

LESSONS FROM THE GLASS CEILING AND THE BLACK BOX

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It is well documented that Economics did not warmly embrace the entrance of women into its profession. Today, still only 6 percent of tenured full professors in economics departments are female (CSWEP 1996). Among business economists, the median base salary of women is 80 percent that of men, a lower percentage than actually prevailed a decade ago (NABE, 1992).

Also well documented is the decline of regional studies in economics as a subfield of economics. The overall respect afforded that subfield is perhaps most sadly related by Cynthia Rogers and Stephan Weiler in their article, "Regional Science: A View from the Doorway":

"During several recent job interviews, interviewers made derogatory remarks about the urban and regional fields being less than respectable in economics Hence, on the job market, we were labor economists with regional interests, rather than regional economists with strong backgrounds in labor." (Rogers and Weiler 1995).

And finally, the demise of macroeconomic forecasting as a field has become legend. Commercially, the Big Three—DRI, Wharton and Chase—downsized to the Big Two; banks and private companies laid off large macroeconomic forecasting staffs in their entirety; and in academia, only a few names linger—such as Saul Hymans at Michigan and Ray Fair at Yale (Passell 1996).

Three struggling communities of scholars—women in economics, regional economists, forecasters. I stand at the intersection of these three sets; I a female regional economic forecaster. Three strikes and you're out comes ominously to mind. One obvious question to ask is, "Why did I make all these absurd career decisions?" A second is "How did I survive?" We'll sidestep the first question, as it is perhaps more appropriate for a professional meeting of psychoanalysts. Survival is a more interesting topic for regional science at the present time and it is certainly a hotly debated one: Has it survived? Will it survive?

Regional science in fact has a lot in common with women in economics and with forecasting, and both the female economists and the forecasters have learned

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some useful survival lessons—albeit with some pain along the way. This essay focuses on three lessons—one from women, one from macroeconomic forecasting and one common to both groups—and then draws the lessons together in a recent example from my own experience.

One major lesson women learned was to quit creating their own barriers to entry, that is, to quit being active participants in making the glass ceiling ever more impenetrable. A commonly cited barrier that women created for themselves was that of self-doubt (Chelimsky, 1992). A number of recent studies have focused on the decline in self-esteem and the increase in lack of confidence that commonly occurs among teenage and college females. Female self-doubt has been a powerful perpetuator of the status quo. But female self-doubt definitely pales in comparison with the self-doubt that permeates the recent spate of literature on regional science in crisis, on regional science lost in space, on regional science as a dimensionless point in a featureless plane of irrelevance. Introspection and reflection are useful, but the internalization of perceived slights is destructive, and there is no slight that appears to gnaw at regional science more than its lack of acceptance in mainstream economics.

After sitting for years through department of economics meetings and through department of economics hiring debates and decisions, I have been tremendously impressed by the number of my departmental colleagues who appear to be out of the "mainstream" of economics or who at least *believe* they are out of it—labor economists, urban economists, health economists, macro economists, empirical-anything economists. Andy Isserman (1995) has urged regional scientists to "go outdoors more," but any economist who has "gone outdoors," who has concerned himself or herself with a "real" market, has in fact drifted away from "mainstream" economics, a central core narrowly focused on the intersection of microeconomic theory and elegant mathematics.

Time changes the value of different areas of research and indeed, regional is already being revived in so-called mainstream economics. Let us not lose faith in our *own* abilities to judge what is valuable and useful research, and certainly let us not engage in destructive self-doubt. Let us just mix our regional interests with some practicality of working within the academic establishment in which so many of us reside.

In macroeconomic forecasting, much of the decline can be traced to the fact that it remained wedded to outmoded statistical techniques and economic theories that dated from the 1950s. It waited too long to shed the dominating influence of its pioneer, Lawrence Klein. Many of the classical techniques of regional science—shift/share, input/output, and the gravity model—similarly reflect the dominance of pioneers in the field, and we have to genuinely ask, "Are they not now outmoded?" Macroeconomic forecasting also languished because a *better*

product came along for dealing with uncertainties of the future—namely hedging price and interest rate risks with financial derivatives. As a multidisciplinary group, we ought to be able to internalize the benefits of growing new methodologies. We do not have to be the Economics Department losing to the Finance Department, but we cannot win if we are not growing those new methodologies.

