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Riding the Third Rail

The 2004/2005 Social Security Reform Proposals in the United States: institutionalism and new political theories

Dissertation submitted by

STEVEN BUTTERFIELD

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Name: Steve Butterfield

Signature:

Location and Date: Hallowell, Maine, USA, July 14 2014

Table of Contents

Abstract

This paper examines the 2004/2005 proposed reforms to Social Security in the United States.4 Given the significant factors favoring a positive outcome for the policy proposal, why did it fail? The author uses a historical institutionalist and critical junctures approach to show that, while this could be termed a "near miss" critical juncture, institutionalized path-dependent constraints on actor actions ultimately doomed the policy. A synthesis of other research is undertaken to delineate a new theoretical construct that seeks to explain the relationship between political figures and their relationship with the voting public, and is termed "electoral gains theory." A process tracing analysis of the specific case is used to reach conclusions about the political viability of entitlement or welfare reform in the United States and, by implication, other affluent democracies.

Introduction

In 2004, George W. Bush was re-elected as President of the United States of America. Perhaps more surprisingly, his Republican party actually increased their majorities in both the House of Representatives (holding a little more than 53% of the chamber) and the Senate (where 55 out of 100 senators were Republican). By nearly any metric, the election was seen as a strong and in some ways surprising success for both President Bush and the Republican party.

On the heels of that electoral victory, the President announced plans for the most significant reform to Social Security – America's old age pension system – since the program's creation. Given the significant political capital at the President's disposal, the positioning of the reform initiative as a central component of his domestic agenda, strong majorities in both chambers of Congress, and ongoing concern over the future stability and solvency of the Social Security trust fund itself, reform, once proposed, seemed almost inevitable.

Yet four years later, on leaving office, President Bush could point to no greater accomplishment on the issue than simply having proposed a change. Not the tiniest element of reform was ever seriously debated in Congress, let alone passed; significant or dramatic overhaul was never even on the table. In the 2006 "midterm" elections, Democrats commandingly overturned the Republican majorities in both the House and Senate, and went on to capture the presidency and significantly build upon their majorities in Congress in 2008.

Blame for the result of those elections cannot be laid entirely, or perhaps even largely, at the feet of Social Security reform (and, as I shall explore shortly, neither political party can claim "ownership" of the issue, nor can it be said that more "liberal" or more "conservative" policy proposals gain any greater traction). I believe, however, that interesting and unique lessons can be drawn from a comparison of the (failed) Social Security proposals in the United States in the early 2000s – lessons that shed light on a crucial mechanism affecting the understanding of how electoral machinations during critical junctures can result in policy success or failure.

The Gap in the Current Literature

I began this paper with a deceptively simple question: what political factors impact pension reform? As a former elected official myself, and one who focused primarily on labor and welfare issues, the topic held both academic and personal interest. My readings of several theoretical models of public policy decisionmaking (historical institutionalism, public choice, critical junctures) left me feeling that each may have accounted for a piece of the puzzle, but the final corner remained elusive. And, based on my professional political experience, each seemed in their own way to be frustratingly abstract, often – to my mind – missing the forest for the trees.

To a historical institutionalist, the lack of progress on reform would be quite unsurprising: a large, mature, quasi-independent bureaucratic behemoth would hardly be expected to undergo a significant change that altered its very functioning, given the power of path dependence. How, though, could they explain those situations in which change does occur – sometimes significantly, often abruptly?

A compelling explanation has been provided in the form of critical junctures theory, with its focus on moments in time in which conditions are set in such a way that change can be allowed to occur. Yet I found myself more interested in what some within the field refer to as "near misses" (Capoccia and Kelemen 2007). If critical junctures theory held that the confluence of a certain set of conditions should allow changes to occur, and yet those confluences sometimes resulted in no change at all, then what happened?

While I confess that my past as a mandate-holding, policymaking elected official may have influenced my own thinking on the issue, I looked then to public and public choice theories to fill some of the gap. Even if the other circumstances posited in a critical juncture were aligned, I felt that the influence of political self-interest could be more significant than had been previously explored in the literature. Yet I find public choice, rooted in dense behavioral economics and mired in debates over "morality," insufficient in explaining the exact type of electoral ramifications that I (and others) believe is a serious risk for politicians who embrace wholesale, unpopular welfare reforms, and I could not find a theory that adequately explained the confluence of conditions which I believe is relevant in these cases. I have therefore synthesized several other existing forms of analysis into a nascent construct that I have called "electoral gains theory."

The Research Question

As outlined in the preceding section, a reading of these theoretical schools provided me with the basis for the question I explore here: **was the 2004/2005 Social Security reform effort in the United States a "near miss" critical juncture, or simply a failed policy initiative?**

I hold that an answer lies in a blend of the theories mentioned above. While historical institutionalism *can* convincingly demonstrate the significant force of path dependence, and critical junctures theory *can* add the framework necessary to explain the "branching" patterns of brief but significant change followed by longer periods of incrementalism, and while electoral gains *can* explain (at least in part) the self-interested motivation behind some actions of elected officials and policymakers in the specific moments of an actual critical juncture, only in considering the three together can a new understanding of policy motivations in a crisis be reached.

While much recent scholarship has focused on longer-term time series analysis leading into and out of an individual critical juncture, I have focused here on the "snapshot" of single "moments," or solitary junctures – those confluences in which change might reasonably be expected as a result of a loosening of path-dependent constraints on action. My choice to focus on actions within these individual moments is no accident. While the boundaries of available options may be forged by (and revealed over) time, the options available to a policymaker within a single crisis will be defined by more than institutional history or precedent alone. It is these constraints that I intend to explore here.

I propose that during some critical junctures within a democracy, the role of elected officials in policy change takes on an added significance as a result of their unique position as both initiators of, and veto point for, large-scale policy changes such as pension reform. In these moments, policymaker *perceptions* of what options are available to them in both a policy-outcomes sense (e.g. structural change) and in an electoral/political one (e.g. reelection and blame avoidance), rather than pure path dependence or institutional inertia, will define the range of potential outcomes.

Outline Of This Paper

Following this introduction, a literature review will provide clarity on the current state of the art as far as pension reform politics (or, more broadly, welfare politics).

A larger section will more deeply explore the three theoretical frameworks – historical institutionalism, critical junctures, and electoral gains – being utilized in my analysis. This will be followed by a brief explanation of my methodology and case selection.

