

## Economic Notes from the Underground

# Economic Dissociative Identity Disorder: The Math Gamer, the Anti-Policy Econometrician and the Narrative Political Economist

Bruce L. Benson<sup>1</sup>

### ABSTRACT

According to Wikipedia, dissociative identity disorder (DID), as defined by the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders, is a psychiatric diagnosis that describes a condition in which a single person displays multiple identities or personalities (known as alter egos or alters), each with its own pattern of perceiving and interacting with the environment. One of the individuals who wrote an outside letter for my promotion to full professor several years ago stated that, until he saw my vita, he did not realize that the Bruce Benson who wrote *The Enterprise of Law* (1990) was the same Bruce Benson who wrote a number of spatial-price-theory papers. I obviously have had multiple economic personalities in my career, but I believe that my struggle with this disorder has been resolved. One of my colleagues recently sent me comments on a job candidate who had presented a macro general equilibrium model, stating that the candidate “had simplified ... [the] model so much as to strip out most of anything that was really interesting and important. It

---

1. Chairman, Department of Economics, DeVoe Moore and Distinguished Research Professor, Florida State University, Tallahassee, FL 32306

seemed to me that a good narrative economist (e.g., you or Doug North or Bill Fischel) could have developed a much better, less technical and less restricted, theory that was also much richer and generated far more and more interesting testable hypotheses.” I was very pleased when I saw myself described as a “good narrative economist,” even though I started my career dominated by a very different alter ego, seeking recognition among mainstream economists as a micro “theorist”. Much of the theory I produced was really just mathematical game playing, however, and while this work was being produced, another personality, a narrative political economist, was also trying to emerge. After the mathematical-game-player personality got sufficiently bored and disillusioned with the games and began to recede, yet another new personality emerged as I turned to econometrics. Still focusing on a desire to gain recognition through publication in mainstream journals, I continued to have two personalities. In recent years, however, treatments and medication have suppressed the variants of my “mainstream” personality. I am now pretty much of a single mind.

My mentor during my PhD program at Texas A&M University was Melvin Greenhut. I was learning that transportation costs, so often neglected, are very significant in the real world. Mel did some very valuable work during his career, although he also suffered some from DID. When he was working on his dissertation there were two main theories about location choice: a cost-minimization theory and a spatial competition theory. Mel combined the two, producing a richer, more fruitful theoretical framework. By the time I was working with him, he had become a leading contributor to the literatures on “spatial price theory.” The Samuelson/Takyama-Judge spatial version of perfect competition model had all production in spatially separated markets taking place at the same point in the markets, on the head of a pin, before being shipped. Mel’s demonstrations that competition in real world markets of spatially-linked firms does not look anything like Samuelson/Takyama-Judge or more familiar models of “perfect competition” had very serious implications for anti-trust and regulatory policy. Some of my early work focused on the policy implications of spatial-price-theory, but my most successful efforts, in the sense of prestige publications, were essentially mathematical game playing.

As spatial price theory developed, various contributors to the literature discovered that, by changing assumptions about functional forms or interdependent behavior, surprising results could be “predicted.” Whether the predictions or the assumptions had any real-world relevance mattered not, as these kinds of models were being accepted by the most prestigious journals. In fact, the first paper I submitted to a journal was accepted at the *American Economic Review*. Two spatial price theorists had demonstrated that two of the most widely used behavioral assumptions implied that price should rise with entry, but I explained that their results arose from their demand function assumption, and that a different demand

function with the same behavioral assumptions predicts falling prices with entry! The *AER* publication was exciting, of course, so my math-gaming personality emerged to dominate my other personality. I engaged in more math games as I strived for recognition among mainstream economists. For instance, I demonstrated that, theoretically, an import tariff can lead to falling prices in the domestic economy, depending on the assumptions about demand functions and behavioral strategies. This, as far as I can tell, is totally irrelevant from an economic perspective, but that did not matter, as it was published in the *Journal of International Economics*. Between 1980 and 1985 such game playing produced 15 “pure theory” journal articles, at least a dozen of which appeared in highly regarded field journals, including the *Journal of International Economics*, the *Journal of Industrial Economics*, the *Journal of Urban Economics*, the *American Journal of Agricultural Economics*, the *International Journal of Industrial Organization*, *Regional Science and Urban Economics*, as well as general journals including *Economic Inquiry* twice, the *Southern Economic Journal* three times, plus the *AER*. I also produced three “applied” theory articles, but they ended up in less prestigious journals.

