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Aims and Scope

The Journal of Prices & Markets, published by the Ludwig von Mises Institute of Canada, is a journal that seeks to improve the understanding of the role of markets in the economy. Submissions should seek to shed light on contemporary issues while being grounded in a praxeological reasoning. Prices & Markets welcomes submissions from a variety of fields such as politics, sociology, and psychology, where ever they can bring relevance to economic and financial questions.

Mission

It is the mission of the Ludwig von Mises Institute of Canada to educate the public to the importance of placing human choice at the center of economic theory, to encourage a revival of critical historical research, and to advance the Misesian tradition of thought through the defense of the market economy, private property, sound money, and peaceful international relations.

"Economics is mainly concerned with the analysis of the determination of money prices of goods and services exchanged on the market. In order to accomplish this task it must start from a comprehensive theory of human action…. It must not restrict its investigations to those modes of action which in mundane speech are called "economic" actions, but must deal also with actions which are in a loose manner of speech called "noneconomic." — Ludwig von Mises

The Value of Land: Capitalization and Entrepreneurship

Douglas French
Email: Douglas.E.French@gmail.com

Murray Rothbard explained there were three differences between how land and labor were valued in *Man, Economy and State*. Rothbard wrote, “land can be capitalized in its price as a ‘whole factor’ and therefore simply earns interest and entrepreneurial changes in asset value” (2009: 566-67).

Rothbard continues by pointing out that labor is more scarce than land, “whereas land tends to be far more specific” (*ibid.*: 567).

There is no reserve price for land, as leisure is to labor, so Rothbard explains

> *The fact that labor is scarcer and nonspecific means that there will always be* unused land*. Only the best and most productive land will be used, i.e., the land with the highest DMVPs* [discounted marginal value products, see Block (1990)]. *Similarly, in the real world of uncertainty, where errors are made, there will also be unused* capital goods*, i.e., in places where malinvestments had been made which turned out to be unprofitable.*

Rothbard’s capitalization and entrepreneurial theories can be used to analyze a certain land parcel located on the Las Vegas Strip, which has experienced large swings in value and entrepreneurial error.

Recently, Australian billionaire James Packer listed 34.56 acres on the Las Vegas Strip for sale. The land parcel is in a very desirable location just north of the Fashion Show Mall and across Las Vegas Boulevard from the Wynn Hotel and Casino. His asking price is $400 million.

"This is one of the most exciting properties to become available in Las Vegas in a long time," listing broker John Knott, head of CBRE's global gaming group, said in a press release (Seagall 2017).

The site retains government approvals for a 1,100-room, 3.4 million-square foot hotel-casino project. That project was proposed by the Israeli El-Ad Group when that company paid $1.2 billion for the property at the top of the boom in 2007. Their plan to build a casino-resort modeled after New York's Plaza Hotel was stopped by the financial crash when funding for major casino projects ended.

Only 18.39 acres of the site are owned fee simple[1] as real estate nomenclature refers to it. The other 16.17 acres are leased from the Elardi family, who through thick and thin have collected rent payments of $3,750,000 per year, payable $312,500 monthly. The lease rate increases to $4,000,000 per year on August 1, 2023. The ground lease expires July 31, 2097.

This land parcel that sold for $1.2 billion in 2007 fell 77 percent in value and was purchased out of foreclosure for $280 million in 2014. Now, Mr. Packer's group cannot obtain financing and is marketing the land for 43 percent more than what was paid in a liquidation. No improvements have been made. In fact the casino that occupied the property--The New Frontier--was torn down after El-Ad purchased the property for the highest amount ever paid for casino land.

Rothbard wrote, "Future rents of durable goods tend to be capitalized and embodied in their capital value and therefore in the money presently needed to acquire them. As a result, the investors and producers of these goods tend to earn simply an interest return on their investment." (Rothbard 2009: 558).

In 2007 when the El-Ad Group made its purchase, the Las Vegas economy was not only robust, but it was believed there was much more to come, despite the Federal Reserve having increased the Fed Funds rate sharply since 2004.

Las Vegas Sands had just opened the 50- floor, 3025 room Palazzo Hotel next to the Venetian Hotel. McCarran International Airport had a $4 billion expansion underway which would eventually become another terminal. Wynn Resorts had its Encore property under

1 Fee simple represents absolute ownership of land, and therefore the owner may do whatever he or she chooses with the land.

construction, MGM's CityCenter was largest construction project in the world, the Fontainebleau was under construction, and Boyd Gaming had torn down the Stardust Hotel and Casino with plans for a huge new hotel property.

Making a huge investment, tearing down an old property, and starting fresh with vacant land was aggressive in 2007, and of course turned out to be just a part of what Rothbard described as a "massive cluster of errors." The CityCenter project almost bankrupted MGM and one of its towers was torn down (Berg 2016). The Fontainebleau was never finished and sold for pennies on the dollar at a bankruptcy auction (Langlois 2010). Boyd Gaming stopped construction after erecting the steel for its structure and remains as a huge skeleton, under different ownership, to this day. The airport's new terminal seems empty most of the time.

Rothbard (2012) explained,

> *An adequate theory of depressions, then, must account for the tendency of the economy to move through successive booms and busts, showing no sign of settling into any sort of smoothly moving, or quietly progressive, approximation of an equilibrium situation. In particular, a theory of depression must account for the mammoth cluster of errors which appears swiftly and suddenly at a moment of economic crisis, and lingers through the depression period until recovery.*

In 2014 when James Packer's group purchased the El-Ad site in foreclosure for $280 million (Katsilometes 2014) the Las Vegas economy was on the mend and the Federal Reserve's zero interest rate policy was in full swing. Key staff were hired and the start of construction was considered imminent. Gross gaming revenue in Las Vegas was over $9.5 billion in 2014, while visitor volume was over 44 million.

However, Packer could not secure the financing to start the project despite his personal wealth. Until early this year, the project was a go if financing could be secured. "There is no question that the environment is difficult for everyone at the moment, but deals are still getting done," Andrew Pascal (Packer's CEO) told the *Las Vegas Review Journal*. "We will continue to advance our development and ultimately secure the capital we need" (Stutz 2016).

So while the entrepreneur has the vision, he or she must convince financiers of their projections for cash flows extending far into the future. The project's debt, equity and profit total its present value. As Rothbard (2009: 571) explains, "The capital value of ground land, as we have seen, sums up the discounted total of all future rents, and these rental sums may exert a tangible influence from a considerable distance in the future, depending on the rate of interest. There is therefore no mystery in the fact of a capital value for an idle site, or in its rise. The site is not being villainously withheld from production."

Packer's ability to sell the property will depend on another entrepreneur being able to secure financing to build a project that will produce, as Rothbard refers to it, "future rents." As

Rothbard wrote, “entrepreneurial skills act only through investment” (2009: 963). The central bank’s continued artificially low interest rates make investments appear more profitable than they really are, and this is especially so for investments with long-term horizons, i.e., in Austrian terms, there is an artificial lengthening of the investment horizon.

If Packer had obtained financing and opened in 2018 as he projected, his timing might have been good. Rosemary Vassiliadis, director of Clark County’s aviation department believes McCarran airport will have more traffic this year eclipsing its record of 47.8 million airline passengers, set in 2007 (Marroquin 2017). However, gross gaming win for the first quarter of this year was $100 million less than the first quarter of 2007.

Las Vegas is attracting plenty of tourists, but they aren’t gambling as much as they used to. Chris Kirkham writes for the *Wall Street Journal*, “For casinos on the Las Vegas Strip, the share of revenue from gambling has been in decline for years, falling to about a third of the total in 2016, from 56% 20 years ago, as operators have pulled in more from hotel rooms, entertainment and food and beverage sales” (Kirkham 2017).

“All of that stuff in Vegas is more profitable than the gaming,” Caesar’s Chief Executive Mark Frissora told the WSJ. “We like that shift, and it’s becoming a bigger part of our overall mix” (Kirkham 2017). Mr. Frissora used to run Hertz rental-car so maybe he’s under the impression that selling $50 steaks is more profitable than dealing $50 blackjack.

However, the average hold percentage for blackjack is 12 percent and for craps it’s more than 13 percent. On the Strip slot machines *hold more* than 7 percent. Bar and restaurant profit margins are 3 to 5 percent (Reimer 2017). Plus, a customer’s stomach limits how much he or she can drink or eat, while only a tourist’s cash and credit limit how much they can gamble.

As Austrians understand, it’s not what a project might earn now, but in the future.[2] “What distinguishes the successful entrepreneur and promoter from other people is precisely the fact that he does not let himself be guided by what was and is, but arranges his affairs on the ground of his opinion about the future. He sees the past and the present as other people do; but he judges the future in a different way,” explained Ludwig von Mises (1949: 582).

Mises.org’s Ryan McMaken (2017) writes the Fed is already tightening. Using “Austrian” money supply figures, he wrote, “The supply of US dollars has slowed during early 2017 with March’s year-over-year percentage increase hitting a 103-month low of 5.9 percent. The last time the year-over-year growth rate was lower was during September of 2008, when the growth rate was 5.2 percent.”

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2 Steve Wynn, who owns two casino hotels across Las Vegas Boulavard (The Strip) purchased the Alon property and an adjacent 3.5-acres parcel for $336 million, in December 2017. “Clearly, they are warehousing this land for future use,” a Deutsche Bank analyst writes (Prince and Segall 2017).

Higher interest rates mean a higher capitalization rate of “future rents” or a lower present value. Add to that, less high margin business like gambling will be replaced by lower margin food & beverage business which means those future rents being capitalized will be lower.

Beauty is in the eye of the entrepreneur. But Mr. Packer, Professor Rothbard would say your price is too high.

