Stupid Public Policies and Other Political Myths

or

(Ten Stupid Things that Government Does that Don't Work and Waste Your Money and Why They do Them)

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Abstract

American public policy is cloaked in many myths. Encompassing such issues as public subsidies to sports stadia, enterprise zones, and welfare migration, there are many ideas recycled from government to government over time with little thought given to the evidence supporting their empirical assumptions or their prospects for success. In light of the Obama administration's plans to develop an economic stimulus package for states to implement, this paper looks ten policy myths and bad ideas. The argument is that before states spend money they need to be more attentive to what social science research says regarding what types of programs are effective.

Introduction

President elect Barack Obama's plan to spend $700 billion or more to stimulate the American economy raises a host of interesting question about how to spend that money in order to achieve the proverbial "biggest bang for the biggest buck." In contemplating this stimulus plan for the economy significant talk has centered on perhaps giving this money to states for them to spend on "shovel ready" projects that could potentially generate up to 2.5 million jobs. But the spending of this money and giving authority to the states to decide which projects to spend them on raises two questions. First, how good are states in spending money to encourage economic development or engaging in good policy making? Second, are some types of projects better than others to help the economy?

States often are viewed favorably as innovators in public policy. Justice Louis Brandeis' famous and often repeated quote in *New State Ice Company v. Liebman*, 285 U.S. 262 (1932), described states as laboratories of democracy where "a single courageous state may, if its citizens choose, serve as a laboratory; and try novel social and economic experiments without risk to the rest of the country." Similarly, Carl E. Van Horn, in lauding the perceived [re]newed capacity of states to innovate and experiment in public policy, asserts that: "Today, at the beginning of the twenty-first century, state governments are at the cutting edge of political and public policy reform" (VanHorn 2006: 1). In an era when devolution and federalism are the buzzwords among some policy makers, and at a time when others views lawmakers in Washington as hopelessly deadlocked in partisan battles, it is no surprise that the attention has shifted back to states to drive the policy process.

Yet while states by default or design may be forced to drive policy making, questions about

their capacity to innovate and implement are still open to debate. In particular, even though Elazar (1984) has asserted that states have different political and legal cultures that affect how their institutions are set up and operate, and Putnam (2000) has argued that the levels of social capital across the country demonstrate contrasting capacity perform, one can still question whether states truly are the laboratories of democracy that Brandeis waxed over.

When states make policy, the reality may demonstrate more often than not that they are less laboratories of democracy and more factories of replication. For example, many state legislatures are not professional or full time, or they lack extensive research staff to undertake policy work (Rosenthal 1998). The reality may be that often the practice one state adopts when seeking to make policy is to ask if another has already enacted it. If so, often that policy becomes adopted subject to minor modifications. Additionally, states may find out about legislation at conferences, such as the National Council of State Legislatures, where they hear about or see programs that have been adopted elsewhere. The point here is that states may be creatures of "me tooism," repeating and replicating policy initiatives found in other states, often adopting them without asking if in fact they work. One of the darker or unfortunate sides to this policy replication is that ideas adopted in one state that do not work might also be adopted elsewhere, only to reproduce policy failures across boarders.

This paper examines state and local governments borrowing ideas from one another, producing what shall be called "stupid public policy." American public policy is cloaked in many myths. Encompassing such issues as public subsidies to sports stadia, enterprise zones, and welfare migration, there are many ideas recycled from government to government over time with little thought given to the evidence supporting their empirical assumptions or their prospects for success.

Like bad meals that repeat on a diner, or a vampire who never dies, these ideas too are recycled and never seem to go away. This paper examines several of the leading ideas and myths dominating American policy making, exploring the reasons for their repetition and the evidence for their failures. The real question raised here is simple: Why do governments consistently enact policies and laws that have already been shown to be failures?

II. The Patterns of Policy Process

"'Innovation' [is] an idea perceived as new by an individual" (Gray. 1973, 1174). Yet however much Brandeis' laboratories of democracy image is invoked, seldom has policy innovation and genesis been studied, at least from the point of view of examining the role that imitation takes on. Instead, a host of related issues are generally explored. Additionally, the genesis and perpetuation of bad policy ideas is not an object of inquiry in policy studies.

One set of literature explores the agenda setting process and how ideas surface, and foment. John Kingdon's *Agendas, Alternatives, and Public Policies* (1995) seeks to explain why specific policy ideas make on to the agenda. He argues that one needs to look to policy windows and entrepreneurs where three streams—problem, policy, and politics—convergence. Specifically, while many worthy ideas might merit consideration and compete for space on the limited platform for Congress or legislatures to consider, successful policy makers or entrepreneurs benefit from the luck of a specific issue being perceived as a problem, a particular policy being seen as an appropriate to it, and as political timing making consideration of the problem and policy salient.

Cobb and Elder (1972), like Kingdon, seek to understand why some ideas are thrust on to the policy agenda. They argue that triggering devices are critical to that occurring. For example,

external events, such as wars or new international conflicts can place items on the agenda. The events of 9/11 placed terrorism on the national and local policy agendas, as did the launching of Sputnik backing in 1957 place science and math on the education agenda. Triggering devises can also be internal events. For example, the rise of AIDS as a health threat in the 1980s, or domestic abuse in the 1970s as precipitated by the women's movements of that decade, both fit this bill.

Anthony Downs (1972) looks at agenda setting and issue identification in yet another way, seeing a pattern to how most policies are addressed. His issue attention cycle starts with a pre-problem stage where a phenomena is not defined or seen as a problem. At some point an event, perhaps a triggering device, as described by Cobb and Elder, leads to an alarmed discovery and euphoric enthusiasm that a problem exists and that the government can in fact do something about it. However, in stage three the public and policy lawmakers come to realize the cost of significant progress, or see that addressing a problem, such as eradicating poverty, will not be quick, easy, and cheap. As a result, there is a gradual decline in public interest in addressing the problem, and finally the cycle moves into the post problem stage where almost like the hero in an old western, the policy and issues surrounding it fade off in the sunset. Downs' model, describing policies almost in Warholian 15 minutes of fame logic, makes the five stages of the policy issue attention cycle feel like Elisabeth Kübler-Ross' (1973) five stages of dying—denial, anger, bargaining, depression, and acceptance. Other writers, such as Jones (1977) and Pressman and Wildavsky (1973) also examine agenda setting and the difficulty of getting ideas into legislative consideration.

Finally, while the policy literature often tries to explain why items make it on to the agenda, Bachrach and Baratz (1962) became interested in explaining why some items are kept off it. Their "Two Faces of Power" was a groundbreaking essay describing non-decision making. For Bachrach

and Baratz, the ability to put or not put issues on the agenda is a powerful way to influence debate. Moreover, non-decision making is still a form of policy making, especially if done repeatedly and consistently over time, when done as a result of decisions by powerful interests or elites. For example, the general lack of regulation of tobacco products, firearms, or significant reform in health care delivery in the United States may perhaps be seen as the result of non-decision making at the behest of the cigarette industry, the National Rife Association, and the private insurance lobby as well as the American Medical Association, respectively. In parallel fashion Schattschneider's (1960) concept of the mobilization of bias in American politics is also supposed to explain why certain issues of interest to one social economic strata are given consideration in American politics, whereas others are not. In fact, the entire pluralist school and its critiques offers suggestions on how the bargaining process among interest groups explains what issues appear or disappear from the policy frontier (Truman 1971; Schattschneider 1960; Lowi 1969: Key 1969: McConnell1966; Dahl 1971; Dahl 1976: Dahl 1979; Dahl 1989; Baumgartner and Leech 1998; Ziegler 1964).