I guess I was playing the model building game pretty well, but it was boring and by the mid-1980s, getting another math-game article accepted did not generate the rush that it had earlier. I also quickly became aware of the fact that mathematical theories can be manipulated to predict practically anything that the theorist wants to predict, simply by changing some assumptions. While I knew I was playing a game, many other theorists seemed to take their models much more seriously. Knowing the ease with which predictions can be altered, I found it disturbing when such “theorists” claimed that their mathematical models carried policy implications. I recall one theorist telling me that good policy analysis requires the “rigor” generated by mathematical modeling. When I pointed out that his model assumed away absolutely vital considerations for policy analysis, his answer was something like: “well, we can build those things into the theory but we need to start out with a more streamlined model.” Unfortunately, such things never seem to be built into models, and yet, policy recommendations based on such mathematically-complex but economically-simplistic model-building abound.

My math-gaming personality had made a large investment in learning the games it was playing, so despite the boredom and the growing disenchantment with “applied theory,” I continued to produce spatial-price-theory papers for a few more years, including two more publications in the *American Economic Review*, as well as others in journals similar to and including those listed above. Nonetheless, the flow of such papers diminished over time as dissatisfaction with mathematical gamesmanship mounted. I began to feel that I was wasting my time and my mind. The last gasp of my mathematical game player personality was published in the *Economic Journal* in 1991.

As the flow of mathematical papers declined I began to consider moving into empirical analysis of spatial models, in hope of exploring the relevance of these models. In fact, for a few years, multiple personalities fought for dominance. I published four spatial-pricing papers using experimental techniques in 1987 and 1988, for instance, culminating with a *Journal of Economic Behavior and Organization* article. At least a half dozen empirical papers testing spatial models were also published between 1990 and 1995, some in well-regarded mainstream places like *Economic Geography* and the *American Journal of Agricultural Economics*. These papers tested fundamental implications of spatial price theory (implications from theoretical work Mel Greenhut had produced in the 1950s), but these personalities were not strong enough to maintain themselves for very long.

Tests of spatial pricing models were not my only forays into applied econometric analysis. My disillusionment with theoretical policy analysis encouraged another personality to emerge and grow in strength: one dedicated to empirical policy analysis. I did not feel that my understanding of econometrics was sufficient to jump into empirical work, but fortunately, I got an opportunity to work with Ron Johnson on a couple of empirical papers. While Ron is not easy to work with, the results were very satisfying. At the time, the empirical literature on interstate tax competition was almost unanimous in the conclusion that tax competition had no significant impact on location choice and economic performance across states. Ron and I did not believe the result, and simply by considering lagged impacts of relative changes in state taxes, we were able to contradict this widely held conclusion. Showing that implications drawn from other studies are not robust was very exciting. Essentially, this was *anti-policy* analysis, in that the objective was to show that policy conclusions drawn by many applied econometrics studies were suspect.

The emerging empirical policy personality morphed into an anti-policy empirical analyst, which dominated my being by the early 1990s. Projects joint with David Rasmussen and other coauthors took on the drugs-cause-crime literature, as well as the drunk-driving literature that, at the time, unanimously contended that the best way to reduce alcohol-related traffic fatalities is to raise taxes on beer. All of my personalities like beer, so this was a direct attack on me. But the literature was also finding relationships that were much too large to be credible. We demonstrated that the large and significant negative relationship between beer taxes and traffic fatalities found in so many empirical studies is not robust, as these models suffer from serious missing variable bias.

Similarly, we produced significant empirical results implying that the drug-war reallocation of criminal-justice resources actually causes crime, since fewer resources are available for the control of non-drug crime. Simultaneously we cast significant doubt on the existence of a strong drug-use-causes-crime relationship.

We also offered evidence implying that drug enforcement efforts do not appear to respond to changes in crime rates, as they should if police really believe that drug enforcement is a powerful tool in fighting crime. Instead, our evidence indicates that police were reallocating their resources to drug control in order to take advantage of changes in asset-seizure law that allowed police agencies to keep what they confiscated.

