends versus random walks in time series analysis. *Econometrica* 56, 1333–1354.
[39] Phillips, P. & P. Perron (1988) Testing for a unit root in time series regression. *Biometrika* 75, 335–346.
[40] Powell, J.L., J. Stock, & T. Stoker (1989) Semiparametric estimation of index coefficients. *Econometrica* 57, 1403–1430.
[41] Stinchcombe, M. & H. White (1998) Consistent specification testing with nuisance parameters present only under the alternative. *Econometric Theory* 14, 295–325.
[42] Stokey, N., R. Lucas, & E. Prescott (1989) *Recursive Methods in Economic Dynamics*. Harvard University Press.
[43] Stute, W. (1997) Nonparametric model checks for regression. *Annals of Statistics* 25, 613–641.
[44] White, W. (1984) *Asymptotic Theory for Econometricians*. Academic Press.
[45] Zheng, J. (2000) A consistent test of conditional parametric distributions. *Econometric Theory* 16, 667–691.

https://doi.org/10.1017/S0266466612000497 Published online by Cambridge University Press