Finally, when it actually comes to evaluation and impact, there is a rich literature in the policy analysis field. However, within this field, there is a significant debate over what role analysis should have in the policy making process. On the one hand Wilson (1887) articulates the classic politics-administration dichotomy, seeing in the former as the realm of values and the latter the world of scientific rationality. There is a hint here that the two should not be joined, at least in the sense that politics is about value production and thus not readily informed by the kind of research done by administrators. Daniel Patrick Moynihan's (1969) *Maximum Feasible Misunderstanding* is the most clear statement on this point where he asserts: "The role of social science lies not in the formulation of social policy, but in the measurement of its results" (193).

Moynihan's argument that the policy process is not driven by social science research is indebted to the fact/value distinction articulated by David Hume (1980). However, others reject this divorce, arguing instead that policy making should be social science-driven if the government is to improve it performance and outcomes. Alice Rivlin's (1973) *Systematic Thinking for Social Action* is the classic book on this point, lamenting the ignorance about service delivery and arguing that we ought to use federalism and random innovation to ascertain what policies work (86-90). Others such as Tufte (1974), Rein (1976), and Lindblom and Cohen (1979) reach similar conclusions. For Rein (1976: 254-260) there is an inextricable connection between how values structure facts and the latter help us to understand the former. Lindblom and Cohen (1979: 16) see professional social inquiry as providing knowledge for social problem solving. Finally, Tufte (1974) demonstrates how empirical knowledge can facilitate policy choices.

What this latter group of writers seems to be hinting at is perhaps most closely related to the issue or main theme of this paper. First, they split over whether and how social science research should impact the policy making process. Second, they seem to hint at a gulf in the policy field. By that, there are many policies that have been heavily and repeatedly researched by social scientists and a significant amount of data are available either to indicate what works or not, or at least to help frame the debate about certain issues. Yet in many cases, despite the social science research, elected officials and policy makers tend to choices as if in denial. The debate, as magnified here, is much like the debate over policy making or the budget; are they rational adventures or do they appeal to emotions and politics.

Overall, while policy making, especially at the state level, shows a capacity to change and innovate, the nature of that innovation is often understudied. Ignored is how much states act as

copycats to replicate what has been done elsewhere, and also overlooked is how this type of policy making is often done unreflectively, adopting proposals that have been tried elsewhere and failed. Perhaps this is done either with ignorance of this fact or with the eternal hope of a Chicago Cubs baseball fan that this time it will be different.

III. Stupid Public Policies and Political Myths

Stupid public policies and political myths often appear to capture the imagination of political elites. By "stupid public policies" is it meant policies that are repeatedly proposed and which continuously fail, even though there is social science research indicating that these policies would not work. In addition, "political myths" refer to ideas which, like the proverbial urban folk legends that seem to circulate everywhere, are often repeated or held up as true, even though there is no hard evidence to support them or worse, data contradicting them. It is bad enough that one state or policy making unit would fall prey to these policies or myths, but if states are less the laboratories of democracy and more the factories of invention than thought, then these ideas may well replicate themselves over and over, never to die the deaths they deserve.

Unfortunately there are too many stupid public policies and political myths to count, necessitating an eventual book to catalog them. Yet this section of the paper seeks briefly to explore ten of them. In an effort to provide guidance to a new president, Congress, and perhaps states as they think about how to spend their money for a variety of public policy purposes, even beyond the possible windfall from the Obama economy recovery plan, here are ten bad ideas that should be avoided.

1. Tax incentives are needed to encourage business relocation decisions.

Perhaps at the top of any list of political myths is the idea that taxes, including their incidences and incentives, are serious factors affecting business relocation decisions (Snell 1998). At the core of this belief is the idea that businesses make decisions about where to locate a facility based primarily or perhaps even exclusively upon taxes. As a result of this belief, state and local governments have engaged in dramatic tax wars against one another in order to lure businesses to their community. What do we really know about the impact of taxes upon business relocation decisions?

The literature is clear—tax breaks to encourage economic relocation are economically inefficient and wasteful. Hundreds of studies, including a bevy of them cataloged in *State Tax Notes*, reach this conclusion (Anderson and Wassmer 2000; Bartik 1991; Bound, Jaeger, and Baker 1995; Fisher and Peters 1997). When businesses are surveyed regarding factors important to their economic [re]location, taxes often come in way behind proximity to markets, suppliers, and the quality of the labor force (Wasylenko 1997). None of this should come as a surprise. Each of these other factors occupies a larger percentage of a business's budget than do taxes, and all of them are far more critical to the long term success of a business than are taxes. Moreover, when pressed, businesses will actually admit this. For example, nearly 62% of those interviewed in a California study on hiring tax credits indicated that they had never or rarely affected their decision to hire individuals. In the same study, nearly half of those interviewed stated that tax incentives for relocation did not affect their decisions (California Budget Project 2006; Hissong 2003). Overall, the economic development literature states that general tax incentives and levels of taxation are not major determinates of relocation, but instead might have some marginal influence when there is

some tax elasticity and where the incidence is significant compared to other contiguous jurisdictions.

Even if the empirical data on relocation decisions was not enough to show the futility of using tax incentives as an economic development tool, two additional arguments bode against it. First is the concept of efficiency, second is the notion of opportunity costs. In terms of efficiency, if in fact tax incentives are not major determinants of relocation, then awarding them pays entities to do something they were already planning to do. In addition, in relocations, while one community wins, another loses (especially if there is a closing and moving to another location). Subsidizing the cost transforms what might have been a net zero impact in terms of economic costs into an overall loss scenario. On top of all that, any money that perhaps should have been awarded to the losers to make the relocation Kaldor-Hicks and Paraeto efficient is lost.

In terms of opportunity costs, communities contemplating tax subsidies should consider whether it would be a better use of their money to give incentives to businesses or spend the money another way if the goal is economic development. Public investments in education, infrastructure, and worker productivity rank significantly higher in terms of encouraging economic development and higher workers' wages than do tax subsidies and incentives. Adam Smith, writing in his 1776 *Wealth of Nations* first pointed that out, and this remains a truism over 200 years later.

The bottom line: Tax subsidies and incentives for economic development do little to impact business investment decisions.

2. High taxes serve as deterrent to work or business activity.

A variant of the "taxes are important factors affecting location decisions argument" is the claim that high taxes are deterrents to economic growth and that tax cuts will generally lead to

investment decisions that produce, for example, jobs and new employment. Moreover, another variation of this argument is that both individuals and entities have high elasticity when it comes to taxes such that both flee from high to low tax jurisdictions.

