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How Academic Research Shapes Labor and Social Policy

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Abstract

How academic research affects labor and social policy is viewed through a program evaluation framework that highlights the difficulties of determining the causal impact of such research on public policy. The impact is illustrated through a number of examples. My conclusion is that academic research can have a modest to substantial impact on policy. Its impact is enhanced if it has a number of key characteristics: high-quality; done by reputable researchers; synthesized and translated into a language understood by policy makers, the general public, and the media; credible champions who will broker and defend it, in the political process or in the public realm; timely; and, political acceptability.

The ideas of economists and political philosophers, both when they are right and wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men who deem themselves to be quite exempt from any intellectual influences, are usually the slave of some defunct academic economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back (John Maynard Keynes, 1936, p.383).

Rarely have I witnessed serious government attention being given to serious social science research (James Q. Wilson, 1978:82).

In the world of public policy a research paper initially is no more than a rumor believed by one person – its author (Henry Aaron, 1990:280).

I. Introduction

The picture of academics as “monks talking to monks” cloistered in tenure-protected ivory towers certainly fosters the image of social irrelevance. The fact that universities are one of the few organizations that have seldom, if ever, experienced a plant closing, mass layoffs, or the midnight (graveyard) shift furthers the cloistered image. Academic tenure, trumpeted under the

guise of freedom of speech rather than job security, can certainly protect social irrelevance. A talk-show host reading the titles of the top ten ridiculous Ph.D dissertation titles, one of which is “The Rectal Temperature of Hibernating Bears: A Probe into the Unknown” provides the nail in the coffin.

The cloistered and irrelevant nature of much academic research is parodied in Swift’s *Gulliver’s Travels* (p. 167). The Kingdom of Laputa is in complete disarray and subject to extreme misery. Every town, however, has its own academy where learned professors work on in never-ending projects such as extracting sunbeams from cucumbers and the reverse engineering of human excrement. The projects are always “in-progress” and “almost completed” (familiar phrases?) but never see the light of day (also somewhat familiar?) and certainly are never applied to practical issues that could benefit the population.

Certainly, there are counter perspectives, at least with respect to the practical relevance of academic research. This is the case, for example, with regard to the synergies and spillovers between universities and high-tech research developments as is the case with Silicon Valley in California, Route 128 around Boston, and the Raleigh-Durham triangle. These synergies, however, relate to practical developments in areas like information technology and bio-material research. In the area of social and public policy development, the links between academic research and policy development are seldom made. In fact, there is remarkably little research on the topic in spite of the extensive public support for universities and academic research.

My purpose herein is to deal with the elusive problem of evaluating the impact of academic research on public policy. As my epigrams indicate, the range of views go from “nothing to everything” so reconciling the differences will not be an easy task. The problem is compounded by the fact that policy making is an elusive and moving target. As stated by Lynn

(1978:17): “Policy making is not an event. It is a process that moves through time-consuming stages, beginning with public recognition that a problem exists, to the adoption of laws or a combination of measures aimed at dealing with aspects of the problem ... to the establishing and operation of a program, to evaluation, review and modification.” Research can be utilized at each of these stages of *problem identification*, *legislative action*, *policy adoption*, and *evaluation*. This implies both bad news and good news.

The bad news is that research can be circumvented at any of the stages. For example, it may not be used to identify important social problems, in which case the problems which it ultimately addresses are not the most important ones. Or research may not be used in the design of laws and policies for problems the research may have identified so that the effect of the laws and policies may be undone by the actions of private market participants. Or research may not be used to evaluate the policies, in which case research has little impact on identifying residual problems and suggesting changes or new initiatives.

The good news is that cumulative research that provides a stock of knowledge may be drawn at any stage. As Lynn (1978, p.17) states: “Despite the immediate pressure of events, there is usually time for significant social R&D. While the immediate questions may change over time, the need for research on fundamental issues is a continuing one. Though individual policy makers usually have short time horizons, the policy process has much longer ones, a circumstance hospitable to the time-consuming nature of knowledge production.”