The subsequent section will contain the actual case study analysis.

Finally, conclusions and closing thoughts will be offered.

The State of the Art

This paper proceeds from a fundamental normative assumption: that some standardized, more or less universal form of income replacement or income security for elderly and/or retired individuals – a pension system – is worthwhile. I do *not* make or intend any assertion about what makes such a system "good" or "bad." The specifics of any such system are beyond the scope of this paper.

Following from this, it is irrelevant for my purposes to consider what the outcome of a proposed reform to an existing system would be. There is ample literature available on the fiscal, monetary, and budgetary realities of various types of pension systems and pension reform, to say nothing of the ramifications in terms of coverage rates, participation, etc. These topics, as well, are not what will be discussed here.

My interest is purely in exploring what motivates or derails attempts to change ("reform" or "dismantle") existing systems. Still, it is essential to explore the basics of pension systems in broad context in order to obtain the basic level of vocabulary and understanding necessary to contextualize the atmosphere or history in which those changes are taking place.

To provide definitions of some basic terms surrounding pensions, one can begin at what many now consider to be "the beginning" as far as the modern era of pension study. In 1994, the World

Bank published a book entitled Averting the Old Age Crisis: Policies to Protect the Old and Promote Growth.

Averting defines pension spending as the following:

> "old age, retirement, survivors', death, and invalidity-disability payments based on past contribution records plus noncontributory, flat universal, or means-tested programs specifically targeting the old." (1994, xxii)

Averting has, in the two decades since its publication, become a significant piece of foundational literature in the field of pension system research. The book is an exhaustively researched and detailed examination of virtually every aspect of pension system design that may confront policymakers in any nation. In the years since its publication, the report "has had a profound effect on the evolution of public social security programmes" and even become "so well-known by those involved with public pensions that the first word of the title suffices" to identify it within the field (McGillivray 2002, 2).

This is especially true of elements regarding system coverage and financing. In its basic research and examination, the book does not limit itself to one particular type, kind, style, or design of system (it has chapters covering "informal systems," public, private, and blended pension systems). In its *advocacy*, however, the World Bank has a pretty clear agenda at play in the book. It is this agenda that serves as the basis for much of the subsequent criticism of the report.

In short, the World Bank report heavily prioritizes overall national economic development, and pension fund economic stability, at what critics would consider to be the expense of system affordability and adequacy for beneficiaries (Beattie 1995). This report, and its criticism, are useful in delineating the boundaries of a primary normative debate within the study of pension system design – namely, balancing, on the one hand, the economic and social interests of a nation or population in decreasing or ameliorating elder poverty, against (on the other) the fiscal and strategic concerns and constraints, and risk exposure, of the state itself. Put simply: who pays?

Both of the McGillivray papers (1995, with Beattie, and the 2002 working paper) and the World Bank report itself do spend some time focusing on political factors influencing pension system design and reform. Indeed, several chapters of the World Bank report include at least some exploration of political factors at play in pension system decisionmaking, but the "crisis" mentality of the report tends to sweep these political concerns aside in the name of making "necessary" changes quickly. The Averting report does not seem to care much for the political nature of enormous and costly changes to public spending, wealth redistribution, elder poverty or income security, or path dependence.

Yet the question of "who pays" leads to the question of "who benefits?" From here, it is possible to begin sketching in the relevant actors and institutions in any discussion of the politics pension system reform. One simply cannot ignore the contributions of Paul Pierson to this field. Indeed, I would like to acknowledge the extent to which Paul Pierson is cited throughout this paper. I am aware of both my heavy reliance on his work, and the risks incumbent with overreliance on a single source. However, his prominence, even dominance, within the topic of welfare politics makes him seemingly unavoidable. Most of the other sources I have cited herein rely on Pierson's work more or less as heavily as I do.

Perhaps nearly as influential as Averting is Pierson's 1994 book Dismantling the Welfare State?, in which he explores the seeming resilience of welfare policies even in the face of governments and administrations whose political agendas would seem to favor welfare retrenchment. Pierson builds an argument around the popularity, within the public at large, of public spending and wealth transfer programs (including pensions) and lays out the path-dependent manner in which new forms of advocacy groups and coalitions who will seek, at length, to protect and preserve the programs from which they benefit. The book lays the foundation for future explorations of welfare system resilience. The importance of Dismantling in the field can hardly be overstated – some authors have referred to the book as "seminal" (Starke 2006, 105).

Pierson built on and expanded this earlier work in 2001 with an edited volume The New Politics of the Welfare State. Featuring works from several contributors, topics of interest to my thesis include Pierson's own concluding chapter "Coping With Permanent Austerity." Synthesizing the preceding contributions in the volume, he concludes that while there is little or no evidence of large scale retrenchment or welfare overhaul, "in most of the affluent democracies, the politics of social policy centre on the renegotiation, restructuring, and modernization of the terms of the post-war social contract rather than its dismantling." (2001, 410)

Here, Pierson alludes to an interesting expansion in the consideration of what influences reforms to existing systems. Some scholarship has considered the impact of globalization on welfare regimes in individual countries. One entry in the genre comes from Sarah Brooks in a 2005 piece that explored the ways in which pension reforms have cascaded among "peer nations." Drawing from examples in and around Latin America, Eastern Europe, and central Asia, the article provides a unique and compelling idea – that international political influences exercised by "peer nations" may provide a compelling or forceful rationale for nations to adopt pension reforms (Brooks 2005).

One author who finds evidence for a slightly stronger effect on welfare states from globalization is Herman Schwartz, whose clever and entertaining contribution to <u>The New Politics of the Welfare State</u> makes the argument that an increasingly market-based approach to wealth transfer and regulation (or more aptly, deregulation), particularly in the United States, resulted in a situation in which "the broad welfare state was murdered, even though the narrow, formal welfare state survived." (Schwartz 2001, 44)

Yet others who have considered the impact of globalization on the potential for a trend toward a welfare "mean," in which a sort of global consensus on a minimum welfare state is reached and nations bend toward the mean, have generally concluded that the evidence for such an effect is mixed at best, weak or conclusively irrelevant at worst. One compelling and rather exhaustive study of nearly 30 years of variables in multiple countries concluded that "Increased globalization and a modest convergence of the welfare state have occurred, but globalization does not clearly cause welfare state expansion, crisis, and reduction or convergence." (Brady *et al* 2005, 921)

In addition to globalization on his list of "usual suspects" for "who killed the growth of the welfare state?", Schwarz (2001, 17) also considers domestic politics. This topic is expanded on in the third section of <u>The New Politics of the Welfare State</u>, in which three chapters explore various aspects of the politics of welfare state retrenchment and other related policies.