For all of the same reasons that taxes are not a major factor in economic relocation decisions, the same is true in terms of the impact of taxes on business or individual activity. While taxes may have some minor impact alone in terms of marginal decisions to produce, the broader claim that they impede serious economic growth is a vastly overblown claim that lies at the heart of supply side economic theory. Again, as with business location decisions, taxes are a relatively minor factor in the costs of production, falling far behind labor, transportation, supplies, and perhaps energy in terms of issues impacting production. Moreover, often overlooked in studies of taxes is the net overall impact of taxes minus services that individuals and entities receive in a jurisdiction. One needs to undertake a complete calculation of what benefits are received from a jurisdiction minus the costs in taxes. It is this overall package that perhaps more appropriately should be considered when deciding what impact government taxes and spending has upon decisions to produce.

There are two additional arguments that can be marshaled in a discussion of taxes and productivity. First there is the often used argument that if high taxes were the only factor affecting production decisions, then states such as Minnesota, New York, Massachusetts, and California would be economically dying whereas Mississippi and Alabama would be prosperous and wealthy states because of their respective high and low tax incidences. However, the opposite is true, suggesting other factors are also important in affecting business sand work decisions.

Second, if in fact high taxes are a deterrent to work, one would expect to see that reflected in migration work patterns among commuters. Schultz (1998; 2000 b) studied the impact of state

income taxes upon individuals who were tax commuters. Tax commuters are individuals who cross state lines for the purposes of work. He found little evidence that the presence or absence of income taxes, high or low income taxes, or even the presence or absence of special commuter taxes served as a deterrent to individuals crossing state jurisdictions to work. While clearly these studies are not the final word on the topic, they do suggest that other variables beyond the presence or absence of income taxes serve as an impediment to work. In fact, assuming many individuals have a certain amount of autonomy and choice to cross jurisdictions to work, they could have engaged in tax avoidance behavior (beyond cheating!) by choosing not to commute.

Overall, while not denying that in some cases that taxes might serve to impact work in some marginal cases, the broader claim that taxes alone discourage individual and entity production is a vastly overblown assertion.

3. Enterprise zones are an efficient means to encourage economic development.

Enterprise zones in the United States were developed as an economic development tool to revitalize depressed areas that were lacking in investment or job production. More often than not these areas were urban communities which, since the Model Cities programs of the 1960s, had been the focus of redevelopment. These were communities which some scholars had seen as creating cultures of poverty or were areas where work and jobs had disappeared. Unfortunately, Model Cities did not work, or at least was perceived not to work or was abandoned by the President Nixon's administration, and instead replaced by the Community Development Block Grant Program and Urban Development Assistance Grants. But by the 1980s even these programs were not viewed favorably and during the Reagan Administration enterprise zones became a popular idea for

economic development. These zones came to be seen as development tools not just for urban cores, but for any depressed community needing an influx of capital investment.

Much of the logic of enterprise zones relies upon the assumptions of using tax abatements as incentives for business relocations or development or to encourage employment decisions. But in the case of enterprise zones, there is an identified geographic region which is deemed to be economically disadvantaged or needing special help. Within this zone a state or local government would provide financial incentives, such as abated or reduced income, property, sales, or other types of taxes. These tax incentives, along with perhaps infrastructure assistance, are supposed to induce businesses to relocate into the enterprise zone, and thereby bring with it jobs and all the benefits associated with their move.

While elegant in theory, how have enterprise zones really worked? Bottom line, they are generally a cost ineffective failure in encouraging economic development and in producing meaningful employment opportunities. As summarized by Peters and Fischer (2003):

> Enterprise zone incentive programs do not seem to provide enough benefit to firms to materially alter their investment and locational habits; as a result, they do not induce much, if any new growth. Moreover, although enterprise zones are justified by politicians and academics alike as helping economically disadvantaged areas, zones do not appear to provide much in the way of employment opportunities to zone inhabitants. Furthermore, they tend to be very costly for government (128).

Enterprise zones thus suffer from three defects. First, the financial benefits are too little to induce relocation decisions. Second, the jobs they produce are not meaningful or are too costly. Third, the overall programs are costly. Add to these three a fourth criticism: Enterprise zones merely move investment from one community to another at public expense, thereby creating no new economic gains while at the same time subsidizing relocation decisions that probably would have already

occurred.

In developing the criticism of enterprise zones their operational logic rests upon the same premises as the overall strategy of using taxes as a drive to induce development or relocation. As Bartik 1991; 1994) and Wasylenko (1997) and others have shown, taxes are far down on the list of factors that influence economic location decisions. They are far less important that the quality of the workforce, access to supplies and markets, and a host of other factors. Thus, for taxes to be a major factor to induce relocation they would have to be of such magnitude and time far beyond what any government offers.

Second the job production associated with enterprise zones is spotty at best. One study in California found for example that nearly half of the business involved in a enterprise zone program stated that the tax credits had little impact on their decisions to hire individuals (California Budget Brief 2006). Even a 2001 HUD study on the federal Empowerment Zones and Enterprise Communities Program could not substantiate that the job growth in these areas was statistically better from the areas outside them during the hot economic binge of the 1990s. The quality and cost of the jobs produced were also of concern in the report on these zones. Overall several studies, such as by Boarnet and Bogart (11996), (Greenbaum 1989), Engberg (2000), and Bondonio and Engberg (2000) reach similar conclusions that job and economic growth were not necessarily enhanced by enterprise zones. Even beyond their use for enterprise zones, tax breaks often fail to create the conditions for companies to hire individuals. For example, Berenson (2007) reported that even with the billions of dollars in tax breaks given to drug companies, those incentives failed to yield the jobs promised.

Third, as just noted, job production in enterprise zones seldom lives up to its hype.

Surveying over 75 zones, Peters and Fischer (2003) found the average public subsidy per job to be nearly $60,000 annually, with overall costs in the millions to induce one job. Hardly economically efficient. Moreover, in terms of providing job opportunities to those living in the zones, often those in those areas lack the requisite job skills, or those living outside the enterprise area take the job, thereby again mitigating the impact of the government as a development and opportunity program.

Finally, if in fact other factors besides taxes are more important factors in influencing relocation and hiring decisions, what is the real impact of providing the subsidy and encouraging a move? Simply this is classic robbing Peter to pay Paul. Businesses and jobs are closed in one community and are paid to relocate to another (that would have probably already occurred anyhow) but this time at public expense or loss of tax revenue. Hence, any gains in one area are offset by losses in another, unless one can assume that cluster development in the new area is so significant that it offsets the losses in another community.

But yet another flaw in the enterprise zone concept is again related to the same problem associated with building too many of any attraction, such as aquariums—one is novel but too many floods the market and diminishes the uniqueness of them. If there is only one aquarium then it enjoys a comparative advantage as a tourist attraction, but if many communities have them then the unique attraction goes away. Many states have multiple enterprise zones. In Minnesota, JOBZ places numerous zones across the state, competing against one another and non enterprise zone communities. At one time in South Carolina, the entire state was declared an enterprise zone. At some point the dilution of tax advantages from these programs negate one another, thereby rendering them ineffective within the state and then across state lines.

Overall, while a nifty idea in theory, enterprise zones have been shown to be repeated failures

in producing the promised benefits.