While this anti-policy research was quite interesting, it also was very frustrating. First, it revealed that econometric analysis is just as easy to manipulate as mathematical model building. Results depend crucially on the variables included and the empirical model tested—OLS or simultaneous equations, cross-section or panel, log-log or log-linear, etc. Clearly, policy advocacy based on statistical analysis is just as suspect as policy implications drawn from math models.

Another source of frustration was the discovery of how much empirical policy analysis seems to be driven by a search for the “right” answers. As a result, it is extremely difficult to get empirical research published if it challenges the conventional wisdom in a literature. Editors send a submission to referees whose work is being challenged, and many look for ways to justify rejection of the paper. These justifications often lack any validity, but editors routinely reject the papers anyway. In mathematical game playing, on the other hand, I find, authors often see challenges as sources of increased citations and openings for additional papers. Model-builders who advocate policy based on their mathematical manipulations are similar to empirical policy analysts, however, in that they generally do not want their normatively-driven policy prescriptions to be attacked. The implications for my chosen profession are depressing: If you play economically irrelevant mathematical games you can publish in the high-prestige journals, but if you do applied analysis that questions the soundness of previous findings (i.e., anti-policy analysis), you are relegated to less prestigious journals—though, I’ve found that once published these studies tend to attract a lot of attention and citations, and before long other researchers start reaching the same conclusions. So, while I continued doing some of this work until the early part of the new century (my last empirical paper was published in 2007), the frustration with the publication process ultimately brought my empirical anti-policy personality to the same fate as my mathematical-gamer personality.

During the entire time that the mathematical-game-player and anti-policy statistical-manipulator personalities were prominent, another personality also existed: a narrative political economist. Many people who associate me with spatial price theory, or perhaps with anti-policy econometrics, may not even realize that I have this other alter ego.

My thesis advisors at the master’s and PhD levels both told me that I did not write well, but this did not defeat the narrative economist in me. While my friend,

Bob Higgs, the editor of *The Independent Review*, still complains about my long run-on sentences, Bob and a few other editors such as Murray Rothbard and Tom Borcherding, who actually believe that one of an editor's responsibility is to edit, have pushed me to become a better writer. It is probably a good thing since the narrative economist in me has finally risen to dominance, with a focus on political economy in the Adam Smith sense (as well as F. A. Hayek, James Buchanan, Douglas North, Ronald Coase, Armen Alchian, Gordon Tullock, etc.): essentially, comparative institutions analysis.

Actually, as the mathematical game player was developing, at least a part-time narrative-political-economist personality was also kicking in the womb of my scholarly being. Luckily, as part of my PhD program, I took public-choice courses from Randy Holcombe and Steve Pejovich, along with a property-rights workshop with Eric Furubotn. I actually began writing in these areas even before I finished graduate school. Some of this work was mathematical and later on, some was empirical, but for the most part, it has been narrative economics. Such work is difficult to publish in high-prestige journals, of course, but the success of my spatial price theorist personality meant that my department chairs and, for the most part, my colleagues, did not complain too much about the time I spent developing my narrative skills and working on topics that I find meaningful. I have certainly heard some complaints, of course, often alleging a "quality-quantity tradeoff" and that "quality" research is research published in prestigious journals—e.g., work I did in spatial price theory or anti-policy analysis. Actually, of course, many of the complaints simply reflect differences in perceptions of quality academic work. Several years ago a friend asked me to apply for a chair in his department. When he asked his colleagues on the search committee about the status of my application he was told that I had some very good publications but that my vita would be even stronger if I did not have all the papers outside the mainstream, most of which were political economy narratives with strong libertarian messages.

For the most part, my narratives are attempts to explain observed relationships between institutions and behavior, often as part of a comparative institutions project. The observations generally are provided by others. I consider, for instance, the evolution of institutions as reported by historians, attempting to explain both the causes and consequences of the observed changes. Similarly, I explore and draw on anthropologist's descriptions, current research, popular press reports, government reports, court and police documents, etc. Such sources may speak to international institutions, internet institutions, regulatory processes and outcomes, taxes and transfers, informal norms and practices, private alternatives to public institutions, and so on. As one alter ego was developing spatial-price-theory models during my early career, another wrote narratives about political

corruption, logrolling, bureaucratic behavior, interest group politics, regulatory transfers, private security markets, contracting out, arbitration, stateless law, and other topics that interested me, even if they did not interest mainstream editors. Many of my early papers of this kind were published in places like the *Journal of Libertarian Studies*, the *Cato Journal*, the *Journal of Contemporary Studies*, and other places outside the mainstream. Had those independent outlier journals not existed, the narrative political economist may have emerged much more slowly, so I am ever grateful to those who support and edit these journals.