The first, by Duane Swank, explores two significant pressures that contribute to an atmosphere of a necessity for welfare reform: domestic fiscal stress, and international capital mobility. Of the two, domestic fiscal stress – in particular a situation in which the proportion of the population aged 65 or older, and especially the proportion of those 80 or older, is growing faster than the ability of national GDP to absorb the increased spending on health care and pension payments for the individuals in those age cohorts – is useful for my consideration of the forces acting on policymakers as they contemplate pension reforms. Swank concludes that such pressures, depending on certain other considerations of political institutions, are likely to result in at least an atmosphere in which there are "downward pressures on social welfare provision," and mentions the United States as a country in which this atmosphere is perhaps particularly expected. (Swank 2001)

The second, "Political Institutions, Veto Points, and the Process of Welfare State Adaptation," by Giuliano Bonoli, explores the impact on policy outcomes of the government frameworks under which policy is considered: that is, among other factors, how the concentration of power (strong executive, e.g.) or the number of veto points (in a more fragmented system) determines the range of options and outcomes available to actors.

Bonoli looks at several aspects of government and political structure to assess their impact on policy formation and outcomes, including the existence of referendums, the specifics of a country's electoral system, and the structure of parliament (242-243), before looking more closely at the experiences of welfare reform efforts in three countries – the United Kingdom (strong concentration of power), Switzerland (fragmented/diffused power with many veto points), and France (straddling the middle of these two extremes) – to see if patterns can be discerned from their experiences.

Is significant policy change more or less likely in a setting where a strong executive and single party control of government exists? On the one hand, Bonoli finds evidence that a concentration of power provides a smoother path for significant change: though he feels that the results are ultimately inconclusive, he does note that "a government might be more capable of steering policy if acting in an institutional context of power concentration, but it will also be more inclined to take into account the electoral consequences of its actions." (Bonoli 2001, 239)

I do not agree with all of Bonoli's findings. For instance, he notes that in the United States, even a concentration of power in the hands of a single political party at each level of elected government – House, Senate, and President – does not necessarily result in significant reform or policy change. He illustrates this with the example of the failed health care reform efforts of President Bill Clinton in the 1990s, who – despite Democratic majorities in both chambers of Congress – could not pass his health care expansion, a failure he attributes at least in part to the lack of institutionalized "party discipline" measures in the U.S. to compel votes on an issue from members of the party (242).

While Bonoli is not wrong about the conditions or failure of the Clinton health care bill, and while I certainly understand (from personal experience) the lack of formal mechanisms to compel legislative voting behavior in the United States, I could point to numerous other examples of major, contentious policy initiatives that *have* succeeded under single-party conditions (the more recent health care reforms, so called "Obamacare"; or the 2001 Bush tax cuts). Further, the lack of formally institutionalized controls on member voting does not mean that there are not significant *informal* tools at the disposal of leadership that can be significant tools to nudge action – such as committee asignments, fundraising, "earmarks" and "sweeteners" (special provisions in legislation that directly benefit an individual member's home district, thus making it difficult for the member to vote against a bill which includes provisions or funding of unique and distinct benefit to their constituents), and PAC (political action committee) support.

Still, another of Bonoli's conclusions – that "the combination of retrenchment and modernization is an effective strategy to neutralize the impact of veto points on welfare reform" (263) – leads to the third, and for my purposes most salient, chapter of this section of New Politics, which is Herbert Kitschelt's "Partisan Competition and Welfare State Retrenchment," in which he asks "When [and why] do politicians choose unpopular policies?" (Kitschelt 2001)

Kitschelt's piece is a compelling approach to unraveling a phenomenon seemingly without clear explanation based on broader assumptions of the stability of welfare policy stemming from its broad and widespread public popularity and the attendant severity with which voters view attempts at dismantling (Pierson, Coping With Permanent Austerity: Welfare State Restructuring in Affluent Democracies 2001, 412-413).

Kitschelt's does not dispute the importance of many other factors in influencing, forcing, loosening the restrictions on, or the potential for policy change to occur – for example, exogenous shocks such as fiscal crisis or demographic pressures (301). What he adds, however, is the concept of "party competition," or "mechanisms that may induce politicians to pursue often unpopular reforms based on internal opportunities offered by the dynamic of competitive party democracy" (265). Put plainly, Kitschelt is opening a space for recognition of the unique role that electoral machinations play in political positioning on issues, even "parties…elected into office that announced unpopular social policy changes ahead of elections, or successfully ran on a track record of social policy retrenchment engineered while being a government party in subsequent election campaigns." (*Ibid*)

Kitschelt's most significant contribution, in my view, is to make the case that despite the tendency to compete for median voters, not every political party is playing for the same constituency – the same votes – in every election in direct competition with each other, and neither is the electoral "path to victory" precisely the same in any two elections (in the American primary system, this is particularly true: consider the dilemma of a moderate Republican facing the prospect of losing the primary to a radically right-wing Tea Party candidate who has no hope of winning an otherwise safe seat in a general election). Parties that are able to capitalize more effectively with the public on their position *vis.* welfare reforms (or other political issues), even with positions that may be more broadly unpopular, can sometimes win elections on those grounds, thus creating and gaining political capital which allows them to pursue the options if conditions allow. This is a critical underpinning for my "electoral gains" theory.

Theoretical Frameworks

In this section, I will present the three theoretical schools that comprise my overall theoretical approach to my case study analysis.

Why three theories? Why not pick one, or at most two (for comparison)? I have two reasons.

The first is that I find the three theories compelling in their own ways, but individually insufficient as an explanatory model for the problem I am attempting to confront. In combination, however, the three present a useful tandem model for considering the issue at hand.

The second is that a far more experienced pair of senior researchers in the field suggested it. In their 2002 piece "Historical Institutionalism in Contemporary Political Science," Professors

Theda Skocpol and Paul Pierson write "that many scholars blend styles of research in highly creative ways" and further note their opinion that these "'boundary crossers' are often among the most creative scholars in our discipline" (695). While I certainly make no claim to the level of creativity they applaud, I will take their conclusion as blessing enough to approach a "boundary crossing" approach to my case study using the theories discussed here.