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"There's a New Mexico?" On *The Fiscal Case Against Statehood*

Art Carden
Email: art.carden@gmail.com

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Abstract

Stephanie Moussalli's *The Fiscal Case Against Statehood* shows that statehood in Arizona and New Mexico meant larger governments but more accountability and transparency. Further lines of inquiry should include an exploration of the trade-offs between bigger and more transparent government, and still further work could explore what larger and more transparent states meant for US relations with Native Americans.

Introduction: Two Big Questions in the Move Toward Statehood

I was fortunate to count Stephanie Moussalli as a colleague during my time at Rhodes College, and I was thrilled to see her bring *The Fiscal Case Against Statehood* to publication. I was even more pleased when the organizers of the 2013 Association of Private Enterprise Education meeting allowed me to organize a symposium on her book. In the comments that follow, Joshua Hall (West Virginia University), Jason Jewell (Faulkner University), and Jayme Lemke (Utah State University) discuss Moussalli's contribution and its implications for how we understand states, federal systems, and property rights more generally.

Moussalli weaves together multiple approaches to the problem of statehood and government growth that offers the best of quantitative analysis and an analytical narrative approach that shows how people were thinking and acting in the face of changing conditions. She brings to scholars' attention what "is not a widely known fact:" "that many residents of American territories oppose statehood on the ground that the new state government will be too expensive" (p. 1). In her exploration of this "not widely known fact," she answers two very big questions using data from Arizona, New Mexico, and Nevada: did statehood mean larger states? She finds that it did. Did statehood mean better accounting standards and more transparency in public finance? Again, the answer is "yes." In short, statehood meant larger government, but it also meant "better" government in that the government was more accountable to the people. Moussalli has offered a contribution that expands our understanding in a number of different fields. Specifically, we learn about American federalism, the political economy of statehood, bargaining among competing interest groups (the "politics as exchange" paradigm that emerges from Virginia Political Economy in the tradition of James Buchanan), and the historical conditions that helped create the modern American union. In this comment, I offer a few ideas for further research based on *The Fiscal Case Against Statehood*.

Institutions, Institutional Change, and Government Growth

It is appropriate that Moussalli included Nevada as her control state not merely because it provides a useful standard of comparison and, on some margins, similar geography to Arizona and New Mexico. Nevada was the subject of Gary Libecap's 1989 study *Contracting for Property Rights*, and this is, in large part, what happens when states and polities bargain over political institutions. In much the same way, Moussalli follows John Brewer (1989) in her discussion of how states grew and became more powerful. It is also a useful complement to Douglas W. Allen's 2012 book *The Institutional Revolution* in that it discusses the importance of changes in how we measure things. This might seem superficial at first, but states' ability to provide reliable, transparent, and easy-to-interpret accounts of their activities will affect their ability to tax and borrow and, ultimately, their ability to specify and enforce different property rights regimes (whether for good or ill).

How do we know which rules are efficient? It is tempting to say that whatever we observe is efficient because if it weren't, people would simply change the rules. Things are more complicated than this, as Caplan (2007) argues and as has been explored by historians of institutions and institutional change (see e.g. North 1981, 1990, 2005 and North, Wallis, and Weingast, 2009). Coercion mars economic calculation, and can short-circuit the processes by which people arrive at new institutional forms that allow them to solve collective action problems. In the spirit of Ostrom (2010) and Libecap (1989), it would be useful to highlight the trade-offs between "bigger" government and "more transparent" government specified by Moussalli in light of the indigenous institutional forms that might have been upset by the reorganization of Arizona and New Mexico as states.

Along these lines, future scholars can take notes on how Moussalli does excellent spadework by assembling very useful data on public accounts. With respect to existing institutional forms, Moussalli helps resolve two questions germane to the statehood debate (on the quantity and quality of state-provided governance) and therefore provides a context in which scholars interested in small-scale institutional forms can advance their work. Mineral and water rights, for example, were of particular interest at the time and today. Moussalli's findings about the expanding scope of the state will provide scholars interested in these issues with an important fact about what statehood meant for the people of Arizona and New Mexico. Was the additional "quality" of governance necessary? Did it solve previously intractable collective action problems? Or did it merely crowd out existing institutional forms that had evolved in response to conflicts over water and mineral rights?

The experience of Native Americans raises often-overlooked but fundamentally important questions about western institutional and economic history. Arizona and New Mexico are each home to large concentrations of Native American Reservations. As Moussalli has shown how size and transparency of governments increased with statehood—and in this case, statehood of relatively recent vintage—this should provide a new vantage point from which to study the relationships between American and Native American governments.

Conclusion

Good scholarship helps settle one question while raising others. In this case, *The Fiscal Case Against Statehood* offers and supports a clear conclusion about the size and transparency of governments after statehood while, at the same time, contributing new insights to other research areas. One obvious extension will be to try to replicate Moussalli's findings for other states that entered the union, and there are clear implications for American territories that might someday seek statehood (Puerto Rico, for example).

In addition to the purely historical question about the size and quality of government in territories that become states, Moussalli also offers a research setting in which future scholars can explore the political economy of institutional change, the kinds of bargains that go on between different levels and types of government, and the ways in which coercive and non-coercive institutions develop to solve collective action problems. Future scholars of Native American history will be able to build on Moussalli's finding that statehood made the governments of Arizona and New Mexico larger and more transparent. The implications for the forced reorganization of Native American life remain incompletely explored.

Again, good scholarship helps us settle some questions while raising others. In this case, *The Fiscal Case Against Statehood* is good scholarship. It offers important insights into the political economy of institutional change and a new perspective on important historical questions. It is the kind of contribution I hope future scholars will follow and emulate.

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Why Statehood? A Note on Interpreting Jurisdictional Competition in U.S. History

Jayme S. Lemke
Mercatus Center at George Mason University
Email: jlemke@mercatus.gmu.edu

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Abstract: Stephanie Moussalli's *The Fiscal Case Against Statehood* is an institutionally rich and genuinely interdisciplinary contribution to understanding the incentives faced by political actors in the states and territories of the 19th century United States. One of Moussalli's key findings is that local governments exercise less fiscal restraint after achieving statehood than they had as federally managed territories. There are two ways to interpret this result. Moussalli's preferred interpretation is that competition between politically decentralized states was not an effective mechanism for constraining local political actors. In this note, I suggest the alternative interpretation that due to the significant gains associated with statehood and the fact that attracting population was an important part of the quest for statehood, territorial politicians may actually have been more susceptible to the influence of jurisdictional competition than state politicians. Consequently, admission to the union removed a source of competitive pressure that had previously constrained fiscal behavior.

Introduction

The Fiscal Case Against Statehood (Moussalli 2012) is a work in two parts, both of which are impressive examples of interdisciplinary research. Moussalli's findings and the record created in their pursuit are a trove of valuable information for the scholar interested in the political economy of early American federalism or 19th century state government.

In the first part, Moussalli attempts to adjudicate the historically contentious debate over the fiscal consequences of the transition from territorial to state government. She finds that statehood did encourage the growth of government in newly formed states. In the second part, she argues for conceiving of accounting as a technology of government that enables politicians to more efficiently expand their domain. Consequently the greater availability of rents associated with statehood incentivized local governments to develop more advanced accounting practices.

The focus of this note will be on the first section of *The Fiscal Case Against Statehood*, the objective of which is to resolve a contentious historical debate over the fiscal burden of state versus territorial governments. Through a careful study of the reports of state and territory financial officers and other historical data, Moussalli finds that statehood does result in an increase in the relative size of government. She interprets these results as finding against the theory of jurisdictional competition in this context, postulating that if jurisdictional competition were at work the decrease in federal oversight and subsequent transfer of authority from federal to state government would result in the newly decentralized government experiencing greater competitive pressures (Moussalli 2012, 10-12).

I wish to propose an alternative conception of jurisdictional competition, and therefore an alternative interpretation of Moussalli's findings. Jurisdictional competition can take many forms depending on institutional context. In the case of 19th century states and territories, territorial politicians stood to benefit from statehood in many ways. If statehood was one of the rewards of treating the local population well, then territorial politicians would have been more subject to competitive pressures than politicians in a post-statehood environment. In this conception, rather than statehood being expected to constrain the growth of government, statehood is expected to relax the constraints faced by territorial legislators. Interpreted in this way, Moussalli's results are consistent with the theory of jurisdictional competition.

Jurisdictional Competition in *The Fiscal Case Against Statehood*

The residents of territories in the 19th century United States took one of two positions on statehood. Those in favor of statehood believed that stronger local control would result in more fiscally responsible government. Those opposed to statehood feared exile from the federal system of subsidization of territories or believed that creating a new government would only place additional burdens on local residents (Moussalli 2012, 8).

Moussalli frames this debate in the context of two competing public choice theories. The first of these is the theory of Leviathan as articulated by Brennan and Buchanan (1980). In the theory of Leviathan, government is a predatory creature that only behaves itself to the extent that it is forced. Leviathan is not benevolent; he must be chained in order to prevent him from consuming his master. Moussalli (2012, 11) predicts that statehood loosens the chains around Leviathan by reducing federal oversight. This relaxation of the constraints on governmental power is essentially equivalent to a reduction in the costs of expropriation. Consequently, the theory of Leviathan predicts that statehood will result in more expropriation by the state (Moussalli 2012, 11).

The theory of jurisdictional competition is advanced as an alternative theory generating an alternative prediction. Moussalli (2012,12) describes the theory of jurisdictional competition as predicting that decentralization—i.e. the transfer of control over the jurisdiction from federal to state authority—will serve as a constraint on the behavior of the government. Consequently, the theory of jurisdictional competition is described as predicting that statehood will result in less expropriation by the newly formed state.