By the mid-1980s I also started occasionally getting narrative papers accepted by mainstream economics journals, such as the *Southern Economic Journal*, the *Journal of Legal Studies*, and *Economic Inquiry*. This was encouraging, and the narrative effort grew as my mathematical side receded. 1988 saw narrative publications in *Public Choice*, the *International Review of Law and Economics*, and the *Journal of Institutional and Theoretical Economics*, and after that, efforts to produce mathematical papers dropped sharply.

The 1988 publications were some of the first to spinoff from my work associated with my 1990 book, *The Enterprise of Law: Justice Without the State*. Sometime in 1981 or 1982, I got a call from David Theroux, then President of the Pacific Research Institute. David, who later founded the Independent Institute, explained that he was putting together a book on gun control that Don Kates was editing for him. Kates, a modern-liberal lawyer who also happened to be anti-gun control, had gathered together other anti-gun control modern liberals to write papers, and David was looking for a classical liberal perspective to add into the mix. He told me that I was recommended by Murray Rothbard, who had just published a couple of my papers in the *Journal of Libertarian Studies*. David offered me \$1,000 to write a paper and I jumped at the opportunity, even though I did not know much about gun control. \$1,000 was a lot of money for someone not long out of graduate school whose wife was a student—she had worked to support us while I went through both my Master’s and Ph.D. programs and now it was my turn to support her while she pursued her degree. The plan was to attack the frequently made claim that “increases in guns cause increases in crime” by suggesting that increases in crime cause increases in guns—that is, growing levels of crime induce people to obtain guns to protect themselves. To support this argument, the narrator in me wanted to stress the failures of the criminal justice system and point out that increasing gun ownership was occurring simultaneously with increasing investments in all sorts of other private-sector crime control and protection activities. I found so much evidence of private sector crime prevention and protection, and of police misbehavior, that I had to call David to ask what I should do. I remember telling him that I already had about 100 pages typed, and suggesting that I could do one of two things: cut the 100 pages down so it could fit as a book

chapter or add even more material to it, and write a book. David replied, “Do both” and sent me a contract for a book on private sector involvement in legal processes and security services.

I soon learned that I actually knew very little about the topic that I was to address, and that many people including Murray Rothbard had already given the subject a great deal of thought. Nonetheless, several drafts and years later *The Enterprise of Law* was published in 1990. In the process, my narrative economist personality was introduced to Hayek, then Mises, Kirzner, and other Austrian economists. This alter ego also developed a new understanding and appreciation of Coase, Alchian, Demsetz, North, Williamson, Cheung, and a number of other brilliant narrative scholars. Several papers on the topics addressed in the book were also written and published in journals over this period. All of this work is narrative political economics.

While the narrative work related to *The Enterprise of Law* has not been published in high-prestige journals as frequently as my spatial price theory papers were, it has attracted much more attention, inside and outside economics. The book itself has been used in law-school, criminology, and economics classes, translated into Spanish and soon Italian, and generated large numbers of citations in both economics and law journals. One of the many papers from this project, “The Evolution of Commercial Law,” was rejected by several journals where people continue to tell me it should have been published. Finally, it was accepted by the *Southern Economic Journal*. Now it probably is my most cited article. It was awarded the Georgescu-Roegen Prize as the best paper published in the journal in 1989-90, and it has been reprinted in at least seven books, some following translation. Subsequent work expanding on the *The Enterprise of Law* and “Evolution of Commercial Law” themes has been published in such places as *Economic Inquiry*, *Constitutional Political Economy*, *Public Choice*, *The Journal of Law, Economics, and Organization*, *The European Journal of Law and Economics*, and *The Journal of Law and Economics* (an empirical paper about private security). A follow-up, award-winning book, *To Protect and Serve: Privatization and Community in Criminal Justice* also was published by the NYU press in 1998. While these publications do not stack up to the *AER* in the eyes of most economists, I am convinced that any one of them present better and more relevant economic insights than all of my old mathematical-game-playing publications put together.