Historical Institutionalism and Path Dependence

This paper focuses on a very limited "moment" in time: that is, a specific period of days, weeks, or months in which a particular policy was proposed, debated, and concluded with either action or terminal (that is, determinative rather than open-ended or attritional) inaction.

It has become increasingly accepted, however, that decisions and policy changes are not made in a temporal or contextual vacuum. Myriad decisions and circumstances lead up to those individual moments and can significantly constrain the range of options available for change. While "[c]ontemporary social scientists typically take a 'snapshot' view of political life," historical institutionalism provides the framework in which researchers can "*systematically* [situate] particular moments…in a temporal sequence of events and processes stretching over extended periods" (emphasis original) (Pierson 2004, 2-3).

Put in the most basic terms, historical institutionalism could be called the idea that "history matters," though as Pierson points out, this statement "is often invoked, but rarely unpacked" (*Ibid*, 5). To a historical institutionalist, "policy unfolds in a rather slow-moving and incremental manner, feeding back into politics in ways that gradually lock it into place" (Jordan *et al* 2012, 5).

Pierson and Skocpol contend that historical institutionalism as a theory broadly contains three defining characteristics: substantive lines of research and inquiry that are of both general as well as purely academic interest; temporal analyses that place a single phenomena or observation within a larger sequential or time-series frame; and wide contextual lenses that are inclusive of a significant variety of actors, institutions, veto points, and other relevant influences (Skocpol and Pierson 2002).

This is reminiscent of a form of path dependence, and indeed there is a particularly strong connection between the two concepts when discussing a specific area of policy (Starke 2006). It is difficult to imagine a historical institutionalist reading of pension system intransigence, for instance, that does not include significant reliance on path dependence as part of the explanatory model. As one author put it, "As a rule…for historical institutionalists, institutional development is incremental and path dependent." (Gorges 2001)

As a thought exercise, consider policy change over time as a river. While both schools are involved in the study of the river, path dependence would be a method by which to examine the river's direction and speed, whereas historical institutionalism would be a method to define the river's precise size and scope. Path dependence seeks to explain how each mile traveled down the river makes it harder to go back to the beginning; historical institutionalism asks how we ended up on the river in the first place.

If a path-dependent, historical institutionalist view of policy maintains that opportunity or options for change are limited, and the general "direction" for change is relatively linear, it is worth asking how or why this is true. From an institutionalist standpoint, the primary constraints on, incentives for, and indeed beneficiaries of change are institutions themselves – their arrangements, their form, their function, and, in particular (setting the "new" institutionalism apart from more traditional views of institutions within political science), their interactions (Thelen and Steinmo 1992, Gorges 2001).

As noted above, institutionalism and its impacts can be self-reinforcing. Early decisions constrain later options in a feedback loop. Pierson uses the illustrative example of a Polya urn, in which a large urn contains one red ball and one black ball. A ball is removed at random, and two balls of the same color are then placed back in the urn; the process is repeated until the urn is full. While each drawing is down to chance, the early draws shift the odds substantially in a cascading process (2004, 17).

In much the same way, early decisions about policy design or change reinforce themselves over time in a process engendering both path dependence (setting direction) and historical institutionalism (building the contextual atmosphere). In economics (from which the concept is largely borrowed) these self-reinforcing mechanisms are known as increasing returns, and the same term can be applied here (Ebbinghaus 2005, Pierson 2000).

Let me begin to draw a hypothetical scenario "inspired" by some experiences I had while a member of the Maine House of Representatives. Though not grounded in any one specific circumstance or event, I can say that the general sketches come from a series of somewhat similar circumstances encountered by various Maine government agencies over the past several years.[1] I will return to, and expand upon, this scenario in the subsequent sections on other theories in order to build an illustration of the "boundary crossing" or meta-theory framework with which I intend to address my final analysis.

Suppose that after a competitive bidding process, a government agency – the Piersonia Department of Finance – licenses a particular software program to process all of its incoming and outgoing payments. As per Piersonia rules for government contracting and purchasing, the Department awards the contract to the lowest-cost proposal it receives. It buys the necessary servers to host the program, buys the new computers necessary for its employees to use the program, invests in training the employees on how to operate the software, and sends multiple notices over several weeks to all of the citizens and business who either make payments to, or receive payments from, the Department in order to make them aware of how the new system works.

Then, on the day of the launch, a glitch in the servers causes a delay of the first payments. The Department must pay to fix the glitch, pay for new notifications to all those affected by the payment delay, and absorb the loss of productivity. Then a year later, an update to the software means new training is required for its employees. Six months later, another update means that the servers must be replaced, and six months after that another new update means that the employee workstations must be upgraded. Then a billing error is discovered which has resulted in years of overpayments to some individuals, which must now be recouped to the fullest extent possible. By

1 For examples of my examples, see "Computers Blamed For Maine DHHS Losing Track of Millions in Overpayments," http://www.govtech.com/computing/Computers-Blamed-for-Maine-DHHS-Losing-Track-of-Millions-in-Overpayments.html ; "Maine Asked to Refund Federal Government $9.2m For Overbilling Medicaid," http://bangordailynews.com/2012/07/26/health/maine-asked-to-refund-federal-government-9-2m-for-overbilling-medicaid/ ; and "Computer Glitch Delays Unemployment Benefits for 2,200 Mainers," http://www.pressherald.com/2014/01/23/unemployment_benefits_delayed_in_maine_/

now, any initial budgetary savings from having chosen the lowest-bidding vendor have long since been erased.

Finally, facing enormous public pressure to cut costs and spending rather than raise taxes or eliminate services, an "efficiency" proposal is made in the legislature of Piersonia to consolidate the billing services between multiple government agencies and departments, leading to savings from consolidation. It turns out, however, that the software at the Department of Finance is incompatible with the software at any other agency. In order to align all of the systems to a single compatible standard, enormous short-term costs will be incurred.

As a frozen moment in time, the choices available to decision-makers may seem binary – consolidate the systems, or not – and the outcomes equally so – save money, or don't. On closer inspection, however, is their choice really so simple?