In order to adjudicate between these theories, Moussalli conducts an event study around the 1912 transition of New Mexico and Arizona from territory to statehood. Using two measures, locally sourced government revenue as a fraction of the assessed value of property within the jurisdiction and locally sourced government revenue as a fraction of gross national product, she finds that the relative size of government does increase after statehood. The government of New Mexico in particular nearly doubles in size. In contrast, Nevada, which remained a territory through the period, did not experience a spike in size (Moussalli 2012, 49-53).

Jurisdictional Competition and "the Prizes of Statehood"[1]

Competition is not inherently good. The effects of competition depend on what prize is being sought, and how the game is played. Competition is valued in a market context because the structure of competition is such that entrepreneurs are rewarded for coming up with a preferred allocation of scarce resources. The prize is profit, and the rules of the game state that the only way to win is to improve the welfare of both yourself and another by engaging in mutually beneficial exchange.

Theories of jurisdictional competition propose that it is possible for political systems to mimic some of the beneficial properties of market systems. Tiebout (1956) shows that under conditions of perfect information and costless entry and exit of both jurisdictions and individual residents, competition between local governments will result in an efficient allocation of public goods. Ostrom, Tiebout, and Warren (1961) propose the more general polycentric framework within which networks of autonomous organizations interact within the same system of rules to provide a complex array and variety of public goods, each at its optimal level of provision. One of the proposed benefits of such a framework is that the multiplicity of organizations enables consumer choice, forcing government agencies and public service providers to satisfy citizens'

1 Credit for this excellent phrase goes to Kenneth Owens (1987).

preferences or else risk losing their budgets.

Jurisdictional competition can be viewed as an imperfect analog of market competition rather than as an example of a perfectly competitive market at work. In this conception, interjurisdictional competition generates tendencies towards more responsible principle-agent behavior on the part of political principles rather than issuing any kind of guarantee of efficiency. So long as the resident of a jurisdiction is able to inflict some punishment upon political agents for predatory activity—or reward political agents for engaging in desired activities—the local government will be less likely to undertake the marginal expropriation. Jurisdictional competition then becomes something that exists in different degrees in different institutional contexts, with higher levels of competitive pressure more effectively constraining legislators.

As such when Moussalli (2012, 57) interprets the theory of jurisdictional competition as predicting greater constraints on legislators and her results as suggesting that "…the Leviathan dynamic trumps the effects of decentralization in this context," she presumes that state governments faced greater competitive pressure than territories. But like states, territories were already entities operating with some degree of autonomy in an environment where they faced some degree of competitive pressure. Both organizational structures allow the jurisdiction some degree of autonomy over a defined range of powers while reserving other authorities to the federal government or to the people. If there were competitive forces at work, why should they affect one jurisdictional classification and not the other? In the absence of further institutional context there is no apriori reason to believe that a state would face any more or less competitive pressure than a territory.

Theories of jurisdictional competition often presume that tax revenue is usually the prize won by the successful participant in a jurisdictional competition, but tax payments are not necessarily the only rewards an individual has to offer a local government. For example, statehood itself could be a prize. If territorial politicians stood to gain from statehood, and if the actions they had to take to procure statehood generally required satisfying the local population, then a model of jurisdictional competition where statehood is the prize could be expected to constrain territorial politicians.

The Pursuit of the Prize

Moussalli (2012) suggests several reasons why politicians would value the prize of statehood. First, territorial governors were appointed by the president. Consequently leadership changes were more likely to occur in response to regime changes in Washington D.C. than in response to local conditions. This was often frustrating for local politicians. For example, Democratic politician Edmund Ross was sent to be the governor of largely republican New Mexico in 1885, where he spends his time in office vetoing nearly everything put before him. This experience led the vast majority of politicians to prefer the independence of statehood to any benefits that might have been associated with territorial status (Moussalli 2012, 21).

Second, federal oversight over territorial expenditures was often burdensome. The Treasury criticized and refused to pay many territorial expenses even after they were already incurred. The Territory of Arizona was only granted sufficient funds to pay one of their four postmen because only one was authorized by statute. Even the fact that the Territory had spent money on a typist instead of hand writing reports was questioned by federal overseers (Moussalli 2012, 38-41).

Third, as the main finding of part one suggests, statehood came accompanied with a significant increase in the power to tax. The government of New Mexico more than doubled relative to the size of the private economy after statehood. As an example of how this might work in practice, consider that land holdings in the Territory of New Mexico were often assessed well below market value. By one estimate large land holders were paying taxes on 1/10 the value of their land (Moussalli 2012, 22). The greater discretion that states had over allocations of tax revenues would have provided legislators with more than sufficient incentive to correct these lax assessment practices.

Further, Moussalli finds that the increase in the size of government following statehood far overshadows federal subsidies previously granted to the jurisdiction because of its territorial status. The increase in local revenue was 28 times greater than federal subsidies in New Mexico and 35 times greater in Arizona (Moussalli 2012, 48). The sheer magnitude of this finding casts doubt on the claim that the loss of federal subsidies would have motivated territorial politicians to oppose statehood.

Territorial politicians stood to benefit from statehood in many ways. If the pursuit of statehood required politicians to act in the interest of the population, then territorial politicians would have been more subject to competitive pressures than politicians in a post-statehood environment. Rather than statehood expecting to constrain the growth of government, this conception of jurisdictional competition predicts that statehood will relax the constraints faced by territorial legislators.

The particular nature of the quest for statehood—namely, the role that population played in the territory's quest to obtain Congressional support for statehood—does suggest that territorial politicians would have been motivated not only to appease the local population but also to attract new residents. The Northwest Ordinance, written by Thomas Jefferson in 1787, established a population requirement as one of the conditions under which a territory could apply for statehood. Territories were to be permitted to petition for entry to the union when the population in the territory reached 60,000. In the 1850's this requirement was increased so that the territory had to attain a population sufficient to merit a seat in the United States House of Representatives (Owens 1987). This emphasis on population created an incentive for local political leaders to encourage population growth within their territory of residence.

The emphasis on attracting and maintaining population created an incentive for territorial politicians to develop an interest in whether or not people and businesses settled in their territory. This motivation helps explain why railroad routes were so bitterly contested in the 19th century—railroads exerted a great influence over the flow of population, and as such had the power to either help or hinder a territory's attainment of statehood. Governor Henry Haight illustrates the quest for population in his 1869 address to the Senate and Assembly of California: "[the transcontinental railroad's] completion has occasioned heartfelt rejoicing throughout California… The importance of facilitating immigration from the Eastern States and Europe is felt by all who are interested in our material development" (Gov. 1869).

Conclusion

Territorial governments in the 19th century United States were subject to an unusual form of jurisdictional competition. Due to the significant gains associated with statehood and the fact that attracting population was an important part of the quest for statehood, territorial politicians were particularly susceptible to the influence of jurisdictional competition. Consequently, admission to the union actually removed a source of competitive pressure that had previously constrained the fiscal behavior of the newly formed state.

The Fiscal Case Against Statehood bring this incentive structure to light with rich institutional detail. In doing so it takes advantage of a highly valuable but little employed research opportunity—the study of jurisdictional competition in a time and place in which the conditions for its function were more closely met than most other times and places in history. The governments of territories and new states in the 19th century United States had an unusually high degree of autonomy in the creation of constitutions and laws; the disciplining entry and exit of potential residents was far from costless, but was an active social movement; and the entry and exit of new jurisdictions was at least possible given the incompletely formed nature of the American union of states.

If more scholars follow Moussalli in her choice of subject and her attention to detail, the study of the political economy of competitive government will be greatly advanced.

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The Liberal Arts and Scholarship on *The Fiscal Effects of Statehood*[1]

Joshua C. Hall
West Virginia University
Email: joshua.c.hall@gmail.com

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Abstract

This comment highlights several important questions in two major areas that were inspired by reading *The Fiscal Case Against Statehood*. The first major area concerns the sociology of business disciplines, accounting in particular, with respect to interdisciplinary scholarship. Questions in the second major area focus on methodological questions surrounding the fiscal effects of statehood.

Keywords
statehood, graduate study, Leviathan

1 I would like to thank Art Carden for organizing the panel on *The Fiscal Case Against Statehood* at the 2013 APEE meetings in Maui.

Introduction

Accounting is generally not thought to be part of the liberal arts. The liberal arts, however, can inform good scholarship and improve our understanding of important issues. Only someone who is liberally educated could have brought to together in one book public choice theory, history, and accounting to provide a better understanding of this important period in U.S. history (Moussalli 2012). A close reading of Moussalli (2012) highlights an important issue regarding the sociology of graduate education in addition to a number of new research questions regarding the fiscal effects of statehood.

The Liberal Arts, Graduate Study, and Liberal Arts Careers

With academic degrees in anthropology, French education, history, public administration, and accounting, Moussalli's interests and training as a scholar are unique and not easily replicated. She is probably the only person alive who would have thought of writing, let alone had the background and training to write, such a unique and insightful book as *The Fiscal Case against Statehood*. Her background was crucial to her being in position to see the research question and answer it in a thorough and compelling manner.

Her experience suggests that scholarship in business and economics would be more varied, more interesting, and more insightful if greater numbers of liberally educated individuals were drawn into graduate study in these fields. One reason they are not drawn to these fields is lack of exposure. Many liberal arts colleges do not offer *any* business courses as part of the economics major (see, for example, Haverford College 2013) and those that do typically offer an accounting course or two as part of a business economics major (see, for example, Beloit College 2013).