4. Public subsidies for sports stadia are a good economic development tool.

Does it make sense for a city or community to fund the construction of a new sports stadium in order to stimulate economic development? Listening to sports reporters, team owners, and many elected officials, the answer is yes. Yet while it may be fun to root, root, root, for the old ball team , does it make economic sense for the public to provide tax dollars to pay, pay, pay to for new stadiums? What are the facts and what do we know about the impact of sports stadia on economic development and urban revitalization? The overwhelming evidence is that the public use of tax dollars for a sports stadium is economically inefficient and a bad investment that produces no real net economic benefit to a community. In short, giving money to building stadia is simply sportsfare—welfare for sports.

In general, as one surveys local debates about stadium construction in the United States, three basic arguments are employed to support using public money to build sports stadia. First, proponents claim that building a new stadium will have a big impact on the economy, generating many new jobs and bringing new businesses to the area. However study after study has demonstrated that advocates of public spending on stadia consistently exaggerate the benefits of sports to a local economy.

A 1996 Congressional Research Service (CRS) report, "Tax-Exempt Bonds and the Economics of Professional Sports Stadiums" (Zimmerman 1996) concluded that sports stadia represent a small percentage (generally less than 1%) of a local economy. It also stated that there is little real impact or multiplier effect associated with building sports stadia. By that, if one looks at the economic impact of the dollars invested in sports stadia, the return is significantly smaller than

compared to other dollars invested in something else. Moreover, the building of stadia merely transfers consumption from one area or one type of leisure activity to another, and that overall, sports and stadia contribute little to the local economy and instead represent an investment that costs the public a lot while failing to return the initial investment. Dollar for dollar, the opportunity costs of investing in sports stadia is a terrible option if the goal is economic development, job development, or producing new economic development in a community. In short, the nearly $3 billion in sports subsidies it documented produced little, at the cost of over $120,000 per job.

Literally hundreds of other studies and books by individuals such as long-time sports economists Arthur T. Johnson in *Minor League Baseball and Economic Development* (1995), Mark Rosentraub in *Major League Losers* (1997), Kenneth Shropshire in *The Sports Franchise Game* (1995), and Roger Noll and Andrew Zimbalist in *Sports, Jobs, and Taxes* (1997), and Michael N. Danielson in *Home Team* (1997) reach the same conclusion—public support of professional and minor league sports is a bad investment. In practically none of the cities these studies examined did new sports stadia lead to any significant new private investment or provide for any significant economic benefits to the local economy besides the jobs generated by the initial capital construction of the stadia. More importantly, the new stadia generally were not even profitable or self-financing. Nor could cities point to rising land prices or economic development in the surrounding community. Even as tourist attractions, the stadia either simply transferred sales from somewhere else, failed to demonstrate that the local hotels were filled as a result of the sports events (Ford 1003: 199). Finally, in terms of the much ballyhooed job production, outside of initial construction and the salaries for the players themselves, part time, seasonal, and no benefit beer and peanut sales jobs were the fare for what the billions of public dollars produced.

A second claim to support public investment in a stadium is that keeping a sports team is necessary to ensure that one remains a first class city. Would the Twin Cities of Minneapolis and St. Paul (which the State Legislature voted in 2006 to authorize a sales tax worth upwards of $300 million for a new stadium) or any city be any worse off by losing a sports team? Without a sports team, most cities would still have parks, museums, zoos, arts facilities, good neighborhoods, schools, and the general quality of life that separates first and second class cities from one another and suburbs.

Moreover, if one accepts this logic of sports being necessary to make a city first class, can we say that New York City became second class when the Giants and Dodgers fled for California in the 1950s, or that Los Angeles became second class when it lost the Angels to Anaheim or the Rams to Saint Louis? The answer is obviously no.

Professional sports are only one small piece of what makes a city first class. Moreover, professional sports are also only a small part of the local entertainment puzzle with many consumers often transferring their consumption to other forms of entertainment, including amateur sports, if pro sports are not available. Similarly, sports are even a smaller piece of the local urban economic pie such that its presence or absence is not significant in the face of other features in a thriving and diverse urban area. In addition, with the cost of attending sports events so high, often approaching or exceeding $200 per game for a family of four, many sporting events are no longer an affordable family entertainment option. Instead, sports owners look to other corporate interests to buy tickets, thereby making sports an aspect of a city's first class status that is beyond the reach of most of its residents.

Finally, advocates for a publicly-funded stadia say that such funding is necessary to maintain

owner's profits. The issue here is not profitability, but the level or amount of profits the owners want. They want to make more money and who is to blame them for that desire. However, there are a couple of different issues here. First, many owners say that larger stadia with more seats are necessary if they are to make more money. To support that, owners often trot out attendance figures to show declining profits.

Attendance figures tell only part of the story since they are only a small part of the revenue stream for owners. Revenue from luxury sports boxes, corporate sponsorship and ads, television and radio contracts, and promotions make up a far bigger and more profitable part of what owners receive from their sports adventures. Yet even this money is not enough because owners often claim they are not making as much money as other owners and thus, building a new stadium is a key to upping their profits. Clearly the end result of this "keeping up with the Jones" logic is to constantly push up the average profitability of all sports teams such that there will always be some teams below the average demanding financial assistance.

Overall, while communities may choose to invest in sports facilities because of the cultural amenities they offer, doing so for economic development reasons is another stupid public policy and political myth that deserves to die.

5. The building of convention and other entertainment centers are successful tools for economic development.

The 1989 baseball movie *Field of Dreams* is famously known for a voice echoing "If you build it he will come." While this line inspired legendary baseball player Shoeless Joe Jackson to appear in a ball field in the middle of corn in Iowa, the quote was also supposed to be a mantra to

encourage individuals to follow their dreams. However, this line has taken on an even bigger meaning, capturing a theory of urban economic development that places an emphasis upon the building of convention and entertainment centers as a spur to economic development. In the case of the shooting of a *Field of Dreams*, Dyersville, Iowa, the ballfield has itself become an attraction built or maintained with the hope that they (tourists) will come.

In the last two decades many cities have shifted their urban economic development strategies, moving away from housing, office, or retail space construction meant to service its own citizens, to approaches meant to woe tourists and conventioneers to their community (Sanders 2002). As Eisinger (2000: 317) states, the basis of this shift is "Thus city leaders make entertainment projects a keystone of their urban economic development strategy, hoping that they will generate ancillary development, high employment multipliers in the hospitality and retail sectors, and local tax revenues." Or as Sanders well captures this "if you build it they will come" belief of local officials when it comes to the building of convention space and centers: "[M]ore space will bring more meetings and tradeshows, generating more attendees, dollars, and economic impact" (Sanders 2002: 206). But does this strategy work? Is the building of convention centers or entertainment facilities to promote tourism alone going to be the silver bullet that revitalizes an urban core or area economy? The simple answer is no.