Obviously, there is the matter of the substantial sums that have already been spent on the system at the Department of Finance, to say nothing of the other agencies and departments in question. The sum total of those prior choices – the long-term training, the computer systems, the relationship between the agency and its clients regarding the billing and payment processes – all equate to a form of *path dependence* wherein those self-reinforcing early decisions, even if made with the best of intentions (a competitive bidding process, the lowest-cost proposal), have now set both the individual agency *and the broader contextual actors* on a path that is difficult, if not impossible, to change (Liebowitz and Margolis 1995, Pierson 2000, Schmitt 2012).

Then there is the question of short term costs (in aligning the systems) versus long term savings (from the efficiencies realized by consolidation). However, even if the long-term savings are significantly more substantial than the short-term costs, the legislators are required by the constitution to balance the budget in a two-year (biennial) cycle: that is, the costs must be paid for immediately by either increased revenues or decreased spending, while the savings will be realized only in future budgets. The legislators cannot "book" the anticipated future savings in the current biennial budget (this is precisely how budgeting works in Maine, and was an endless source of frustration to me as a legislator). The Department of Finance cannot be blamed entirely for the situation, either, since they followed proper procedures in advertising and awarding the contract. In other words, choices in this scenario have been constrained by *institutional* *circumstance* (budgetary processes, constitutional checks and balances, procurement policies, etc) as well as *institutional interplay* (between the various agencies, between the legislature and the agencies, between the agencies and the contractors, etc) (Thelen and Steinmo 1992, Schmitt 2012).

This example, however, as a demonstration of both path dependence and historical institutionalism in policy change, fails to account for those circumstances in which policy changes, even significant ones, *do* occur. How is that possible? For that, we must turn to two additional explanatory theories: critical junctures, and public choice.

Critical Juncture Theory

In some ways, separating historical institutionalism and critical junctures as two separate theoretical schools downplays the close interrelationship between the two in the literature (indeed, some authors have gone so far as to say that "the concept of 'critical junctures' is an essential building block of historical institutionalism." (Capoccia and Kelemen 2007, 341)

What is a critical juncture? Definitions of the term itself tend to be both surprisingly accommodating of a range of interpretations (unusual in academic literature) and surprisingly vague (perhaps contributing to the level of agreement between them). Critical junctures can be understood as pivot points in policy direction – chances for new policy to be established, existing policy to be changed, or old policy to be eliminated (Collier and Collier 1991). They represent the joints in the "branching tree" view of policy, political, or institutional development (Capoccia and Kelemen 2007). They can result from an exogenous shock (such as a major economic crisis or a war) or from the realization of endogenous developments (Collier and Collier 1991, Hogan 2006). The actual timespan for change may exist for only a relatively brief moment, or they may stretch over a much longer period (Collier and Collier 1991). The outcomes of the juncture may be extreme change (Donnelly and Hogan 2012) or mild adaptation (Calder and Ye 2004).

As stated: surprisingly similar, surprisingly vague. Or perhaps it is better to say, agreement is broad, and definitions are tolerant. Within the wide borders of the definitions, some commonalities emerge, and the definition in Cappocia and Kelemen – "*relatively* short periods of time during which there is a *substantially* heightened probability that agents' choices will affect the outcome of interest" (emphases original; 2007, p. 348) – provides a solid foundation on which to unpack the theory and examine its implications for this paper. Put even more simply: "[c]ritical junctures are events that set processes of institutional/policy change in motion." (Donnelly and Hogan 2012)

The first benefit of critical junctures theory as either a corollary of, or add-on to, historical institutionalism can be found in the second half of the definition above: "agents' choices." While critical junctures theory is still rooted in the primacy of institutional settings and constraints, it allows for a heightened view of the capacity of individual actors to affect the course of development. This could include, but does not exclusively or explicitly refer to, individual persons or decisionmakers. With its long-term, institutional emphasis, historical institutionalism struggles to account for situations in which actors or actor preferences (beyond institutions or institutional processes themselves) impact outcomes in ways more direct than the norm (Hogan 2006). Critical junctures allows for an explanation of those anomalies.

The second benefit is that critical junctures creates a construct that can aid in explaining moments in which significant change occurs in ways, and/or to extents, which defy the institutionalist view of stable, change-resistant institutions established over long time horizons. In the long-form view of incrementalist change, abrupt and major shifts can be both startling and difficult to explain.

This, in fact, has also become a source of criticism of the theory: Gorges sounds a significant note of caution on the use of a "grab-bag of explanations", including critical junctures, by researchers (2001, p. 137) and both his article (p. 141) and the Capoccia and Kelemen piece (p. 343) use the phrase "deux ex machina" to explain a tendency of researchers to reach for critical junctures when the main theoretical framework cannot adequately account for a piece of the puzzle.

The primary concern in the employment of a critical junctures argument to analyze particular policy changes is in defining what precisely constitutes a critical juncture. When is change "critical," and when is it less significant? What time standard can be used to identify a critical juncture – that is, for how long can a process of change, even major change, drag on before it is considered incremental or gradual rather than abrupt or critical? (Hogan 2006, Gorges 2001, Capoccia and Kelemen 2007)

Capoccia and Kelemen further analyze the question of "near misses," which is of particular interest in my analysis. If the "stars align," so to speak, and meet the conditions *for* a critical juncture (a period in which there is a chance for unusual change), but no change occurs, then has a critical juncture really happened? They maintain that it is possible. In their view the *potential* for a change outcome, rather than a change outcome itself, is what defines a juncture, and note that "[i]f change was possible and plausible, considered, and ultimately rejected in a situation of high uncertainty, then there is no reason to discard these cases as 'non-critical' junctures." (352) There is disagreement on this point. Hogan, for instance, is unequivocal in his insistence that change – indeed, "significant, swift, and encompassing" change – is essential to the identification of a critical juncture (2006, 665).

This leads to the question of whether or not the failed 2005 Social Security reforms proposed by then-President Bush met the definition of a critical juncture, despite the lack of reform (this will be revisited in my final analysis). I find the arguments of Capoccia and Kelemen more persuasive. Think about the following:

1) path dependency and historical institutionalism demonstrate significant constraints on policy development by creating feedback loops and self-reinforcing processes; and
2) there exist brief intermittent phases in which there is a recognizable, identifiable opening for a departure from the norm; and
3) those moments pass *without* any change occurring, ; then
4) a new point of especially significant self-reinforcement has been created in which actors may now be able to identify that even in moments where "the stars aligned," nothing happened – and thus the capacity for change to be considered in future critical moments has been diminished.