Another piece of the puzzle of why more young people from liberal arts colleges do not purse doctoral work in economics or business is because they find the core courses and dissertations being undertaken by current graduate students to be narrow and boring. One piece of evidence in favor of this hypothesis is that many current economics graduate students feel that their core courses hold little interest or relevance (Colander 2005). Attracting people who are not just motivated by higher business school salaries requires signaling to students who are interested in better understanding the world around them that they can pursue a wide variety of research questions raised during the course of their studies. Committing to several years of graduate study and then six years of the tenure track is a big commitment for anyone, let alone someone who is uncertain as to whether or not she'll be able to conduct the research that interests her. One of the best things about *The Fiscal Case against Statehood* is it provides a model for young scholars who dare to be different.

Moussalli is upfront about her scholarship being outside what is normal in accounting. The first sentence of her acknowledgments states (p. xi): "There are not many doctoral programs that would allow research as interdisciplinary as this." While this would not be surprising for the very top programs in any discipline as increasing specialization is often seen as necessary for disciplinary advancement, it seems somewhat odd once you move away from the frontier. Graduates from the University of Mississippi PhD program in accountancy mainly place in smaller, non-graduate programs in the South. A few, like Professor Moussalli, end up at liberal arts colleges.

So why, in general, is scholarship like this so outside the norm? Is it the sociology of the profession, especially in what is considered good scholarship by those teaching in graduate programs? Is it selection bias of who wants to be an accounting professor? Is it AACSB publication standards making schools wary of hiring interdisciplinary scholars? These are important questions to find answers to if you believe that Professor Moussalli's work in *The Fiscal Case against Statehood* is an important and valuable contribution to our understanding of the fiscal effects of statehood.

Beyond scholarship, however, there is also the question of training people to be effective teachers at liberal arts colleges. Rhodes College, where Moussalli teaches, is somewhat unique in that it has such a large and thriving accounting program. As noted earlier, many other liberal arts courses have a course or two in financial or managerial accounting and would likely offer more if they could find someone with the background and interests of Moussalli. A background in the liberal arts is important because it informs how a PhD in business, or accounting, or economics interacts with their colleagues, students, and the interdisciplinary nature of the curriculum at liberal arts colleges. (For more on the uniqueness of liberal arts colleges, see Horwitz 2011).

Entrepreneurship can be thought of as picking up previously unnoticed dollar bills, either through pure arbitrage or by combining resources together in a way that creates value (Kirzner 1973). Moussalli (2011) and her career are evidence that there are dollar bills lying around unnoticed, waiting to be picked up by entrepreneurial graduate students and programs.

The Fiscal Effects of Statehood

One measure of the value of a scholarly volume is the number of questions it raises. Research that does not stimulate dozens of questions begging to be answered is not advancing our scholarly understanding of a topic. By that standard, Moussalli (2011) is extremely valuable in that it raises several important questions regarding the fiscal effects of statehood.

Moussalli finds that statehood raised the value of property within a territory after statehood. This suggests that the fiscal constraints on territorial revenue generation led to pent-up demands for publicly provided goods that were satisfied following statehood. If this is the case, however, it is unclear exactly how territorial governments were constrained in raising revenue prior to statehood. For example, the lack of own source-revenue could be because of fiscal constraints in raising revenue placed on them as territorial governments. Our knowledge of such constraints is limited, however, and better information on such constraints would help scholars better understand the mechanism by which statehood caused own-source revenue to increase.

For example, an analogy can be made to local school districts, which are often constrained in their ability to raise money to one tax base – the property tax. In Ohio, however, school districts were given the ability to levy a residency-based income tax in 1989 (Hall and Ross 2010). After being given the ability to tax another base, their total revenue increased (Ross and Nyguen 2013). Whether or not that is a good thing is a normative question, but it is pretty clear that relaxing the fiscal constraint is what caused revenues to rise.

A related question is whether these fiscal constraints are necessary to restrain Leviathan. Turnbull and Geon (2006) look at the effect of home rule on U.S. counties. They find that rural counties, because they are not constrained by Tiebout competition, often expand their spending beyond what is preferred by the median voter. In addition, they find that fiscal constraints states place on counties do affect county fiscal behavior and therefore might be justified to constrain the Leviathan-like behavior of rural counties.

Moussalli finds that statehood was crucial to getting improved financial reports and processes in New Mexico and Arizona. It is unclear, however, exactly *how* statehood allowed for the adoption and improvement of theses improved financial reports and processes. In particular, what exactly where the constraints that prevented territorial governments from improving their financial reports? Was it because the requirements of territorial governments were low or different? While Moussalli seems to be correct that these improved reports provided legitimacy and increased the ability to raise revenue, further evidence on the impact of statehood on accounting standards *not* during a time of generally increasing accounting standards would be an important piece of evidence that statehood leads to improved financial reporting.

This point highlights the biggest methodological issue related to Moussalli's (2011) findings—controlling for other important changes going on at the time. Moussalli's use of Nevada is instructive in this regard, but what if Nevada is an outlier among states at the time? After all, Nevada had an economic bust in 1910 and a serious population decline from during the decade of the 1910s. Arizona, on the other hand, gained over 100,000 residents from 1910 to 1920. New Mexico also increased in population during this period. Using Nevada as the *only* counterfactual seems to me to be problematic given its very different experience during this period.

It would be instructive to look at changes in the own-source revenue of *all* states (not just Nevada) at the time. This would be particularly important given the tremendous changes in state and local taxation going on at the time that led to increases in own-source revenues in many states. According to Wallis (2001), between 1902 and 1932 state revenues rose from 0.8 percent of GNP to 2.1 percent. What if New Mexico and Arizona are just part of this great increase in state revenues? Given the large number of new taxes that were implemented at the state level between 1900 and 1940 – taxes on gas, sales, and income – the role of statehood relative to improvements in the technology of taxation are difficult to disentangle without looking at *all* states as well as Nevada.

Conclusion

There are many reasons to read *The Fiscal Case against Statehood*. For example, one could be interested in the history of New Mexico or Arizona. Or one could be a public choice scholar interested in better understanding the growth of government in the early part of the twentieth century. A scholar of accounting history would find much of interest the role that accounting played in territorial governments.

Regardless of the reader's background, however, two things are certain. First, they will come away from the book knowing more about the topic, regardless of their background. Second, they will have a list of new questions that they want to pursue inspired by the book. That is the value of interdisciplinary scholarship.

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Not Letting a Crisis Go to Waste: Statehood, Reformers, and the Historians

Jason Jewell
Faulkner University
JJewell@faulkner.edu

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Abstract

Stephanie Moussalli in *The Fiscal Case against Statehood* argues that "something about statehood" led to increased taxation in Arizona and New Mexico. Various episodes of increased taxation in English history parallel these states' experiences in some ways, suggesting that a discontinuity in government gives fiscal and accounting reformers an opportunity to introduce innovations designed to increase government revenue. Moussalli's examination of the statehood process also highlights deficiencies in most American historians' treatment of the statehood process, a treatment which serves a nationalist narrative and generally ignores the effects of statehood on the inhabitants of the territories themselves.

The Fiscal Case against Statehood is a powerful example of the new kinds of understanding and interpretation that can result when an interdisciplinary background leads a scholar to make connections and ask questions that colleagues lacking that background have overlooked. Those who teach in the humanities frequently encounter these insights from colleagues who study the interplay among fields such as literature, philosophy, visual arts, and the like. However, Prof. Moussalli's book affords the unusual pleasure of seeing for the first time the comparable fruit from an exploration of the overlap among accounting, history, economics, and political science.

Prof. Moussalli's methodology is reasonable and calculated to produce persuasive findings. Obviously she was limited by the available data on the fiscal impact of statehood and its accessibility in various state archives. Working within these constraints, her selection of Arizona, New Mexico, and Nevada for her study makes sense. The use of Nevada as a control state is effective in showing how fiscal changes in Arizona and New Mexico after they became states in 1912 were not due solely to regional or national trends. Although Arizona and New Mexico shared many characteristics, their appreciable differences—for example, in ethnic makeup and partisan loyalties—lend further plausibility to the thesis that statehood was what led to the increased fiscal burden on their respective populations.

Skeptics can point to other factors that might explain, if data were available, at least part of the increased fiscal burden after 1912 in Arizona and New Mexico as opposed to that of Nevada. These might include variations in demographics or economic production, for example. Moreover, Prof. Moussalli clearly recognizes the limits of her study, stating that no generalizations of her thesis to other states can be made with measurable probability. However, it seems that, at the very least, the data she has uncovered justifies the further exploration of the fiscal impact of statehood in other parts of the United States, the extent to which contemporary observers recognized that impact, and the role that it played in debates over statehood.

The following comments focus on two topics particularly interesting from the historian's perspective. First, the circumstances surrounding the new states' collection of property taxes offer several parallels with taxation in English history, parallels that may provide a clue to the reasons behind the increased taxation following statehood. Second, *The Fiscal Case against Statehood* provides a clearer picture of the statehood process for American historians, who tend to overlook the impact it probably had on the inhabitants of territories.

Professor Moussalli describes in detail the mechanics of the assessing and collecting of property taxes in New Mexico and Arizona both before and after statehood and shows that both assessments and the efficiency of collections increased after 1912. Inhabitants of Nevada, by contrast, experienced only "slight increases" to their property taxes in 1914, increases they challenged in many cases by lawsuits. Moussalli concludes that "something about statehood caused much better financial reporting," which in turn resulted in the dramatically higher revenues in New Mexico and Arizona; at one point she suggests there was a sort of nexus of statehood and the general mood of Progressivism in the United States that led to these outcomes. Readers are left to wonder what exactly that "something about statehood" is.