There are many reasons why the convention-entertainment center/tourism strategy has been unsuccessful. First, again as Sanders points out, the massive expansion of convention space in the last 25 years—from 25 million square feet of space in 1980 to 53.7 million in 2001 to nearly 70 million in 2006 (Sanders 195)—was premised upon faulty studies and projections regarding the demand for such type of space. All of these studies seemed to assume similarly sustained growth,

economic impact, and promises that will flow from tourism. However, all these studies ignored other issues, such as infrastructure costs and needs (such as for airports) to bring in tourism, or they ignored how the competition from other communities also building similar facilities would drain away demand. In short, Sanders pointed out that the tourism frenzy often relied upon untested claims of development, with studies generally financed and commissioned by downtown tourism bureaus that failed to provide a sound empirical grounding for their rosy projections. These studies, as Sanders pints out, were almost verbatim repeated in city after city, ignored the dilution issues that building multiple conventions centers around the nation would yield. They also ignored that some cities, no matter what, may not simply be the draws for tourism, no matter how much they built.

A second and more fundamental reason behind the failure of the "if you build it" philosophy is premised in the belief that a single factor makes an urban center economically viable. As both Jane Jacobs pointed out in *The Death and Life of Great American Cities* (2002) and Larry Ford in *America's New Downtowns* (2003), cities depend upon a mix of factors to make them thrive, including commercial, retail, housing, office, and other amenities. Jacobs argued that, in part, what makes a city exciting and vibrant is encouraging a mixture of uses that bring people and purposes together. Often times convention centers, built to look like big fortresses, wall off attendees from the rest of the city (Ford 2003: 185). This precludes an integration of the center into the rest of a city's economy. The belief that a city can buy development is incorrect according to Jacobs (1985); no single factor can spur revitalization, instead a cluster of factors are critical, including many of those already discussed in terms of what makes an area ripe for business relocation and development.

Tracy (2005) examined the creation of the Orange County, Florida convention center, concluding that it failed to have the economic impact predicted. In searching for answers, one

conclusion was that while a lot of money passed through the community, much of it did not stay in Orange County. Often times up to one-third of the conventioneers did not leave the center or stay overnight, thereby mitigating their economic impact. Similarly, places like Niagara Falls receive 12-14 million visitors per year, but despite that traffic, this New York city is only able to capture a small percentage of these individuals and encourage them to spend money in their community. One conclusion to explain al this is that while convention centers and tourist attractions produced a lot of [economic] activity, this is different from economic development. Convention centers, just like enterprise zones or baseball stadiums, shift leisure activities from one place or pursuit to another, but they overall generally fail to produce the desired economic outputs because they are seldom used continuously in the way other facilities are. In many cases, they are often vacant while awaiting the next convention. Comparing the multiplier impact of a convention center against building an office or housing which is continuously used, it is no surprise that the former fails to live up to its promises.

Finally, again for all of the reasons cited with regard to enterprise zones and baseball stadiums, convention centers and tourism fail as economic development tools in that they assume that the tax incentives to produce jobs are either cost effective, are real and new economic development (and not shifts in consumption), or otherwise have better job multipliers or uses of land than rival investments of public dollars (Eisinger 2000; Sanders 1998). Overall, while it might make sense for some cities to pursue a convention center strategy, their effects are eroded when multiple players simultaneously pursue this strategy.

6. *Welfare recipients migrate to state simply to seek higher benefits.*

One policy legend centers on the welfare magnet thesis that contends that individuals on or

seeking public assistance migrate to states with the highest benefits. In Minnesota, a high--benefit states, elected officials have reached a fever pitch at times in claiming that busloads of individuals in Chicago monthly travel to their state to collect their welfare checks, only to return to Illinois for the duration of the month. Moreover, the lure of the welfare migration thesis was so strong that in 1996 when the Personal Responsibility and Work Opportunity Act was passed, it contained in it a provision allowing states to deny higher benefits to recent migrants to their state. While the Supreme Court eventually invalided this provision, welfare reform in the states produced fears that the anxiety over welfare migration would produce a benefits race to the bottom.

What is the reality of welfare migration? Simply stated, it has largely been discredited. For example, the National Research Council found that few AFDC recipients moved between states, and when they did do so, it was not a result of benefit levels (Welfare Law Center 1996: Citro and Michael 1995). Instead, poor people and those on public assistance move from state to state largely for the same reasons other people. By that, they generally follow overall immigration patterns, moving to states where there are job opportunities, seeking to relocate for family reasons, or looking to get a fresh start in life, just as all other Americans seek to do (Welfare Law Center 1996: Long 1980). Migration patterns in the 1980s and 1990s for example, demonstrated that the poor tracked towards the sunbelt states the same as everyone else. These are the states that generally have lower benefits than the high benefit ones in the midwest and northeast. Additional studies by Allard and Danziger (1997), Hanson and Hartman 1994; Walker 1994; Levine and Zimmerman 1996; and Frey 1996) also largely discredit the welfare migration thesis as a significant factor affecting the decision of the poor to migrate. While there are studies that counter the thesis, the latter largely ignore either how small the migration impact is, how the movement of the poor tracks larger demographic shifts,

or misunderstands or characterizes who the poor are and the resources they have to be mobile.

Perhaps the best series of studies in an attempt to document welfare migration and its costs to the state is in Minnesota (typically a high benefit state, especially in comparison to contiguous neighbors) where the state demographer and other offices have examined the issue. A 1987 State Auditor study in Minnesota noted that in 1986 individuals on public assistance both entered and left the state, leaving open questions about the net impact of welfare migration on the state. A 1994 Department of Planning report entitled "Welfare Migrants Add to Minnesota's Rolls" the state found that based on the 1990 census, four percent of all the migrants to the state were on welfare, costing the state approximately $17 million dollars. These welfare migrants cost the state approximately eight percent of its total public assistance budget. However, the report was unable to ascertain whether the migrants were on public assistance prior to coming to Minnesota or whether their moving to the state was based upon the welfare benefits or for other reasons such as family or job opportunities.

Another report by the Legislative Auditor (2000) found that migration from other states had in fact increased the state's welfare load, although it could not conclude that individuals came to Minnesota as a result the state's benefits. Again the report noted that individuals on welfare also left the state but that overall it cold not ascertain the net impact of migration. Nor could it, the report noted, state clearly the reasons for the migration, but it did state that "welfare benefits are not the primary reasons for migration by welfare families" (48). However, the overall impact of migration on welfare benefits was small. Finally, a 2000 study by the state demographer, drawing upon census data, also refuted the notion that welfare benefits were a major factor in migration decisions. In short, , as the state demographer stated in comments to the researcher: "In any case, if we were a

'welfare Mecca,' as we have been called, wouldn't the welfare roles reflect that with an overwhelming number of recipients accumulated over the past nearly 4 decades?" In short, in Minnesota at least, the evidence of welfare migration is negligible at best.

Perhaps the paradigms of the images of poor people migrating is located in John Steinbeck's *Grapes of Wrath* or in the 1960s television series *The Beverly Hillbillies*. In the former, Oakies move from the dustbowl farms of Oklahoma to seek out the good life in California while in the latter a fictional family moves to that state after striking it rich. Ignored in these depictions are two critical issues. First, the Oakies who moved (and they were based on real accounts of migration during the Depression) did so not for welfare benefits but to find jobs. In the case of Jed, Granny, Jethro, and Ellie Mae, they migrated because they were rich and could afford to do so.