Put another way, I do not believe that exceptional significance in policy development can only be achieved by unique change rather than by unique stability.

Previously, I established a scenario in which the Piersonia legislature, facing voter pressure to cut government spending and increase government efficiency, had proposed a measure to consolidate and centralize billing and payment procedures across multiple agencies – but would face significant short-term costs in doing so without the ability able to offset those costs with the projected long-term savings (because of their budgetary processes).

On its face, the scenario seems straightforward enough. The Piersonia legislature has limited options, as established; given that the rationale for consolidating the systems would be to save money, and the proposal would actually cause increased costs in the short term, the somewhat obvious, perhaps even only, option is to scrap the proposal.

Let me add a "wrinkle." 12 months before the proposal reached the legislature, Piersonia experienced a massive economic downturn as a result of a global financial crisis. Unemployment surged above 10% for the first time in two decades; ballooning mortgage payments caused hundreds of families to face foreclosure and abrupt homelessness; and polls quickly registered a massive spike in voter dissatisfaction heading into the upcoming elections. The Libercan party of Piersonia had enjoyed a more or less unchallenged majority for the past 45 years, but now poll after poll after poll showed their rival Republicrats surging ahead. Seizing on a message of "Fixing a Broken System" and "The Change We Need, Now", and on a platform of spending cuts and major reforms of "job-killing government waste" if elected, the Republicrats sweep the elections, winning the presidency and installing supermajorities in both chambers of the Piersonia legislature. At the time they take office, unemployment is still at around 11% and the economy shows no signs of recovery.

Let us assume that this financial crisis and electoral shock represents a critical juncture in every sense but the final analysis of whether or not change occurs: it meets the conditions of a moment in which conditions are favorable *for* change to occur. Within the limits of the scenario I have presented here, it still seems unlikely that the anti-spending, anti-waste Republicrats would embrace a proposal that resulted in significant increases in short-term spending, even if there were chances for consolidation and long-term savings.

Here, we reach the final piece of the puzzle as I view it: to explain what is going on in the minds of those policymakers, we need a final theory.

Electoral Gains Theory

What motivates a politician? I would define "politician," on my own terms, to mean an individual person in a position of political authority and electoral accountability. Generally that would indicate an actual elected official, though in some systems (such as the United States) it could also indicate somebody who holds a significant public office without having been directly or popularly elected, such as a cabinet secretary or appointed position (White House Chief of Staff, or National Security Advisor, for instance). The question of who holds such positions is ultimately in the hands of voters (the collective of whom I shall also refer to as "the electorate").

This gets at a more philosophical question of the role of an elected official in a republic – a question which I often confronted personally in office. Is it an elected official's duty to vote in ways that reflect the views of the majority of their constituents, even if they believe those votes to be in some way (morally, intellectually, or on the basis of evidence) misguided? Or is it an elected official's duty to vote in ways that they can personally "live with" or believe to be appropriate, even if their voters may not fully understand or approve of those votes?

I confess that I encountered some difficulty in identifying a specific theory that, in my view, adequately captured the extent to which I believe a policymaker's *perceptions* of voter reprisal impacted their political decision-making. As examined in my prior section on "The State of the Art," many authors studying welfare retrenchment have explored the implications and ramifications of the general popularity of social spending programs. There is very little (in fact almost zero) debate over the popularity itself: plainly put, the electorate tends to love these programs, and politicians know it (Pierson, Coping With Permanent Austerity: Welfare State Restructuring in Affluent Democracies 2001, 412-413). As was shown in Kitschelt's work, there is reason to believe that winning electoral strategies can be built around policy initiatives which would, from the conventional point of view, seem risky at best or politically suicidal at worst.

In short, the claim is that electoral factors may be bigger influences on policymaker preference than some in the field seem to believe. Indeed, both Pierson – "I have argued that the failure to take voters seriously helps to explain why analysts systematically underestimated the welfare state's resilience over the past two decades" (Introduction: Investigating the Welfare State at Century's End 2001, 8) – and Kitschelt – "my paper lays out mechanisms that may induce politicians to pursue often unpopular reforms based on internal opportunities offered by the dynamic of competitive party democracy that have received only scant attention in the comparative political economy and social policy literature (2001, 265) – have noted the lack of scholastic regard for commentary based in a politician's relationship with perceived voter preferences.

How can I claim a new theory based on so little work? I cannot claim that this theory is "ready for prime time," so to speak, but neither can I say that there *is* a theoretical construct – at least, not one of which I am aware – that provides the framework necessary for me to elucidate my findings. I can, in drawing on some other sources, provide the following introduction to what I propose.

First, we assume that politicians are rational actors seeking to maximize their preferred outcomes – "an actor's behaviour is likely to be driven, not by impersonal historical forces, but by a strategic calculus and, second, that *this calculus will be deeply affected by the actor's expectations about how others are likely to behave as well*" (emphasis added) (Hall and Taylor 1996, 945) – and that their most preferred outcome is, *ceteris paribus*, them and their political allies winning elections, or if not winning everything outright then at least collectively doing better than their opponents, or if all else fails, not losing their individual race. In this setting, policy outcomes may not have

The second half of the Hall and Taylor quote above is particularly relevant. Rationality, in this definition, hinges on the actor's *expectations* about the behavior of others. This leads to my critical second point: politicians' rationality is significantly bounded. That is, they behave rationally within the confines of what they know or expect or believe to be true (Simon 1972), but given that they also face significant impediments to perfect knowledge (information asymmetries, time lags, and – as posited in the preceding theory sections – institutional constraints) they will make imperfect decisions.

Third, as our path dependent view of historical institutionalism maintains, the institutional constraints on their actions will include self-reinforcing feedback mechanisms where policy decisions feed political outcomes, which feed policy outcomes, which feed political decisions –an endless Ouroboros of policy-politics feedback that creates a distinctly powerful *conventional wisdom* among politicians. I can attest that this conventional wisdom is nearly impossible to overcome with evidence, leading to much fear-based or, as I have also heard it referred to, "faith-based" rather than evidence-based policymaking (for a quick example, try to convince the average American politician to vote, based on evidence of its success over alternatives, in favor of drug decriminalization).