The circumstances surrounding New Mexico's and Arizona's statehood are in some ways reminiscent of two important episodes in English history that were followed by greatly increased taxation of the population: the Norman Conquest of 1066 and the civil wars of the seventeenth century. Both events offer certain parallels with the process Moussalli describes in *The Fiscal Case against Statehood*. Following the Norman Conquest, William the Conqueror sought to assess the value of his subjects' wealth more accurately as a prelude to increasing his revenue. The results were the famous Domesday Book, which attempted to catalogue every manor, farm, and barnyard animal in the kingdom. William hoped to identify through the Domesday Book a larger pool of wealth to tax in the same way the new state governments wanted access to more wealth with their increased assessments. With this information in hand, William revived the *danegeld*, a land tax formerly paid by the Anglo-Saxons to their Danish overlords in the pre-Conquest period. William's successors imposed the *danegeld* annually until the 1160s.

In later years, the subjects of the English kings had some success in imposing limits on the taxes they were required to pay. At the beginning of the seventeenth century, assessment of property for the purposes of taxation was at a fairly low level, and students of the period often encounter assertions from modern historians that the subjects of the English king were "undertaxed." Some members of the nobility and gentry paid taxes on as little as 10% of their estates' value. Moreover, the English people and Parliament expected their kings to "live of their own," funding most of the ordinary operations of government from their own estates. Parliament was in the habit of granting kings a lifetime right to customs duties, but property taxes were only to be levied in extraordinary circumstances, usually during times of war. When Charles I ascended to the throne in 1625, Parliament even refused to vote him the lifetime customs revenue. Charles had to resort to reviving long-ignored taxes—taxes that were still on the books but had not been collected in centuries—to fund the monarchy's ordinary operations in the face of Parliament's reluctance to supply revenue. Most Englishmen viewed these taxes as extremely onerous, and anger over their collection helped lead to Parliament's revolt against the king in the 1640s.

The government's revenue situation changed dramatically during and after the civil wars of the 1640s. Parliament began extracting taxes from the English population at a rate several times greater than what Charles I had been requesting prior to the wars. Even after the restoration of the monarchy in 1660, the government's revenue remained at a level much higher than that of 1640, in large part because excise taxes became a permanent part of the tax structure. Then, following the "Glorious Revolution" in 1688, Parliament brought about another significant increase in revenue with the levying in 1692 of a permanent land tax calculated from rental values. What seems to have been a critical factor in these tax increases is the erosion of class antagonism between taxpayers and the government stemming from the gradual replacement of royal sovereignty with parliamentary sovereignty. This dynamic is familiar to many observers of modern democracy, where taxpayers are persuaded that "we are the government" and are willing to submit to higher levels of taxation in the belief that government revenue will be spent in the taxpayer's interest.

The purpose of rehearsing this history is to connect some dots that Prof. Moussalli's work suggests. Moussalli cites scholarship linking the rise of the British Empire in the eighteenth century to government clerks' meticulous recordkeeping, which helped to allay public fears of corruption and tyranny and lessen popular resistance to taxation. She goes on to write, "My argument is that statehood ranks with wars and emergencies as a creator of dramatically improved tax and government accounting procedures and reports." She also notes that the goal of improved accounting, from the State's point of view, is to increase its revenue.

Dr. Moussalli does not attempt to identify the precise "something about statehood" that leads to these outcomes. Perhaps statehood is similar to an event like the Norman Conquest or the Glorious Revolution in that it brings about a massive discontinuity in the ordinary functioning of government, and that this discontinuity provides an opportunity for financial and accounting reformers to introduce innovations designed ultimately to increase the revenue and taxing authority of the government. (Under normal circumstances, entrenched interests, public resistance, or bureaucratic inertia would prevent these innovations from taking place, but reformers acting with a ruler's blessing can avoid "letting a crisis go to waste.") The increased taxation can result simply from the government's identification of more taxable property held by the public (as after the Norman Conquest) or from reduced popular resistance to taxation brought about by increased confidence in the government, whether from greater transparency or a form of class solidarity (as in the years following the Glorious Revolution). Both of these dynamics were in play as New Mexico and Arizona became states. Perhaps Prof. Moussalli could comment on this idea and suggest how researchers might test it.

To illustrate the potential impact of *The Fiscal Case against Statehood* on our understanding of the statehood process, consider the typical survey textbook of American history. Virtually all of them follow a similar pattern. They devote a modest amount of space to the Northwest Ordinance of 1787, which established a process by which U.S. territories north and west of the Ohio River could become states; this process was later extended to all U.S. territories. Thereafter what we read of territories' transitions to statehood deals almost exclusively with statehood's national impact, particularly the balance between "slave states" and "free states" before 1860. Thus most textbooks dwell on the Missouri Compromise of 1820, the question of Texas's petition to join the United States in the 1830s and 1840s, etc., all from a "top-down" perspective. By contrast, they devote little or no space to the impact of statehood on the people actually living in the geographical areas concerned.

Two significant exceptions to this rule exist. The first is the account of the statehood process for Kansas in the mid-1850s. The bizarre chain of events resulting in the setting up of rival pro-slavery and anti-slavery governments seeking validation from Washington, D.C. merits discussion in most textbooks, as does the subsequent outbreak of violence between the two camps: "Bleeding Kansas," featuring characters like John Brown. Of course, Kansas's statehood ties conveniently into the larger narrative of slave states *versus* free states.

The second exception is the account of the controversial way in which Hawaii became a state following an unauthorized military intervention into the islands' internal affairs on behalf of American landowners. This intervention, ordered by the U.S. Minister to Hawaii, led to the overthrow of the Hawaiian monarchy and the establishing of a constitutional republic run by largely Anglo-American interests who inserted a standing clause in the new constitution providing for annexation of Hawaii by the United States. After a delay of several years, President William McKinley and the U.S. Congress followed through on annexation. Here again, the story of Hawaiian statehood as related by most textbooks serves a larger goal, in this case that of illustrating the phenomenon of American imperialism in the late nineteenth and early twentieth centuries.

One important thing missing from the received narrative of the statehood process is a critical examination of whether statehood, in general, was actually beneficial for the people living in the territories. In fact, as Dr. Moussalli relates, scholarship on all aspects of statehood is quite thin; only a handful of historians have even taken up the question. The unstated assumption seems to be that statehood was certainly beneficial. Perhaps scholars may be forgiven for making this assumption. After all, the Congress that passed the Northwest Ordinance and most Americans in subsequent years viewed statehood as a natural part of the development of a territory. No doubt the idea that a territory would remain under the direct control of the federal government even after reaching a population level suitable for statehood would have struck the founding generation as inappropriate.

However, the dramatic increase in the size and scope of government at all levels—federal, state, and local—since that time gives historians reason to ask whether adding a state government has always been a good deal for territorial inhabitants. Certainly it should make them curious whether territorial inhabitants argued the question among themselves.[1] In recent years the people of Puerto Rico have repeatedly declined to seek statehood, and it seems obvious that some of the same factors may have been at work in other territories that ultimately did seek statehood in the nineteenth and twentieth centuries. Prof. Moussalli shows that a great many inhabitants of New Mexico and Arizona were firmly opposed to statehood; moreover, she shows that their predictions of increased taxation turned out to be true.

I am curious to know Dr. Moussalli's thoughts on some issues relating to the statehood process. First, I would like her to elaborate on her statement that she believes that statehood was ultimately good for New Mexico and Arizona. In her view, what benefits outweighed the negative impact of increased taxation and regulation in the new states? Second, how should historians and political scientists alter their approach to the study and teaching of the statehood process in light of the fiscal and accounting evidence she has uncovered? Finally, she has shown that many people in New Mexico and Arizona second-guessed statehood after the fact and argued that it had been a bad idea. Now that the concept of secession seems to be creeping back into

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1 On a hunch, I asked two colleagues who have taught Alabama history for many years whether they knew of any work on the fiscal impact o statehood for Alabama or on whether fiscal questions influenced the debate over statehood. Neither was able to locate any information on the subject.

public discussion, not only in outlying states such as Hawaii and Alaska, but also in places such as Vermont, does the notion of the "price of statehood" have any bearing on that conversation?

Prof. Moussalli is to be thanked for her fine contribution to several academic disciplines in *The Fiscal Case against Statehood*. Hopefully it will serve as an inspiration and a model to other scholars who wish to approach familiar questions in new and interdisciplinary ways.

Reply to Comments on *The Fiscal Case Against Statehood: Accounting for Statehood in New Mexico and Arizona*

Stephanie D. Moussalli
University of Mississippi
Email: sdmoussa@olemiss.edu

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Abstract

This reply to the comments by Carden, Hall, Jewell, and Lemke on The Fiscal Case Against Statehood: Accounting for Statehood in New Mexico and Arizona first speculates on the individual and institutional impediments to the scholarly meshing of business and the liberal arts, thoughts inspired by Hall's comment. In response to Jewell, I consider how to integrate statehood into existing narratives in American history. I question part of Lemke's argument that my evidence supports a Tieboutian interpretation of the fiscal dynamics of statehood, speculate on Hall's question about why financial reporting improved after rather than before statehood, and discuss the merits of Jewell's identification of massive discontinuity as the cause for accounting reforms. I address the value of future research proposed by Carden on the possibility that "indigenous institutional forms" were upset by statehood and end by discussing the benefits and costs of greater self-government and better financial transparency.

Kudos to Art Carden for putting together this symposium. He has apparently decided to take his enormous talent for integrating diverse ideas and expand it to an organizational level, creating situations that encourage the rest of us to do the same thing. I'm honored that a historian (Jewell), an economist/economic historian (Hall), and two political economists (Lemke and Carden) should all comment so insightfully on this book.