Much of the myth of welfare migration is premised upon several questionable assumptions. First, it assumes that the poor are rational calculators who make cost/benefit life style decisions. Second, it assumes they have knowledge about different welfare benefit levels. Third, the benefit differences are enough to outweigh the costs of travel. Fourth, that these individuals have the resources to migrate. All of these assumptions are questionable. In fact, one could assert that part of what it means to be poor is that one is denied the access to the knowledge and resources to make many of the choices others could make. Overall, the reality of migration is largely confirmed by *The Beverly Hillbillies*—it is the more affluent who move and who are mobile.

7. Three strikes laws and mandatory minimums are effective deterrents to crime.

Mandatory minimum penalties are a popular proposal for getting tough on crime. The idea behind them is simple and it is rooted in deterrence theory: Simply increase the sentence for

committing a crime and the idea is that the average calculating [would-be] criminal will do the math and decide not to break the law. While the idea of mandatory minimums have been around for awhile and in part are at the core of determinate sentencing philosophy since the 1980s, the 1990s saw the birth of" three strikes and your out" laws.

Violent crime was on the rise in the early 1990s, even though the overall crime rate and victimization was still lower than it had been several years before. However, the public rated crime as a major problem, with large majorities supporting enhanced or increased sentences that made it difficult for those convicted of a crime to be paroled. More important, penological research indicated that repeat offenders were a major source of crime. By some estimates, "as few as 5 percent of all offenders may account for over half of all robberies and other violent crimes for gain" (Sherman 1983). This research suggested that in many cases simple incarceration for a longer period of time of some habitual criminals would reduce the number of crimes. In addition, there was a growing belief that rehabilitation as a penological goal had failed (Wilson 1983). Increasing recidivism rates, indicating that larger percentages of inmates were committing crimes and returning to jail, again suggested that incarceration for longer periods of time, was a solution to the perceived rising number of crimes and crime rates.

Tougher mandatory minimums were depicted as one way to deter criminals, yet there was little evidence that such laws had much impact. For example, Franklin Zimring and Gordon Hawkins' study of mandatory minimum laws found little impact in deterring crime (Zimring and Hawkins 1995). Studies in Massachusetts, Michigan, Florida, New York, and elsewhere reached similar conclusions Parent 1997). Good social science evidence was thus available to frame the debates on three strikes to show that mandatory minimums and enhanced penalty laws had little

impact on crime. However, while social scientists often like to believe that their research will have policy import, it appears that sentencing studies had little impact on the three strikes debate.

In the early 1990s, a significant portion of public viewed crime as the most serious noneconomic problem facing the country and demands to get tough on it were hardening (Gallup 1993; Gallup 1994). Both media accounts of crime on the local news that created the impression of escalating violence on the streets and the use of the crime by politicians as an election issue fueled this demand. In 1992, President Bush ran for reelection calling for the passage of three strikes laws, while in1994 Governor Wilson in California rode to reelection on a get tough on crime platform demanding the passage of a similar law. Finally, several high profile cases, such as the Polly Klass murder by a released criminal in California, also drove the public demand to increase penalties and adopt what would come to be known as three strikes legislation.

Thus, debates about tougher criminal sentences in 1993 came in the context that could be called a frenzied emotional setting. Fears of crime and victimization were running high. Politicians were appealing to this mood, and the media was increasing its coverage of violent crime rendering the local news as no more than "crime, weather, and sports." Given this climate, three strikes laws were passed by twenty-two states and the federal government between 1993 and 1995. Exactly what offenses were counted as strikes rather than foul balls or how the laws in each case worked varied significantly.

Did the three strikes law decrease crime? Not surprisingly, the answer is no. Both Zimring, Hawkins, and Kamin (2001) and Schultz (2000) reached similar conclusions in that the three strike laws had little deterrent effect and instead produced several unwanted externalities. Zimring, Hawkins, and Kamin (2001) found little evidence of deterrence. For example, there was no evidence

that those facing the three strikes laws were less deterred or committed less crimes than those not facing the law. Similarly, this study and the Schultz (2000) noted that crime rates were already going down before the three strikes laws went into effect. Additionally, their spotty use in most states questions how much of an impact the laws had on crime rates. Further, comparing three strikes to non three strikes states found no evidence that the former had crime rate decreases greater than the latter. Overall, by all statistical measures, three strike laws did not seem to have much impact on crime rates. Instead, natural down turns in crime as they recessed towards their historical norms, the improvement of the economy, the 100,000 Clinton cops, or perhaps other factor could account for the decreases in crime.

But three strike laws not only had no measurable impact on crime rates but they produced several adverse impacts. For one, to pay for the three strikes laws, states had to increase significantly their corrections budgets, often at the expense of education. This was the case in California where the choice of teaching kids how to read or prisoners how to make license plates led to the latter being better funded. Three strike laws also led to states incarcerating individuals for minor crimes or for terms way in excessive of the safety treat posed by these individuals. Finally, three strikes laws led to a potential generation of elderly inmates, replete with medical and health bills that came with them.

Overall, while sounding like a good idea, three strikes laws strike out as an effective law enforcement tool.

8. Sex education causes teenagers to engage in sexual activity.

Venturing into social policy where moral and religious issues surface, one finds the discussion of sex education to be a hotbed area of much political myth. The United States, like Great

Britain, because of their traditionally conservative treatment of human sexuality, has had a history of difficulty with legislation dealing with contraception and birth control. At the top of the list in this area is controversy over the teaching of sex education in schools. Among the objections raised is the assertion that the teaching of sex education, or about birth control, for example, will lead to increased sexual curiosity and therefore activity among juveniles. Thus, if one wishes to decrease sexual activity among minors, or at least sexually-transmitted diseases, then teaching about sex is exactly the opposite policy that should be adopted. Instead, one should not discuss the issue. One quick response often offered here is that minors are going to engage in sexual activity no matter what, so one might as well tell them the way to do it safely. This response, however, ignores the argument regarding the alleged link of sex ed to sexual activity.

Is there a linkage? Does sex education cause teenagers to engage in sexual activity? Of course, causal claims are hard to substantiate, but even the evidence for correlations is refuted. In general, scientific research has demonstrated that sex education for minors does not increase sexual activity (Pleck 1992; Sulak 2006; Kirby 2002; Oettinger 1999 ; Baldo 1998; Furstenberg 1997; Wellings1995; Wymelenberg 1990; Bearman and Bruckner (2001); Bruckner and Bearman (2005); Dailard (2002); Kirby (2001); Satcher (2001)). More specifically, AIDs and sex education led to a decrease sexual activity. (Pleck 1992). Increased in sexual knowledge resulted in a decrease and delay in sexual activity (Sulak 2006). School-based clinics and school condom-availability programs do not increase sexual activity (Kirby 2002; Edwards, Steinman, Arnold, & Hakanson, 1980; Kirby et al., 1993; Kirby, Waszak & Ziegler, 1991; Kisker, Brown & Hill, 1994; Newcomer & Duggan, 1996; Zabin, Hirsh, Smith, Streett, & Hardy, 1986). A World Health Organization study found that sex and AIDs education did not lead to sexual activity (Baldo 1998). Condom access did not lead

to increases in sexual activity (Furstenberg 1997), and sex education did not lead to an increase in sexual activity or the lowering of the age for first sexual experience (Wellings1995). Overall, the evidence is solid, refuting the claims that increased sexual knowledge increased sexual activity.