Finally, both women and macroeconomic forecasting crumbled under the weight of the mega model dream. At the opposite extreme from the women who lacked confidence were those who attempted to be superwomen. As females increasingly entered the labor force in the 1960s, many were poorly guided by their *alma maters*. Instead of thoughtfully addressing how to define a viable *intersection* of homemaker space and labor market space, too often these colleges defined the goal for women as the *union* of both spaces—homemakers as multi-talented and multi-giving as their mothers and corporate executives or academic luminaries as professionally respected as their fathers. Women were blinded by the lure of an apparently glorious all-encompassing model, but unfortunately that model did not address the reality of life with its time constraints. And when they could not do it *all*, they were forced to drop some part of the goal, including dropping out of active pursuit of the highest rungs of the corporate and academic ladders.

Macroeconomic forecastings superwoman complex *far* exceeded that of any woman I ever knew. It oversold what its mega model black boxes could really do, thereby creating an extraordinary credibility gap, especially in the stagflation era of the 1970s. As Stephen McNees has succinctly put it, "They were not only consistently wrong, they constantly changed their forecasts in the wrong direction" (*Business Week*, 1981). They ignored the reality that their clients wanted accuracy, and the means—the increasingly complex models—overshadowed the ends.

The history of regional science abounds with mega model visions. Recently, there has been a call for regional science to get more complex (e.g., Bailly and Coffey 1994; Bolton and Jensen 1995; Markusen 1995; Warf 1995). Authors have suggested that the malaise of the field would be cured if only its theories and models embodied political science, social philosophy, anthropology, historical geography, cultural geography, sociology, risk analysis and information theory. In short, regional science would be fine if it simply did it all, if it ate the *whole* thing, if it was super science.

As I read this recent literature, I had difficulty reconciling my tendency to reject the mega model dream with my feeling that perhaps we need lofty goals to avoid just plodding along. There is an intermediary ground here, and the resolution I found is in whence the mega model arises. My discomfort is with the call for complexity that is not derived from addressing a specific problem; the notion

that somehow more complex models are inherently better models. That vision is the one doomed to failure. However, when complex, interdisciplinary models arise in the context of solving a well-defined problem, they are exciting.

Let me bring these thoughts together in a more specific example. Recently, I have worked in the area of modeling issues related to natural disasters, a field I was blown into by the high, force-four winds of Hurricane Andrew and the need to forecast the path of the Florida economy in the wake of that event. Earthquakes, floods and hurricanes are intensely regional in impact, and they are among the most wrenching of regional phenomena in their simultaneous assault on the physical region, the economic region and the sociological region. Yet, when I looked to the regional science literature for guidance on this forecasting problem, there was almost nothing useful (West and Lenze, 1994). We have created hypothetical, stylized natural disasters that are amenable to analysis with *existing* methodologies—regional econometric models, input-output models, linear programming models and computable general equilibrium models. These conceptual disasters—e.g., the earthquake whose *only* physical impact is to knock out one specific infrastructure lifeline—are *so* abstracted from real events that they are not even the same item. The hypothetical event is essentially our traditional well-defined firm closure or firm opening in a specific SIC, an event we are more comfortable addressing using traditional methodologies.

Alternatively, we threw the same *existing* methodologies at *real* events and concluded that existing methodologies were woefully inadequate for addressing the issues. To name but a few shortcomings, physical stocks were not in the models, so the models could not simulate impacts of destroying those physical stocks, migration models did not embody how households react when their destroyed house is suddenly a liquid asset in the form of an insurance pay-off. Do they rebuild or do they move? And they all failed to capture the historical reality of the ingenuity of firms and households in establishing alternative purchasing arrangements when traditional ones were severed. Having found existing methodologies inadequate and lacking the mega model that would readily simulate the situation for us, we simply dropped the subject.

This represents the worst of regional science—clinging to old methodologies, letting the old methodologies instead of the real problems dictate the approach, and then ultimately, simply giving up, becoming like the disappointed female pursuing the superwoman goal or the beaten-down macro forecaster pondering the dents in his black box.