Josh Hall's comments on the "sociology of graduate education" as it relates to cross-disciplinary work sparked a lot of thought. Hall notes the huge value the liberal arts can add to business research; liberal arts graduate students, however, often eschew business because they find current research in the area "narrow and boring." Probably true, but speaking as one with both liberal arts and business backgrounds, I find it stunning. Business in one form or another is what adult humans have always spent many, even most, of their waking hours on. It is an amazingly constricted view that denies that entire realm of human behavior to the curiosity of the liberal arts.

Hall asks what prevents more interdisciplinary inquiry. The two academic worlds – business and the liberal arts – are quite separate, both institutionally and in worldview. Economics spans both, being found in the liberal arts colleges of some schools and the business colleges of others. It doesn't fit perfectly in either place and is viewed with some suspicion by both, thus illustrating the problems.

Barriers to more integration exist on both sides. On the liberal arts side, how many scholars have ever taken the entry-level business course, principles of financial accounting? Accounting is literally the language of business. It opens up a world larger than that opened by the study of French or Arabic, but very few non-business academics have taken it.

The feeder level for the academic life is undergraduate studies. To be sure, liberal arts schools that facilitate business studies, like my own former institution, Rhodes College, see students flocking to business. But these are mostly students who prioritize the career flexibility and financial opportunities it offers. Certainly that's what belatedly drove me to accounting.

Botany or classics students, in contrast, are often driven by an intellectual passion to devote their lives to scholarship. Not much motivated (at least in their 20s) by the money a career may offer, I think they underestimate the information carried by prices. An accounting professor is paid much better than an art professor. Even liberal arts schools generally treat the one better than the other, no matter their stated intellectual preference for the arts. Business professors often have a lighter teaching load, lower research requirements, or more pleasant offices. They suffer less abuse from the occasional petty tyrants of academia, too, since they can more easily switch jobs.

All of these advantages make it easier for the business professor who enjoys research to pursue it where it leads him. And it can lead him to draw theory and subject matter from a discipline that originally attracted him more, be it music, chemistry, or psychology. I can't believe that young anthropology or political science students, understanding these facts of the scholarly life, would not seriously consider pursuing their studies from the comfort of a business department.

But not if they think business is a terminally boring subject. Many do, like the reviewer of my book for the *Southwestern Historical Quarterly* who wrote that it "has more than most anyone would ever want to know, even most accountants, about the fiscal affairs of New Mexico and Arizona …" (Reyhner 2013, 106). Ouch.

Of course, business erects its own barriers to interdisciplinary work. Many business schools will not accept articles published in non-business journals for tenure and promotion. The very fact that I've published a book instead of more articles is objectionable to some. Despite innumerable recent calls for diversification by the American Accounting Association, the top accounting journals continue to focus on just three or four areas of scholarship. If you want to do accounting history, someone at a research school once said to me, you'll have to do it after tenure. There is some truth in that.

Furthermore, because of the facts of the academic market, the liberal arts faculties at every level of higher education tend to have business school colleagues from far less prestigious programs. The liberal arts school that can afford to hire a literature professor from Yale cannot hire its new finance professor from there. This is not only disconcerting, but it also creates an intra-institutional mismatch in research talent.

Then there are the inherent hazards of interdisciplinary work. Despite having done a little doctoral work in history, I will never know as much about the history literature as Jason Jewell does. Which is why I'm flattered that he asks me how historians should "alter their approach to the study and teaching of the statehood process in light of the … evidence" in my book.

As Jewell notes, the only two cases in which American survey texts discuss the local impact of statehood movements are Kansas and Hawaii, because they illustrate larger themes the authors want to cover. Practically speaking, I think the story of territorial residents' opposing statehood due to well-founded fears of its cost needs to fit into a larger story, too. This might be the idea that the country's federalist system highlights the vast differences that typically exist between national pre-occupations and local ones. Progressive-era partisanship may have ruled Congress's decision about admitting New Mexico and Arizona, but inside those territories, people worried about whether they could afford a state government and whether the mines or the urban landowners would pay for it. The slave-free state controversy determined when Congress accepted Florida and Iowa into the Union, but within Florida the question turned entirely on the economic devastation wrought by the Seminole wars in the eastern part of the territory.

The story could also be cast in terms of the generally rising but unevenly distributed wealth experienced by 19^{th}-century Americans, a theme that many history texts do cover. Wealthier local populations were more likely to think they could afford statehood than were poorer groups.

On the other hand, wealthy but politically unpopular groups such as mine owners usually opposed it for fear of unfavorable tax reapportionment.

Furthermore, the Leviathan dynamic of statehood could be a useful check on presentism for the student readers of these textbooks. The amount of money involved in statehood was trivial by modern standards. It is a reminder to students (and the rest of us) that as recently as 100 years or so ago, most Americans had a very different view of how important and pervasive government should be.

In another illustration of the hazards of crossing disciplinary lines, Jayme Lemke uses her vastly greater understanding of the economics literature to poke salutary holes in my claim that Tiebout-style governmental competition cannot account for the evidence I found. I hasten to thank her for the comments; I was puzzled how best to apply the Tiebout paradigm to this situation, and she does a much better job. But I still question her argument.

As I understand Lemke's reasoning, 1) territorial politicians avidly sought the rewards statehood would bring them. Since a majority vote from the locals was required to obtain the prize, politicians were under great pressure to please them. So far, I agree. 2) Territorial politicians were under more pressure than were state leaders, who had already won the prize of statehood. Maybe so; it seems plausible. 3) This pressure required territorial politicians to compete with other jurisdictions to attract people, because Congress required a certain population before admitting a new state.

This last leg of the argument is what seems wobbly. It's true that Congress always had a population requirement for territories to become states. However, the requirement was not always necessary in practice. Nevada had less than one-third the population required by law when Congress admitted it in 1864. Certainly, more people typically bring more wealth and more revenues for the government, but this is as true for states as it is for territories.

And after all, is jurisdictional competition for population necessary to the argument? Isn't item 2 above sufficient? That is, territorial politicians desiring the prize of statehood needed to refrain from alarming the locals by excessive government growth because a popular vote favoring statehood was required (generally; Nevada may have been an exception here, too). Some local caution added to the federal government's fiscal braking role that I discuss in the book may explain the relatively low fiscal bite taken by the territories.

Josh Hall speculates about the other part of my work – what prevented the territories from dramatically improving their financial reporting *before* statehood, as they did afterwards. I suspect the biggest practical reason was that, to legislative bodies lacking ultimate responsibility for revenues and spending, good fiscal information was not particularly important. Congress in the end would bail them out if it couldn't squash their excesses.

It is also possible that the population actually preferred some lack of efficiency in government operations. Recall that better tax assessments, including the publication of 100%-of-true-value assessments, only occurred upon statehood in New Mexico and Arizona, and then inspired massive popular protests. Such dramatic technical improvements did not occur at all in Nevada, which had not changed its status, at the time. Could it be that the public willingly trades some public accountability for restraints on the size of government?

This brings me to Jason Jewell's identification of a "massive discontinuity in government" as the factor that allows the huge public accounting reforms to occur that are the technical prerequisite to the much greater government expropriation that follows. I like this argument; it meshes well with Waymire and Basu's "punctuated equilibrium" model (2011) for accounting change (see pp. 166, 173, 183 in my book). It may help explain why some leaps in F endure (William's seizures after he conquered England) and others do not (George's attempt to take more from the American colonies).

However, I think the increase in sovereignty in New Mexico and Arizona was also crucial; the governments required the additional powers of statehood to seize a larger share of local resources. This could be tested by studying Texas. Texas, a sovereign nation before its union with its larger neighbor, saw its sovereignty plummet upon statehood. My theory predicts that F also fell and the quality of its financial statements either fell or held steady. Texas needs to be studied.

In fact, all the post-Revolution states and territories of the United States should be studied, as Carden and Hall both point out. I hope to work on a few of them.[1]

This, unfortunately, is where reality sets the brakes. It took me a year to create the data sets for just three states. Sylla, Legler, and Wallis (1995) used graduate students to create revenue series for a number of states, but that might not speed the process much. If the governments themselves did not report bond activity and interfund transfers separately, the Sylla team did not re-calculate revenues to exclude them. Nor did they consult multiple government sources as a validity check, estimate true property values to control for the size of the economy, or systematically observe the quality of the financial reports. Graduate students qualified to do these things would probably need to be experienced accountants; to do the entire set of governments would require access to an accounting doctoral pool and a multi-year research project.

Might enough benefits result from such an endeavor to justify its cost? I think so. In addition to the items for future research I discussed in the book, Art Carden identifies another set: what "indigenous institutional forms … might have been upset by" statehood? Did statehood "merely crowd out existing institutional forms that had evolved in response to conflicts over

1 Next on my list is Mississippi, which will take me to the early decades of the 19th century, a period when accounting practices may not have been under such general pressure to improve. Thanks to Hall for mentioning the need for such a case in future research; it is an advantage of tackling Mississippi I hadn't thought of.

water and mineral rights," as Carden speculates, including those created by Native Americans? Answers to these questions would add a great deal to our understanding of U.S. history at the local level. The research would also cast useful light on current issues of sovereignty such as those involving U.S. territories, the perennial movements in the United States for parts of existing cities or even states to form separate governments, and other such movements in India, Canada, sub-Saharan Africa, the Middle East, and, in the recent past, the republics of the former Soviet Union.

The hardest questions are normative ones, and here, research would create better-informed debates. Why, asked Jewell, do I still favor statehood and more efficient and transparent accounting, in light of their likely role as enablers of a much larger government than most people, even among statehood supporters, probably wanted?

Basically, because I prefer more self-government to less and more information about the government's use of the public purse to less, *ceteris paribus*. Governments tend to abuse their powers and should be easily checked by the people. This is a benefit of self-government to the local population. For the nation, it is vital that the country not have an imperial form, with a small number of elite polities ruling a large number of forever subordinate colonies. As for the benefits of transparent fiscal operations, it is much harder for a government to engage in corruption or completely dishonest spending if the information is routinely made public.