The discussion of sex education and minors here is meant to highlight what appears to be a broader myth or error that many policy makers make. That is, often times legislation is debated or discussed and the argument against or for it is something to the effect that "It will send the wrong message to children." Quite simply and for the most part, most adults have no idea what messages are being heard by children and how they are being interpreted. As a result of Piaget and other developmental psychologists, we know that children interpret and organize the world differently from adults. What ever messages adults may think policies are sending is often mere conjecture. Instead of guessing what message a teenager may be receiving, go ask one. . .or several! Perhaps with the exception of adults at the cigarette and tobacco companies responsible for Joe Camel©, few probably understand what children are learning from adult messages. This suggests that unless one is prepared to document from surveys or other empirical means what a specific message may meant to children if they are listening, this objection to policy making needs to be banished.

9. Immigration and immigrants take jobs away from Americans and serve as a drain on the economy.

Immigration seems to be a perennially politically salient issue in the United States. While there are numerous reasons surrounding its political volatility, two specific arguments are generally levied against immigration. One criticism is that immigrants are a net drain on the economy; specifically, they are a bigger drain on taxes and public services than they are overall contributors

to the economy. Second, immigrants are depicted as taking jobs away from Americans. What do we know about both of these claims?

First, while acknowledging that immigrants may in some local settings or jurisdictions place some short term significant burdens upon public services, overall they are net contributors to the economy. Several studies substantiate this point. Most recently, the *2005 Economic Report of the President* provided a detailed analysis of the impact of immigration upon the United States economy. In it the report noted they as a group they had up to a $10 billion net positive impact upon the economy (*Economic Report of the President* 2005: 106-108). The report noted for example that while immigrants may be more likely than native born Americans to be on public assistance, the "net present value of immigrants' estimated future tax payments exceeded the cost of services they were expected to us by$80,000 for the average immigrant and his or her descendants" (107). However, with changes in public assistance laws, that figure had been upped to $88,000 (107). Yet this figure masks the fact that better educated immigrants (highschool degree or better) definitely display this positive figure, but even among those not as well educated, the gains from their and their descendants' productivity nearly if not totally offset the costs they impose upon public services that accrue to state and local governments. Finally, the president's report also provided other documentation regarding the impact of immigrants upon the economy. For example, it noted that immigrants paid Social Security taxes on income of $463 billion dollars (108).Moreover, because illegal immigrants cannot collect Social Security, it is likely that immigrants overall pay more into this government program than they receive from it.

In addition to the President's 2005 report, other studies have noted the net economic benefit of immigrants to the United States. A 1997 National Academy of Sciences study found several net

benefits associated with immigration, including being the originator of the analysis showing up to the $10 billion net benefit to the economy. Edmondson (1996) found that: "Illegal aliens in prison cost about $471 million a year, and they consume about $445 million more in Medicaid funds. But these costs are offset by about $1.9 billion in taxes paid by illegals and billions more in consumer spending." Furthermore, the National Academy of Sciences, President's Report, and the Edmondson study, all indicted that younger workers provided for important sources of productivity that also served the economy well. Overall, these and other studies clearly contested the myths that immigrants were a drain on the economy.

The second criticism leveled against immigrants is that they take jobs away from American workers or that they negatively impact wages. Again, several studies refuted that (Chomsky 2007). For example, the National Academy of Science study found the wage impact to be negligible, while the President's Report found little impact on wages of native Americans (105-106). The report also noted and dismissed the argument that immigrants displaced American citizens in the labor market. Instead, they often filled in gaps abandoned by others, such as farming and agriculture, and they definitely constituted a new source of productive labor particularly at a time when the size of the labor pool for other workers had disappeared.

In addition to the above studies a Pew Hispanic Center (2006) reached similar conclusions. It compared the economic growth in selected states with high versus low immigration and found no differences in economic growth or in its impact on the labor markets. It also found that there was in fact in 12 states a positive correlation between the growth of immigrant and native-born workers. By that, there was no evidence that instates where more immigrants entered the labor market it depressed the entry of others into work. Finally, even among immigrants who were young and

lacking in education, there was no indication that they directly competed against and hurt native-born workers with similar background.

In sum, the evidence that immigrants are financial drains on the economy and that they take jobs away or hurt the wages of native-born workers is wanting. There may be short-term impacts in selected areas where local governments are burdened by immigrants, but both overall and among sport labor markets, it was a myth that immigrants hurt the economy or workers in the way critics of immigration allege.

10. Legislative term limits will dismantle incumbent advantages, break ties to special interests, and discourage career politicians.

The idea of legislative term limits have deep roots in American politics. Dating back to the Anti-Federalists and their critique of the new constitution, advocates of term limits see in them important values, including the republican idea of rotation in office to produce citizen-legislators. Yet frustration with special interest politics or the apparent corruption of career politicians lead to the passage of legislative term limits in 21 states during the 1990s. The idea behind the recent movement for term limits is the belief that many of the ills affecting state legislative politics is rooted in career politicians who place self-interest above the public's interest, or who are otherwise embedded in lobbyist or special interest networks that make it impossible for them to legislate. Moreover, advocates also believed that term limits would change the type of people serving in legislatures, increase and promote competitive elections, and would change how work and power is allocated (Cain, Hanley, and Kousser (2006).

Have term limits lived up to their expectations as an important reform tool? At best, their

reforms have fallen far short of their desired outcomes, while at worst, they have had a negative impact on many aspects of legislative politics and power and, in some cases, have produced effects almost exactly counter to what was desired.

One of the earliest and most comprehensive studies on legislative term limits was an edited volume *The Test of Time* (Farmer, Rausch, and Green 2003). Written only within a couple of years after some of the first legislatures were experiencing the initial impact of the term limits, the various articles in this volume concluded that the limits produced results that suggested "substantive, but mixed" results (5). For example, there was little evidence of a change in who ran for office, although there were modest gains in some states for women and minorities (6). Term limits seemed to have little impact on career incentives, with many of these individuals running for other offices or taking appointed political positions or becoming lobbyists (Powell 2003: 144). There was little evidence of increased competition for offices and, in fact, because career politicians continued to cycle their way through offices, there was some evidence that this phenomena (along with increased campaign costs for open seats) created new impediments for citizens contemplating a bid for office. Moreover, partisanship seemed to increase, knowledge among legislators decreased, and the ties to lobbyists and special interests that was supposed to break generally failed to materialize (Farmer, Rausch, and Green 2003). Finally, leadership skills in the legislature seemed weaker and the power of this branch of government overall diminished vis-a-vis the governor and executive branch. Perhaps the only real change that did occur in terms of what term limit advocates had hoped for was the production of more open seats.

Subsequent studies seemed to confirm these initial conclusions. Cain, Hanley, and Kousser (2006) found that while more seats were contested, the races were not necessarily more competitive.

Term limits also did not seem to produce party turnout of seats, more citizen responsiveness, or less bias (218). Sarbaugh-Thompson, et al (2002; 2004) found little if no evidence that term limits in Michigan produced increased voter turnout, weakened incumbency advantages, or increased more competitive elections. They also found that the elections under term limits generally cost more, did not necessarily make candidates less reliant upon special interest or lobbyist money, and that, in many cases, led to an increase in more self-financed wealthy candidates running. In short, they saw little evidence that term limits in Michigan attracted more citizen legislators (Sarbaugh-Thompson 2004: 187) or that it diversified the legislature much from what was previously seen. Among the 11 promises they list that term limits advocates hoped their reforms would achieve, Sarbaugh-Thompson found only three of them were secured, with the remainder failing (191).