I contrast this situation with the dynamic, fascinating field of catastrophe modeling that grew up *outside* of regional science. This field focused on a very specific problem: How do you estimate the expected insurance industry losses from natural disasters? What should be the catastrophe load on Florida

homeowners' insurance? Or on that of South Carolinians, North Carolinians, Californians, etc.? In the mid-eighties, some thoughtful observers became concerned about potential underestimation of the traditional accounting methodologies. In hurricane-prone areas, expected catastrophe losses were based on calculations of excess wind damage from actual historical experience, about two decades of experience. By the mid-1980s, Mother Nature had just handed over two decades of lull in hurricane activity. It was not unusual, viewed in the context of a century of meteorological data peppered with periods of intense activity and periods of relative calm, but it certainly did not represent *expected* activity, and *expected value* is the cornerstone of insurance. Tinkering with the standard methodology did not appear fruitful. Loss records did not replicate the extended length of meteorological data; and even if they had, such records were meaningless because regional redistribution of the population and property at risk had changed dramatically over time.

These scientists scrapped the old methodology and considered how to represent the entire century of meteorological data in the context of the current regional distribution of real estate. What emerged was a complex computer simulation methodology. It was *ultra* multi-disciplinary. Meteorologists and statisticians converted the weather data to probability distributions of hurricanes characterized by central pressure, radius of maximum windspeed, forward speed, etc. Historical landfall data were smoothed to allow non-zero probabilities of strikes at points that had never experienced an actual hurricane landfall. Even a hundred years of data is relatively sparse for events such as hurricanes. Geographers worked on geocoding of real estate and testing how fine a geographical grid was needed to accurately reflect significant changes in disaster risk. They coded critical land and development features that allowed meteorologists to model how a hurricane loses intensity as it travels over different types of physical land areas. Engineers addressed problems like this: If a building of A stories is made of B construction materials and is located in a spot of C development intensity at a point D in the windfield of a hurricane of E magnitude, what is the expected damage to the building? Actuaries worried about issues such as, if we know the physical damage, how do we estimate the insured damage? This calculation is affected not only by details of insurance policies, but also by details of regional natural disaster management—e.g., if a forced evacuation occurs, homeowners are entitled to loss-of-use compensation. Put together, these multidisciplinary pieces allowed simulations of thousands of years of hurricane experience on today's regional real estate distribution. A complex model focused on one specific problem: What is the expected loss cost function?

In the 1980s, this model development somewhat intrigued *re-insurers*, but largely was ignored by primary insurers and state agencies charged with insurance

industry regulation. In 1990, following Hurricane Hugo, Karen Clark, a pioneer in the field of catastrophe modeling, warned that *more* probable than a Hugo in South Carolina was the occurrence of a \$10-\$20 billion insured-loss event in Florida. After August, 1992, when that \$15 billion event arrived in the form of Hurricane Andrew, primary insurers and state regulators sat up and took notice. These models are now major players in the insurance industry; they are beginning to have a say in homeowner insurance rates all over the southern United States. Karen Clark's fledgling enterprise is no longer "small business."

I have recently been serving on a state commission in Florida to decide how these models will be used in state regulation of insurance. It has been exciting to study these models. It has been exciting to interact on the commission—sometimes cooperatively and sometimes confrontationally—with meteorologists, computer scientists, engineers and actuaries. Its been great to be outdoors—working on resolving a real regional problem—the very real insurance crisis in Florida. While by contrast, it highlighted my disappointment with regional science contributions to the literature on natural disasters, it simultaneously by example rekindled my enthusiasm for the multi-disciplinary modeling that is the core of regional science. Self-doubt about the field turned to constructive energy. Equally importantly, it reminded me of how successful the Dean/Witter approach is—one real regional problem at a time. Those catastrophe models started with a very specific problem, they developed the methods or combined the methods necessary to address that one specific problem, and the complexity of the model was solely the result of what was needed to solve the problem.

The Southern Regional Science Association has a tradition of the Dean/Witter approach, and it is one we need to maintain. I would add to that my observation that we have a tradition of enthusiasm—this is a fun meeting and a lively meeting. Certainly that annual rekindling of enthusiasm—both individually and collectively—is also important to maintain. It also has a tradition of presidential addresses that add a new perspective from a different field, from a different background. I appreciate the opportunity to have added my own perspective, I am honored to have served as your President, and I look forward to working with you in the future on the many fascinating regional problems out there to be tackled.

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