On the other hand, neither increased self-government nor more extensive, reliable financial information and procedures are unalloyed goods. We have long limited democracy for a variety of good reasons, as in the imposition of term limits, of representative instead of direct democratic decision-making, of bills of rights, and of constitutions protected from frequent change by super-majority requirements.

It is equally essential to limit the length and level of detail of financial statements, and to limit the extent of audits of these statements, if the statements or the audits are actually to convey information usefully. Interesting recent accounting research has focused on just such issues (e.g., Arya and Sunder 2003; Messner 2009). Could it be that New Mexican and Arizonan opponents of statehood knowingly tolerated some lack of local democratic decision-making power and some muddiness in fiscal procedures and reports in exchange for the checks that these features imposed on the overall power of the government? A disquieting question.

In all the U.S. territories, people worked out answers to these issues. Their experiences can illuminate these dynamics for historians, political scientists, economists, and accountants. And what fun it would be! The fruitfulness of the comments by Carden, Hall, Jewell, and Lemke demonstrates that research plums hang low all around us, awaiting the synergies of multiple disciplines and methods to pluck them.

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Ali Kabiri, ***The Great Crash of 1929: A Reconciliation of Theory and Evidence***

David Howden
Saint Louis University – Madrid Campus

Ali Kabiri, *The Great Crash of 1929: A Reconciliation of Theory and Evidence* (London: Palgrave Macmillan, 2015)

Does the world really need another book on the Great Depression? Ali Kabiri, lecturer in economics at the University of Buckingham, trudges into disputed territory but holds the reader´s attention by making three new contributions. First, he has collected new data on dividend yields and growth rates for U.S. common stocks during the 1920s to augment the plethora of existing price data. Second, Kabiri constructs a valuation model for the aviation industry of the time, which he roughly equates with modern technology stocks. Finally he uses the returns for a new investment vehicle, the closed-end fund, to illustrate actual investor returns free of any survival bias. Taken together, the three innovations allow the reader to come to grips with the financial side of the crash of 1929, instead of simply rehashing the more common macroeconomic side.

In explaining the correction in stock prices during 1930-32, Kabiri sees two factors as relevant: the fall in demand from the Great Depression and the banking crisis, which ultimately saw over 9,000 banks close their doors during the 1930s. Neither of these is especially controversial, and Kabiri does the reader a great disservice by not pushing his reasoning further. For example, by applying the logic of a basic dividend discount model, he sees the fall in stock prices as a rational (though unforeseeable) response to the decrease in earnings, and ultimately, dividends. It´s easy to blame the fall in earnings on the economic decline of the 1930s, but that only pushes the problem back one step. One cannot use the fall in demand from the Great Depression as a cause of the stock market crash while also using the fall of stock prices as a cause of bank instability causing the Great Depression.

A more fruitful approach is found in Rothbard (1963), who uses Austrian Business Cycle Theory to explain why the boom of the 1920s sowed the seeds for the subsequent depression. Kabiri uses a similar approach as Rothbard by making use of the boom of the ´20s as the origin of the troubles later on. Unlike Rothbard, however, he does not place much emphasis on loose credit conditions or the Federal Reserve´s easy monetary policy. Despite reviewing the literature and commenting that "[r]esearch conducted at the time and more recently points to the large dislocations of the old monetary system of the world and the emergence of a more fragile one" (p. 71), he fails to integrate these findings in his own analysis. While discussing the change in the composition of debt from corporate to consumer, Kabiri remains mostly ambivalent on the effects, commenting that such a change "may have made the depression worse" (p.29). This approach overlooks the role that short-term economic growth, promoted by an inflationary monetary policy, had in inflating investor expectations about returns and thus propagating a stock market boom. The reader should not overlook the fact that at a discount rate of 7%, the P/E ratio of a stock would increase by 50% if dividend growth was expected to increase from 4 to 5%. Kabiri does not pursue the linkage between expectations, business conditions and monetary conditions, and this omission leaves the reader yearning for answers.

Kabiri gives the reader a unique financial perspective on the boom of the 1920s, but at the cost of not paying enough attention to the real economy. This is unfortunate, but no more so than most other books giving nearly exclusive attention to the real economy at the expense of the financial arena.

It is also unfortunate because Kabiri roots his analysis in a belief that markets are efficient, and that the stock market bust was mostly unforeseeable. Attention to the type of economic boom the country experienced during the 1920s would shed light on its ultimate unsustainability. More importantly, the link between rapidly changing expectations and share price changes are only properly analyzed, so it seems to this reviewer, during stock market declines (e.g., Black Monday 1987 has commonly been explained as a rational pricing response to an expected decline in the dividend growth rate). Kabiri missed a chance to apply the same logic the other way ´round. It is just as possible, indeed likely, that rapid increases in stock prices are a response to expected growth in earnings or dividends. At the end of the day, Kabiri gets half-way to the cause of stock market crash but that is not sufficient to convince this reviewer that he has all the answers.

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Edward Peter Stringham, *Private Governance: Creating Order in Economic and Social Life*

Caleb S. Fuller
Grove City College
Email: fullercs@gcc.edu

Edward Peter Stringham, *Private Governance: Creating Order in Economic and Social Life* (Oxford: Oxford University Press, 2015)

As a competitor in junior tennis tournaments, I recall the surprise many expressed upon learning that most matches have no third-party oversight. This lack of enforcement notwithstanding, participants contest thousands of matches annually—most without a dispute.

In Edward Stringham's thoroughly-documented book, *Private Governance: Creating Order in Economic and Social Life*, he explains the shock I described above: private governance works so seamlessly that most people—even professional economists—overlook it, all the while attributing social order to formal law. This "legal centralist" perspective, though all-to-pervasive, is reduced to tatters by the final pages of Stringham's book.

Part one of *Private Governance* argues that individuals form clubs to provide governance, part two examines "privately governed markets," (p. 37), and part three concludes with additional insights about the market for governance, the relationship between market and state, and the promise of future research.

As Stringham notes in part one, economists are correct to recognize the widespread potential for conflict; they are incorrect, however, to assume a third-party monopolist (i.e. government) must intervene for cooperation to emerge. The greater the gains to be had from cooperating, the greater the incentive for exchange partners to innovate solutions to governance problems. One way that individuals supply governance is by forming "clubs" with rules that govern member behavior. These groups punish deviant members by excluding them from access to club benefits. Unlike government, club participation is voluntary; furthermore, club members can use economic calculation to compare the benefits of an additional rule with its attendant costs.

After laying the theoretical foundation, Stringham illuminates several painstakingly-researched examples of club governance. He explains the emergence of the world's first stock market in the Netherlands, the origins of the New York and London stock exchanges, as well as London's Alternative Investment Market. These financial markets developed in the absence of state enforcement, instead relying on reputational mechanisms and exclusion of fraudsters. In fact, once off the ground, they were often forced to contend with ignorant and heavy-handed government regulators.

In chapter 7, Stringham shows how PayPal—on the verge of extinction due to fraudulent transactions—tackled their governance crisis without waiting for the help of slow-moving government. In fact, at the time of PayPal's problems, there were already extensive (and ineffectual) cybercrime laws on the books. PayPal's leadership solved their problems by implementing algorithms to detect fraud *ex ante* rather than waiting for government to rectify their issues *ex post.* In the next chapter, Stringham addresses the private police that have patrolled San Francisco since the Gold Rush. As he documents, even when local government began supplying a tax-funded police force, many residents voiced their preference for their voluntary counterparts.

In a chapter that promises to make Neoclassical economists uncomfortable, Stringham draws on moral philosophy and psychology to argue that one of the most powerful—and overlooked—mechanisms of private governance is *self*-governance. Whether because of Adam Smith's "impartial spectator" or due to cultural norms such as religion, evidence indicates that many individuals cooperate even in the absence of external constraints.

Finally, Stringham tackles two hard cases. The first challenge consists of markets "requiring" third-party adjudication, such as seafaring merchants who repeatedly came to one another's aid prior to contractual negotiations. Voluntary solutions enable disputants to select private adjudicators skilled in the relevant area of commerce; by contrast, one-size-fits-all government regulators often demonstrate a knowledge-base bordering on cluelessness.

The second hard case stems from the most complex markets: are private arrangements sufficient to handle the messy world of mortgage-backed securities and credit default swaps? In fact, Stringham shows that private governance is up to the task by not only highlighting innovative private arrangements, but also dispelling popular financial myths in the process.

As Stringham emphasizes in his concluding chapters, not only does private governance work, but the best-case scenario for government is matching the performance of markets (thereby crowding out private solutions); in many cases, however, government imposes a litany of new compliance costs that dwarf any attendant benefits.

One small criticism of Stringham's book is that it might have been more comprehensive. While he draws upon the world of finance for several of his chapters, he alludes to the fact that private governance is everywhere. It would be nice to see an expanded version of this book that includes more far-ranging examples, specifically some drawn from modern life (the chapter on PayPal is great, so it would be instructive to see other cases of private "cyber-governance.")

Stringham ends by urging economists to recognize that the "unseen beauty that underpins markets" (p. 226) also grounds markets for governance. Even if private governance was responsible only for the examples he describes, "its accomplishments should still be considered among the greatest achieved by humanity," (p. 228). The prospect of future research in private governance, though, is limitless says Stringham, and so he concludes with an optimistic call for others to join him in exploring how cooperative interaction is solving our biggest problems. One can only hope that many heed the call.