Finally, synthesizing the above into Kitschelt's findings, it is possible that unusual or unexpected policy initiatives could be viewed as electorally beneficial, and consequently receive significant attention that might appear inscrutable to the researcher accustomed to believing in the resilience of welfare programs or the difficulty in reforming them.

Before moving on to the main case analysis, let me finalize the Piersonia scenario. As we know from the previous section, the Republicrats are unexpectedly in a position of power amidst a major economic crisis, elected on a platform of cutting spending and eliminating government waste. A proposal to consolidate and centralize billing and payment software between various government departments and agencies would save money in the long run (fulfilling their pledge to the voters to cut waste) but incur significant short-term costs (reversing their pledge to cut spending).

Now that we have the lens of electoral gains theory, can we see an "out" for the Republicrats – a way for them to have their cake and eat it too? Their choices are vote for a spending increase (angering their electoral base with new spending but providing a positive campaign message of finding efficiencies that may appeal to the broader electorate) or drop the consolidation proposal (perhaps making nobody happy, since the waste continues despite the lack of new spending). Maybe they could embrace Pierson's techniques of "obfuscation" (fuzzying the issue) or "blame avoidance" (perhaps by claiming they were fixing a system that was broken long before they came in) – two tactics which seem to have been used with some regularity in welfare reform attempts (Giger and Nelson 2010, 1).

There is, of course, no right or wrong answer: this was merely an exercise to examine the influences and factors impacting policymaker decisionmaking, through the theoretical lenses discussed here. Now those lessons can be applied to the central analysis of this paper.

Case Selection and Methodology

In its absolute earliest iterations, this thesis was meant to involve a study of political factors influencing pension development and reform in less-developed countries. It quickly became quite clear that, first, there was not as much experience with reform in less-developed countries as there was (rather predictably) in affluent democracies, and second, ignoring my professional background and prior engagement in U.S. politics would be a waste of a resource. This initial plan was scrapped.

Subsequently, I considered a comparative case study between the United States and Japan. This was also rejected on the basis of finding greater value in a within-case, process tracing analysis of the 2004/2005 Social Security reform effort in the United States.

As noted by Capoccia and Kelemen, "because heightened contingency is a core characteristic of critical junctures…narrative process tracing [is] particularly important and must be explicitly employed to study them" (343). I believe the 2005 case is a good opportunity for an examination of the three theories mentioned above – and a test of my "electoral gains" theory – with the potential for serious counterfactual analysis in order to test my assumptions (D. Collier 2011).

Social Security Reform in the United States

The more or less modern incarnation of Social Security – the public system of old-age income security in the United States – came into existence in 1935 when then-President Franklin D. Roosevelt signed the program into law. From the time it was created until the late 1970s, changes to the program only expanded it or made the benefits more generous. The system expanded, several times, the population who had access to benefits (e.g. disabled adult workers, in 1956). A cost-of-living allowance, or COLA, was first granted in 1950. In 1972, COLAs were instituted as automatic annual increases in payments in order to keep pace with rising costs of living, and a 20% benefit increase was instituted (AARP 2010).

It was not until 1977 that the first unpopular reform would arrive, when an increase in the payroll tax used to fund the Social Security trust was coupled with an unusual benefits reduction in order to extend the solvency of the system. The benefits reduction only affected some individuals. An "error" in the initial formulas used in the calculation of benefit levels would have resulted in some individuals being eligible for higher benefits, in actual dollar value, than their pre-retirement wages. Projections showed that this would eventually bankrupt the system. The "notch fix," as it became known, seems to have been relatively uncontroversial, since it prevented a significant financial crisis in the system, resulted from an unintended error, and affected only some beneficiaries (The Senior Citizens League 2011).

In the early 1980s, during Ronald Reagan's first term as President, more gloomy projections about the future solvency of the Social Security system led the President to propose a reduction in benefits for existing beneficiaries in the system under the age of 65. As a test case for my consideration of the political ramifications of Social Security reform, this one was fairly clear: 96 members of the Senate voted against the proposal, with precisely zero Senators voting in favor. Eventually, a bipartisan compromise was reached that gradually raised the age at which benefits can begin being collected (from 65 years of age to 67, a measure which was phased in over more than 40 years from the time of passage and still has not entirely taken effect) (Dallek 2009).

From that point until 2005, little in the way of Social Security reform was attempted or even suggested: as Dallek writes, "The compromise also cemented a new reigning political consensus on Social Security—Social Security, in historian Sean Wilentz's words, was 'untouchable' because it had become more than ever the 'third rail' of national politics." (*Ibid*)

What made the program so untouchable? First, it is worth noting the staggering decrease in elderly poverty that correlates almost perfectly with the implementation and expansion of Social Security. While researchers at the National Bureau of Economic Research have cautioned against drawing broader causal inferences from the correlation ((National Bureau of Economic Research n.d.), the connection between the two (Social Security up, elderly poverty down) in the minds of advocacy coalitions, and consequently voters, and, consequently, politicians, has become a foundational component of the American "conventional wisdom" I previously mentioned as a significant factor in politician decision-making.

Second, the average age of the voting population has been steadily increasing; recently the median voter age in the United States hit 45 years old (Yen 2011). This, too, has contributed to a perception that the elderly vote in greater numbers than other voting cohorts by age, and consequently increases the political risks of tackling Social Security reform.

In fact, even now the 1983 compromise is recognized as a particular turning point in the history of Social Security (Dallek 2009), a critical juncture whose effects are still being felt – which brings us to the 2005 reform.

George W. Bush campaigned on a partial privatization of Social Security in his first run for office, in the year 2000, but his political agenda in his first term was largely subsumed by the so-called Bush tax cuts (passed in early 2001) and, very shortly thereafter, the September 11 attacks. During the 2004 election, however, then-President Bush once again made Social Security reform the centerpiece of his domestic agenda campaigning heavily on the prospect of allowing workers to divert a portion of their payroll taxes from the general Social Security trust into individually managed private investment accounts and focusing on it in his 2005 State of the Union speech before Congress (Sahadi 2005).

The 2004 election delivered a significant amount of political capital to President Bush: not only was he successfully reelected (far from a foregone conclusion through the race), but Republicans actually expanded upon their majorities in the Senate (increasing their majority from 51 to 55 seats out of 100)[2] and in the House (increasing from 229 to 231 seats out of 435) (CNN 2004). President Bush had campaigned on the issue largely on a message of allowing Americans greater freedom and broader choices in how to invest their "own" money for their own futures, which perhaps could be considered a form of obfuscation disguising the projected impacts to the Social Security trust of having enormous amounts of revenue diverted away into private accounts.