Questions that I will address herein include: Does academic research affect public policy? What are the characteristics of research that appears to have, and to not have, an impact? Are there any other factors that may be necessary to interact with social research so as to achieve its potential impact on public policy? What are some examples that illustrate these points?

I begin with a sketch of a theoretical model that highlights the issues and the difficulty of establishing a *causal* link between research and policy. I then illustrate the issues and problems by examining some of the (very limited) literature on the topic in the labor and social policy area largely from the U.S. Reflecting the comparative advantage and experience of the author, a more detailed examination will be provided by some Canadian examples.

As the extensive quotes throughout the paper indicate, my methodology used is in the time-honored tradition of “vulture-metrics” – soaring high above the existing studies and scooping down to selectively pick the juicier parts.

My focus is on the influence of academic research and not of think tanks – the latter is beyond the scope of my analysis. Abelson (2000:216) indicates that over 1,300 think tanks exist in the U.S. and about 100 in Canada. He further cites the growing literature in this area but concludes “few definitive conclusions have been reached regarding their impact in policy making.”

II. *Some Theoretical Considerations*

In theory, evaluating how academic research affects public policy could be done using the same methodology as used in the evaluation literature for any other initiative. The procedure would take the general form

$$Y = X\beta + \alpha R + \delta R^*I + \mu$$

where Y is the output or public policy initiative in this case, such as increasing the minimum wage (or in continuous form such as the magnitude of the minimum wage increase); X is a vector of determinants of such policy initiatives, such as the type of political party in power, demographic characteristics of the voting population to reflect preferences of the median voter, resources from interest groups to reflect the intensity of preferences of groups who have a vested interest in supporting or killing the initiative; R is a measure of academic research (our key

variable of interest); I is vector of intervening variables that interact with research to make the research effective (or that requires the backing of the research for itself to be effective); μ is a random error term; and β , α , and δ are impact parameters to be estimated, with α and δ being the parameters of interest – α reflects the independent impact of academic research on public policy, and δ reflects how research influences other intervening variables.

While only a stylized illustration, the above formulation highlights the problems associated with trying to determine the *causal* impact of academic research on public policy, qualitatively. These are conventional difficulties in the program evaluation literature.

First and foremost, the key independent variable – academic research – may be endogenous and simultaneously determined. Policy makers who want a policy initiative in place may well foster the research to support the initiative. This fostering could come in various forms: commissioning background studies from sources known to favor the initiatives; designing the terms of reference in ways that will yield favorable results; “advertising” favorable results while “burying” unfavorable results; or, reviewing the research with suggestions tilted towards influencing the results or having them presented favorably.

Second, and in a related vein, an *association* between academic research policy outcomes does not imply *causality*. For causality to work, the research results would have to be so powerful as to have an independent impact on altering the behavior of the parties to change the policy outcomes in spite of vested interests or other influences. Following the evaluation literature, this could potentially be accomplished through various ways, not one of which is likely in the area of assessing the impact of academic research. For example, some policy initiatives could be randomly assigned the “treatment” of research, while others assigned no research in order to form the “comparison” group. Mean differences in policy initiatives would

indicate the effect of the research on policy. Alternatively, and more feasibly, one could search for “natural experiments” where differences in research on a topic are not likely the result of a desire to influence the policy outcome in a jurisdiction. This could be the case with research that “spontaneously” occurred as an innovation. The research could also be exogenous if it were done in and financed by other political jurisdictions or countries. Here, however, the relevance of the research to the jurisdiction where the policy change is being contemplated could be an issue – in fact, an endogenous issue if its relevance could be emphasized or buried. Other ways of dealing with endogeneity of an explanatory variable could be contemplated. Conceptually, these could involve finding exogenous instruments that influence but do not affect the policy outcome itself – obviously an elusive search.