Steven Kates, *What's Wrong with Keynesian Economic Theory*

David Howden
Saint Louis University – Madrid Campus

What's Wrong with Keynesian Economic Theory, edited by Steven Kates. Cheltenham, UK, and Northampton, MA: Edward Elgar, 2016 (xii + 277 pages).

What's wrong with Keynesian economic theory? asks Steven Kates. In this edited volume the Associate professor at Australia's RMIT University gathers an eclectic mix of economists to outline their misgivings of Keynesian theory. The result is a concise and handy reader that comprehensively overviews the areas where Keynesian economists run afoul.

There is not much that is groundbreaking in the work, with many of the criticisms variations of or developments to arguments found in Hazlitt (1959) and Hutt (1963). Furthermore, Kates own recent two-volume work *Keynesian Economics and its Critics* contains over 1800 pages delving into what must be every anti-Keynesian argument ever made (Kates 2016). While these two volumes are more complete than the present work, 1800 pages is no doubt unwieldly for the average reader and one of Kates' goal in *What's Wrong with Keynesian Economic Theory* is to make these arguments more accessible to the lay reader. In this he, and his contributors, are wildly successful.

The common denominator uniting most of the criticisms is two-fold. First, the Keynesian notion that Say's law – that supply is a precondition for demand – gets the problem precisely backwards is exposed as the fallacious claim that it is. Several contributors unmask the fallacies of the Keynesian idea that demand creates supply offering points made over the past thirty years by various supply-side economists. The second common denominator of the contributions is the idea that the specific flavor of "macro" that Keynesian macroeconomics makes use of is more harmful than helpful. These arguments are more nuanced views on the problem since Hayek (1931) first formulated it, and form the core of the book's thrust against Keynes.

Of the arguments centered against undue aggregation in Keynesian theory, Mark Skousen's chapter goes the furthest in offering an alternative. In a brief elaboration on his idea of gross output which has recently caught traction as the BEA now publishes quarterly data, Skousen outlines not only why looking at total expenditures is important, but also the economic fallacies that result from focusing narrowly on final expenditures (e.g., GDP). In this chapter, Skousen provides the most concise methodology on how to calculate gross expenditure from existing expenditure categories. He also reiterates his now common point that business spending (including investment and B2B transactions) represents the majority of economic activity, amounting to double that of consumer spending. Such a point is important to combat the widespread fallacy that the consumer is the driving force of the economy, a belief entrenched firmly from since Keynes's claim, contra Say's law, that demand creates its own supply. While this point is important, one cannot help but feel that the Keynesian economist will not be swayed by the argument and instead will see Skousen's contribution as supporting material for one of Keynes's central claim: that the unpredictable vicissitudes of investors – proxied here as they are through business expenditures – are the main instigator of the business cycle. When investors lose their nerve, business spending falls and the economy slumps. The obvious missing link is that the choices of investors are not unpredictable, but are instead guided by prices (including interest rates) and resultant profit figures. Understanding why businesses refrain from spending is the fodder of many other chapters and Skousen would do well to elaborate on this point with respect to his use of gross output figures to demonstrate where the problems of the recession lie, and how the path to recovery will proceed.

Like most edited volumes, this one faces deficiencies. The academic level of the chapters is not uniform. Some chapters are fodder for higher-level economic theory, while others are more practical, aimed at the practitioner or lay reader. This quibble is forgivable but the formatting of the book leaves much to be desired. Section headings are not uniform, and there is no standard citation or reference format. The use of endnotes is keeps the reading quick, but for the reader who wishes to delve into the notes it requires a lot of page turning. At first glance the book is easy to read, but it becomes a bit of a chore if one wishes to get everything that it has to offer from it. This is a shame since there is much good content in the book that should be easily accessible.

There is an air of fatality to the book. Several contributors note that the arguments against Keynesian theory are not new, nor does the fact that reality does not match theory go unnoticed amongst policy makers, lay people and even economists. Yet, the textbooks are set and each new generation of economists learns the same basic facts, that income equals expenditure and that supply creates demand, before they are able to assess what they are learning with a critical eye. What is needed to correct the course is a change not in higher level understanding of economic theory, but at the introductory level. In making this collection concise and accessible, Kates has taken a step in that direction.

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George Bragues, *Money, Markets, and Democracy: Politically Skewed Financial Markets and How to Fix Them*.

David Howden
Saint Louis University – Madrid Campus

Money, Markets, and Democracy: Politically Skewed Financial Markets and How to Fix Them, by George Bragues. New York: Palgrave Macmillan, 2017.

How politically independent can financial markets be? Not very, at least not if history is any guide with abundant recent examples including the Federal Reserve's quantitative easing programs, the U.S. Treasury's various bailouts of financial companies or the European Central Bank's large-scale asset purchases. This realization forms the first of three central theses in George Bragues' *Money, Markets, and Democracy*.

The intertwined world of politics and finance rests on some pretty obvious, if not always well-accepted facts. Anecdotally, one can look at the commensurate rise in both the political and financial components of the economy by pointing to each sphere's growing share of GDP (pp. 8-9). With a little effort, the well-read layman should be able to see that since the central bank is able to control the economy's base rate of interest, by extension it also influences earnings growth and dividend yields of private firms. Less well acknowledged, though expertly navigated by Bragues, is that the burgeoning derivatives market is mostly a response to uncertainties created by the government and that this sector is now so large that 1) government bailouts like we witnessed in the recent financial crisis are widely seen as "necessary" and 2) the derivatives market has enabled the public sector to lower its borrowing costs and thus grow larger than would otherwise be the case (p. 223).

These three themes linking finance with the government may not seem entirely novel, depending on the degree to which the reader has followed the history of financial booms and busts. It is in explaining why this link between politics and financial markets formed, and is getting stronger, that Bragues makes a unique contribution to not just economic theory, but also to political science.

Bragues' second thesis is that the growth of the financial and political sectors over the past three-hundred years, as well as their increasingly close-knit nature, are a response to a growth in democratic institutions. As most democracies are elected by majorities, enticing a larger portion of voters to cast their vote for than against you is central to coming to power. Since there are inevitably more tax consumers than payers, the government can elicit votes by making their services look cheap (i.e., make its operations look efficient), usually by running budget deficits and borrowing to cover any present funding shortfall (p. 73).

Since the rise of democracy will also coincide with an increase in public indebtedness, financial markets to service such debt become instrumental to the very existence of the government. Not only is the government dependent on the financial sector's ability to attract other peoples' savings to service this debt, but the financial sector is dependent on the government for tax-funded bailouts when things get out of hand.

What this reviewer found especially enlightening is Bragues' discussion of the social factors both responsible for the growth in democracy, as well as the ill-effects resulting from it.

Natural law as the foundation of our legal, political and ethical systems has been steadily displaced by utilitarianism, especially since the end of World War II (p. 43). Using "the greatest good for the greatest number" as the benchmark to judge whether an action is beneficial has promoted the rise of democratic rule even when demonstrably negative outcomes for the vast minority of citizens is apparent. (If one were forward looking enough, democratic intuitions also often imperil the greatest good for the greatest number if that number includes future generations. Social security springs to mind, and other examples abound.)

Most importantly, democracy has allowed for a change in societal values – away from morality and towards commercial activities which will fund material existence by way of creature comforts (pp. 90-91). Debt, and especially public debt, has arisen to allow a de-coupling of income and expenditures. One apparent result is that people have shifted their focus away from pursuing a moral existence to pursuing more superficial ends.

One can quibble at this point that we should not judge the importance of ends, the morality of actions, or whether there is a real or rather only an apparent conflict between markets and morality. Bragues' great contribution is that since so many *do* see a conflict between these two spheres – the moral and commercial – there are definite though difficult to discern reasons for this conflict. Understanding the link between democracy and finance is a necessary step to understanding this conflict.

One could also quibble, as this reviewer does, that Bragues overstates the direction of causation in claiming that the rise of democracy has promoted the shift in values and the rise of the financial sector. For example, is it true that the rise of liberal democracies not just coincided, but also promoted the allowance of usury (p. 87)? While there is no doubt that democratic pressures to permit interest so that income and expenditures could be separated played a role in changing religious, moral and legal attitudes to usury, it must also be admitted that this shift emerged slowly over centuries of arguments for and against the practice. Formal debt markets were just the culmination of a thousand years of slow progress in religious, legal and economic attitudes to interest as detailed extensively by Rothbard (1995).

Bragues' third thesis is that removing politics from financial markets must start at the source, namely, by reintroducing a hard-money standard that will take control of this most basic economic institution away from government hands. Obvious benefits include shifting resources to better ends and away from artificially created markets that facilitate the link between the government and financing (e.g., the derivatives market); more stable interest rates since deficit spending will be discouraged; less volatile and unpredictable rates of inflation; and a steadier stock market – both on the up and down side – which will bring forth less apparent "need" for government interventions when fortunes evaporate.

Bragues argues that a gold standard would tie the government's hands and force a return to less profligate times. However, any law is only as good as the paper it's written on. The Eurozone operates in a manner similar to a gold standard to the extent that no member country can unilaterally control its money supply (Huerta de Soto 2012). This similarity notwithstanding, laws introduced to promote economic stability, like those outlined in the Maastricht Treaty, have since been broken and today the link between various European governments and financial markets is stronger than ever.

Bragues ends with a more fruitful approach. Public opinion must be changed in order for public policies to follow suit (p. 284). The period of 1871-1914 witnessed broad-based democratic participation constrained by a gold standard. It was also a prolonged period of economic and technological growth. This reviewer doubts that democracy is fading any time soon, and if it does the medicine may well be worse than the disease. Altering public opinion to take money out of the government's hands is a more feasible approach that promotes the same ends. This is a challenging task, but Bragues provides some of the necessary fodder to impress this point on the public.

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