Kousser (2005) also concurred with many of the results noted above. However, in his research he examined how the increased professionalism of legislatures (more pay or staff, for example) and term limits impacted this branch of government. His conclusion was that term limits offset any legislative improvements that were produced by professionalism. He also found that generally term limits changed the knowledge and skill level of legislators and, most importantly, had a damaging impact both up policy innovation and time lines for legislators (202). Term-limited legislators were less likely to look to policies that had longer time horizons for success, and generally less knowledgeable officeholders had less time to work on policies or dra upon their skills and experience to craft more innovative policies. Instead, they seemed more likely to follow what had been developed elsewhere, or relied upon ideas from staff or lobbyists.

Kousser (2005) as well as other research also reinforced much of what the initial research had tentatively concluded. Term limits strengthened the executive branch, especially with the budget,

and failed to produce a new type of legislator. Research by Schaffner, Wagner, and Winburn (2004) indicated that term limits made redistricting more partisan. Carey, Niemi, and Powell (2000) found little demographic change in term-limited legislatures, while Carroll and Jenkins (2001) found little evidence that female candidates benefitted from the reform. Carey, Niemi, and Powell (2000) also noted increased partisanship, less cooperation in bicameral bodies, a shifting of power towards the governor, and a decrease in legislative knowledge and innovation. A 2006 study by the National Council of State Legislatures reached similar results.

To summarize, term limits as a "throw the bums out" strategy have failed to live up to their promise as a significant political reform and, in some cases, may actually weaken the very values its advocates wanted to promote. Term limits, if not a waste of money, certainly cannot be described as a successful reform but instead one that seems failed to solve the problems they were designed to address.

IV. Why Stupid Public Policies and Political Myths Never Die

If the evidence suggests that some public policy ideas are myths or are shown to be defective based upon social science research, how can one explain their immortality? Several theses are possible.

First, perhaps the most simple answer is that many elected officials, policy makers, and members of the public may be unaware that the policies do not work. It may also be that the lack of staff time and resources at the state and local level reinforce this ignorance. If the research on term limits is accurate, less experienced legislators might be more likely to repeat these failures.

Even though this paper has potentially run roughshod over some of the nuances of the

research regarding the different policies discussed, there is no question that many policy makers may well be unaware or unable to digest the various studies on welfare migration, economic development, or any of the other topics under discussion here. Given that many policy makers may rely upon almost a word of mouth in constructing policies, they simply may be outside the information loop when it comes to policy evaluation. Moreover, they may simply not be able to appreciate the differences or significance of what social scientists are arguing, or the latter may not be very good at communicating their thoughts and speaking to the broader public.

A second argument for the persistence of stupid policies and myths is a belief that if it works in one place will work in another, or that while it did not work somewhere else, this time or in this place it is different. There are two problems here. The first argument relies upon a false aggregation thesis. By that, something that works in Chicago should also work here. However, not every place is Chicago, and the novelty or scarcity of an attraction may make it a draw in a limited number of cities, but not one when the market is flooded with them. Conversely, policymakers, while acknowledging that a stadium plan did not work elsewhere, will argue that this time it is different because the sports subsidy now is embedded in, for example, a broader commercial revitalization project. Thus, unlike the failures of the past, this project learns from their mistakes and will work.

More cynically, there are also several reasons why stupid public policies and political myths persist. Interest group politics, as well as the impact of money in politics, could account for why some policies endure despite evidence to the contrary. In the case of welfare migration and immigration, fear and prejudice may drive the topics. In other cases, partisan or personal electoral political may explain why elected officials push three strikes laws. Here, despite whatever one may know intellectually, politically it may be pragmatic to push specific policies for personal gain.

Furthermore, and even more cynically, perhaps those individuals and groups who are most likely to be pressing many of these unsuccessful policy ideas are those who least support an activist government. Thus, while at the same time engaging in rent-seeking behavior they are also setting the government up to fail. The result is that this both reinforces the idea that the government is the problem, not the solution, and it feeds upon public cynicism towards it.

Another reason why some policies live on may be rooted in the narrow political orientations found within American culture. Given a basically pro-market orientation towards the economy, appeals to market incentives might well determine what types of economic solutions may be offered by political leaders and be considered acceptable by the public. For example, when Francois Mitterand was elected the Socialist President of France he was asked if he planned nationalize a specific French car company. He replied no, asking why he would take over a company losing money. Instead, he wanted to take over businesses making money so that it could finance government programs. In the United States, nationalizing a business, especially a profitable one, would clearly cut against the grain of the ideology of many.

But there are two final reasons for the immortality of some policy ideas. First, perhaps politics and political choices are not about reason but passion. By that, David Hume, Edelman (1985), and recent works by Frank (2005) and Westin (2007) may be correct—politics and policy making is not rational, but more symbolic and emotive. This means regardless of what research may suggest, policy ideas are driven by variables other than disinterested reason. Second, the persistence of often repeated failed policies may speak Brandeis' assertion that states are laboratories of democracy. Perhaps they should not be so characterized. Instead, states may be more often than not described as displaying a lack of innovation. In reaching this conclusion, researchers almost

need to invert the diffusion literature, asking not why or how states innovate, but how do they mindlessly replicate policy mistakes and myths.

V. Conclusion

"Stupid is as stupid does." So said Forrest Gump of the motion picture by the same name, in referring to how stupid individuals do stupid things. While this paper examined only ten stupid public policies and other political myths, a host of others—including public subsidies for casinos and lotteries, privatization of government services as a cost savings device, and the myth of government ineptitude—warrant exploration and demolition. Governments may not be inherently stupid, but oftentimes they pursue policies that fit that category.

This paper has examined stupid public policies and political myths within the context of the literature on state diffusion and innovation in order to raise several questions. First, it looked at the gap between political myth and social science research, asking how the political process can be better informed when policy is made. It also queries to whether policy making should be data-driven. These two questions reflect the often contrasting perspectives of elected and appointed officials who see the world from different vantage points. While many would like to see budgets, policy, and laws reflect good social science evidence that is rational, others may say that all three of these items are political issues that reflect clashing ideologies and values.

Second, this paper asked questions about state innovation and learning. While the diffusion literature has sought to understand how jurisdictions learn, this paper has suggested that the innovation is less than meets the eye. States may adopt policies from elsewhere but how good of a job do they do in learning from the mistakes of others. It is not clear they do, raising questions about

the laboratories of democracy label often pinned on to states. Third, the paper sought to document ten policies or myths that never die, despite their repetition, questioning why they continue to live on despite their bankruptcy. If in fact the government is inevitably going to make policy, it should at least do so in a way that generates value for the taxpayer (Osborne and Hutchinson (2004).

Finally, the paper questions the motives and interests of those who advocate for failed policies. The concern here is what are the broader or more narrow political agendas that drive groups and individuals to push for them.

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