Third, there may be other factors not controlled for in the statistical analysis that can affect policy outcomes. If these other factors are correlated with research initiatives but not controlled for, the research initiative may be “picking up” the effect of these other factors (i.e., the classic omitted variable bias). For example, if a particular political party is likely to support a certain policy initiative and that party is also likely to support research, the research may appear to be impacting the policy initiative, when in reality the political party is driving the initiative.

Fourth, selection bias can prevail. For example, we may only observe the research results for our key explanatory variable when they confirm the desired policy change. This can give the appearance that the research positively impacted the policy change, especially as they are “trotted out” to support the change. Otherwise, they may be “buried” or counter studies commissioned to offset their impact. As well, there may be a publication bias in that statistically insignificant results are not as likely to get published.

Fifth, there is obvious measurement error in any index of research that could be constructed. This is different from variation in research results – that is simply variation in the key explanatory variable – a variation that is desirable for measuring impact. The measured error comes about simply because of the uncertainty of the research results and the difficulty in weighing the quality of the results.

Sixth, as represented by the random error term, deviations can matter. Luck or unexplained factors may play a part. There are simply times when research, no matter how well done, will have little or no impact, and times when it may have an unusually large impact. My focus, however, is on the systematic relationship between research and policy.

III. *Illustrative Literature on Relationship between Research and Policy*

There is a small general literature on the relationship between research and public policy. While it is not cast in the framework of the program evaluation model sketched above, it does illustrate many of the points.

For reasons detailed below, my general conclusion is that research can have a moderate to substantial impact, but this generally requires it to be accompanied by other characteristics (interaction effects in the evaluation model above). Specifically, for research to be effective in the policy arena it should be of *high-quality* and done by *reputable researchers*. The research must be *synthesized* and *translated* into a language that is understood by policy makers, the general public, and the media. Its impact is enhanced if it has *credible champions* who will broker and defend the research in the political process or in the public realm, especially against vested interest groups. *Timeliness* also matters. Given the lags in the research process, research that is prompted by a policy concern may hit the public realm only after the policy interest has

dissipated. *Political acceptability* is obviously a key ingredient. There are simply times and conditions when the political process will not be altered by research results, and other times when it will be open to change that can be affected by such research. These issues are illustrated below, based first on a discussion of the small literature on the relationship between research and policy, followed by some more detailed examples.

Reverse Causality. Haveman (1987) analyzes the relationship between research and the war on poverty in the U.S. between 1965 and 1980. He documents the reverse causality whereby policy “causes” research as evidenced by how the large increase in budgets for such research (pp. 37-41) led to an explosion of research in the area, including research published in top academic journals (pp. 42-48). The largest increase occurred in the *Journal of Political Economy* – a journal that emphasizes the importance of incentives! Clearly there was “money in poverty” and the endogeneity of the research will make it difficult to disentangle its independent impact. It becomes difficult to determine if the process is “evidence-based policy” to which we generally aspire, or “policy-based evidence” with the evidence being produced to rationalize already pre-determined policies. As the expression goes: “It is amazing how large sums of money can clarify one’s thinking.”

The possibility that academic research may respond to, rather than lead, policy development has also been noted by others including Aaron (1978, 1990) and Gagnon (1990). Aaron (1978:17) states: “research reflects prevailing political moods at least as much as it influences them.” Heineman et al. (1997:61) raise the concern that research may lose its credibility if it becomes regarded as a “commodity to be bought and sold” and that “analysis primarily for the reinforcement of already made choices seems to be a common feature of political reality.” Heckman (1990:310) discusses the Faustian bargain often made by policy

activist social scientists of the 1960s: “Academic research was often funded to support specific policies. Promises to deliver timely specific results made large funds for research available. Many academic agendas were set by government officials who sought timely precise answers.” Lynn (1978:16) emphasizes how research may be used in the political process for tactical rather than substantive purposes: “a research program may be a device for keeping an issue alive or for delaying action.” Weiss (1978:33) states: “Social science research can be used by policy decision makers to delay action or to avoid taking responsibility for a decision. It can be used to gain recognition for a successful program – even to win re-election. Skillful use of research can discredit a political opponent or a disliked policy or maintain the prestige of an agency through its support of prestigious researchers.” Politicians cherry picking the research that fosters their case is also emphasized by Stromsdorfer (1979).