Economic narratives also can be significantly influenced by the normative views of their authors, of course, so even though I find this kind of work to be very interesting and satisfying, I am not contending that it avoids the problems of manipulation that can characterize mathematical and econometric research. Results from math models, statistical models, and narratives can all be manipulated by formulation. However, the assumptions made in a narrative

analysis tend to be relatively simple and transparent, so readers can understand what is going on. Crucial assumptions in mathematical models can be very opaque, and often obscure rather than clarify. The assumptions my narrative personality makes about institutions and behavior are generally explicitly stated, and quite basic. They start with scarcity and reasonable conjectures of human purposes, conjectures that make sense from our vicarious position as students of the actors' contexts. These fundamental assumptions are brought to bear in the context of an uncertain world where transactions costs impede human pursuits. Institutions tend to evolve to reduce such costs, and facilitate voluntary interactions. I also generally assume that individuals and groups have two principal ways of augmenting their wealth, voluntary cooperation in joint production and trade, and taking wealth from others by force or guile. Some individuals develop comparative advantages in violence and predation, so institutions also may coordinate joint production of plunder or extortion. When extortion is institutionalized, a set of rules regarding wealth transfers is developed by one group and imposed on others. In most polities, there is an attempt to legitimize physical coercion monopolized by what we call government, and its apparatus is then solicited or suborned by various interests. My formulations suggest that recognition of both kinds of incentives, to cooperate voluntarily and to transfer through coercion, enhance our understanding of institutions and their behavioral implications. These assumptions and formulations often explain institutions and their evolution quite well, once one does the research to provide a rich and well-informed narrative. I feel this narrative political economy work has real value, not just for me but for the community of genuine scholars pursuing the great conversation of liberal civilization. By contrast, the work I did as a mathematical model-builder was purely self-serving, as it enhanced my standing in the mainstream of economics without adding to the body of useful economic understanding. While the anti-policy econometric work may have had some value, encouraging others to question policy pronouncements derived from statistical modeling, such benefits are, at best, fleeting.

It took more than 25 years for me to suppress my destructive economic personalities so that the narrative political economist could finally be free. I suppose I could relapse if I fail to take my meds (anti-depressants supplemented with beer and Jack Daniels), miss too many sessions with my therapists (Jim Gwartney, participants in Liberty Fund Colloquia), or lose touch with my support groups (APEE members, *EJW* editors and supporters, my wife and daughters). For now, I continue to write, sending my papers to places like *The Independent Review*, the *Review of Austrian Economics*, and a few other journals where editors want readable papers that have real-world relevance, with an occasional submission to

mainstream outlets. Two or three more books also will hopefully be produced before this narrative personality passes from the scene.

## About the Author



**Bruce L. Benson** grew up in a small town in North Central Montana. In 1969, he was drafted and assigned to a combat infantry unit in Vietnam. Before he returned to the “world” 12 months later, the roughly 350 men who cycled through his company experienced the deaths of more than 40 young Americans, almost 100 more Americans physically wounded, and God knows how many Vietnamese killed and wounded.

The totally unnecessary and unjustified carnage he witnessed and contributed to would shake any faith he had had in political and bureaucratic rhetoric. Afterward, he returned to Montana and attended the University of Montana, obtained his BA (1973) and MA (1975) in economics, and met Terrie now his wife of 36 years. He obtained a PhD in economics at Texas A&M University (1978) and took a position at Penn State University (1978-82), where his first daughter, Lacey, was born in 1981, before, once again, returning to Montana by accepting a position at Montana State University (1982-85). Florida State University made Professor Benson an offer he could not refuse, however, and he has been there since 1985. His second daughter, Katie, was born in 1991. In 1993 he was named as a University Distinguished Research Professor in 1993, and in 1997 received a DeVoe Moore endowed Professorship. He also was designated in 2006, as a Courtesy Professor in Law by FSU’s College of Law. An administrative personality tried to emerge in 2006, and while his research personalities quickly challenged and suppressed this new alter ego, this was not done quickly enough. He is now about half way through his six-year sentence as Department Chair, the punishment he received for allowing this evil personality to tentatively show its head, and he has no hope of early release with parole. Professor Benson’s e-mail addresses are [bbenson@fsu.edu](mailto:bbenson@fsu.edu) and [jumpstrt2@yahoo.com](mailto:jumpstrt2@yahoo.com).

[Go to September 2009 Table of Contents with links to articles](#)

[Go to Archive of Watchpad section](#)