Still, the President threw himself behind the effort: a Brookings Institute report from 2007 looking back on the failed initiative describes a president who was eager to invest every ounce of political capital he could into a significant, keystone reform of the largest entitlement program in the country with the backing of his newly expanded Congressional majorities (Galston 2007).

Almost immediately, however, there were stumbling blocks. Galston notes similarities to the problems widely perceived as having doomed the aforementioned health care reform efforts by the Clinton Administration in the 1990s: not enough congressional consultation, and an overestimation of both the amount of capital gained in the election (the President was returned to

2 Despite their success nationwide, the Republican candidate for U.S. Senate in Illinois lost to a Democratic state senator with the somewhat unusual name of Barack Obama.

office with the lowest approval rating ever registered for a just-reelected chief executive) and the amount that would be necessary to pass the measure.

Kitschelt and Bonoli both noted the potential for blame avoidance and obfuscation to become more difficult tactics in the event that a single party controlled all branches of government: Congressional Republicans, drawing on feedback from the recent election as well as their own sense of the political atmosphere around the issue (and, as Dallek wrote, with the lessons from 1983 still fresh in the institutional memory), almost immediately began signaling their reluctance to pursue the President's proposal given the projected negative impact on the Social Security trust's long-term solvency, which even the tepidly supportive Alan Greenspan, then chairman of the Federal Reserve, admitted had to be a top priority (The Economist 2005).

The administration was also beset by a series of missteps in its handling of the public and congressional sides of the roll-out. First, Republican congressional leaders consistently felt that they were being left out of sensitive negotiations aimed at winning Democratic votes in order to pass a bipartisan measure (similar to the 1983 coalition): those Democratic votes may have been desirable from some political standpoints, but even then-House Majority Leader Tom Delay, a fierce administration ally, complained openly and bitterly about the administration's perceived mishandling of the optics (*Ibid*).

Second, the reception to the measure by the public was not quite what President Bush may have been expecting as he emphasized the benefits of expanded personal choice and investment freedom offered by his plan. The AARP denounced the very conceptual underpinnings of the plan almost as soon as President Bush was re-elected, and observers both of Congress and within Congress were quick to voice their doubts that the plan would get anywhere without the AARP's blessing (Pear 2004). The President's coalition of conservative allies, meanwhile, did not invest nearly what they had been expected to in mounting a public defense for the proposal (Galston 2007).

Mere months after winning reelection, the President's signature domestic initiative had been all but officially scrapped, delivering him his first and ultimately most significant political loss (Weisberg 2005).

Conclusions

What went wrong? How did a newly re-elected President, who had never faced a significant defection from his own party on a priority issue (let alone a significant defeat), lose the single largest piece of his entire second-term domestic agenda?

Despite the loss, was this a critical juncture for Social Security reform – a “near miss”?

I believe that this event can be safely considered as a “near miss” critical juncture. The definition of a critical juncture, as put forth by Capoccia and Kelemen, is “*relatively* short periods of time during which there is a *substantially* heightened probability that agents’ choices will affect the outcome of interest.” Given the President’s history of success with his own party and agenda in the first time, his reelection, the increased majorities in both chambers of Congress, and the expectation of significant outside support for his initiative, I believe the threshold of a “*substantially* heightened probability” of agents’ choices affecting the outcome was met.

The second test of a critical juncture is whether it causes a branch or a fork in the direction of policy. I believe it did, in the United States. As so many of the authors cited herein have written – perhaps none more forcefully than Pierson in “Coping with Permanent Austerity” – the general ideology surrounding welfare and entitlement spending in affluent democracies now seems to be one of austerity rather than expansion, and even though such reforms still carry certain electoral dangers, there is safe space for political gains to be made on a message that embraces a holistic message of parsimonious stewardship of public funds such as that put forward by President Bush in his campaign for partial privatization (Bonoli 2001, Kitschelt 2001).

The branching effect, however, was initiated by that failure. In my “electoral gains” theory, I noted that for a politician, the self-reinforcing “policy-politics-policy” mechanisms incumbent in an historical institutionalist view of policy change can cement conventional wisdom in a way that becomes difficult, if not impossible, to overcome in future policy debates. I feel that this effect is still being felt today as a result of the 2005 “near miss”: that is, the prevailing sentiment amongst political figures in Washington is that you cannot win on Social Security reform (Salsman 2011). If the conventional wisdom was that austerity was now, more than ever, a winning proposition, than what are the branching effects of such a significant loss on an austerity measure? As I noted in the beginning, while institutional constraints can certainly exist strongly over time, large-scale policy changes of the type being discussed here are generally only able to be initiated by the same legislators and legislative bodies that also serve as significant veto points in the process of change itself. They are the judge, the jury, and the executioner in a single chamber.

On the institutionalist note, I find significant credibility in a historical institutionalist explanation of the boundaries of the debate as it unfolded. The institutional and path-dependent constraints on what options were available for policymakers to consider were rendered quite obvious when any threat to the long-term solvency of the Social Security trust resulted in an immediate rejection of that policy option. That is to say, once it became known that partial privatization was a threat to the institutional equilibrium of Social Security, the policy ceased to be credible.

The limitations of my research are the same limitations felt by other authors in the field. Kitschelt laments the lack of "hard comparable data on social policy retrenchment" (299). I am inclined to agree with his assessment. So many definitions in the literature are so vague, or so tolerant of generous interpretations, as to be rendered frustratingly inexact to the point of near uselessness. While disagreement is of course both common and welcome in academic discourse, there is a difference between two firmly staked positions on opposite ends of an intellectual interpretation, and two loosely defined baskets of ideas that can be defended with little better than a shrug and a "well, it depends…"

If I can hope to have contributed anything, it is the idea that voters do matter, and particularly that politicians' *perceptions* of voter preferences matter more than the literature seems to account for. I am unaware of any circumstance under which a politician anywhere, ever, has been accused of being "perfect," but the imperfections in information flow and time lag render politicians especially imperfect actors. It is my belief that further study of the relationship between political figures and voters could add significant value and clarity to some pressing questions in this field.

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