Impact of Early Research on Poverty. Haveman (1987) highlights that the early research on poverty affected policy in a number of ways. The research on the different definitions and dimensions of poverty “entered directly into public debate over antipoverty and social policy during the 1960s and 1970s ... [and] these advances in empirical research have generated federal government data collection and research activities designed to further improve the measures.” More generally, Haveman (1987:104) concludes: “There is little question that this research [on poverty] ... influenced policy debate and brought facts, knowledge and scholars themselves closer to the policy process.”

After assessing the growing field of evaluation research in general, Haveman (1987, p. 173) further concludes: “Policy analysis and evaluation research have progressed to the point that some studies have attained levels of reliability sufficient to have a major impact on the policy-making process, especially in the area of social policy.” Similar assessments are made in

Behn (1981), Mundel (1985), Rossi and Wright (1977), Stromsdorfer (1979), and Wildavsky (1985). These and other studies, however, have also emphasized the difficulty of influencing the political process given its own agenda and incentives (Aaron, 1978, 1990; Behn, 1981; Haveman, 1976, 1987; Covello, 1980; Lynn, 1978; Weiss, 1977, 1978, 1990). Survey evidence on the importance of political factors is given in Patton et al. (1977). What appears important is a blending of the interests and skills of social scientists with a policy interest, and policy makers with an interest in social science research (Berger, 1980:vii) as well as “research brokers” who will serve as intermediaries between researchers and policy makers (Sundquist, 1978).

Political Acceptability. The extensive research on the incentive effects of negative income taxes generated proposals under President Nixon for programs for families with children, but such programs were not implemented in part because research results showed adverse incentive effects on work and family stability. Haveman (1987:101) states: “Too liberal a scheme for conservatives and too conservative for liberals, it ultimately failed because of concerns over labor-supply reductions when its benefit-reduction rate was cumulated with those in other programs.” Or, as Aaron (1990:279) states: “When elected officials looked at the high cost of large-scale welfare reform and at findings about family stability, they shrugged off serious consideration of welfare reform, leaving the data on labor supply for dissertations, journal articles and simulation models.” Nathan (2000:49) concludes: “The immediate result was a decision to trim back the Carter plan [a type of negative-income tax] because the research results increased the cost estimates for the benefit schedule in the Carter proposal.” “The cost of covering millions of additional people under a negative income tax presented a policy choice that was simply not in the cards in the 1970s” (Nathan, 1988:61). Weiss (1978:61) more generally concludes: “In the broadest sense, the opinion of the public dictates what is acceptable and [what

is] not... the further removed research conclusions and recommendations are from mainstream opinion of major groups, the less likely the results are to influence policy in the short run.” In commenting on the subsequent use of measures to encourage or require work from able-bodied welfare recipients, Aaron (1990:278) states: “Social science can facilitate policy when it finds that measures congenial to the values of elected officials are at least modestly beneficial.”

Timing and Consensus of Results. Nathan (1988:57) also emphasizes that the lack of consensus on the research results of a negative-income tax harmed its being translated into policy and that this lack of consensus can be taken advantage of in the political process: “Unlike social scientists, politicians often regard ambiguity as a good thing. It can enable them to forge a coalition of different interests, each perceiving that its aims have a priority” (1988:125). Overall, he concludes: “I believe that the negative income tax demonstrations were moderately successful as research projects. They were much less successful, as an aid in policymaking. Their results came very late in the policy process, and were ambiguous to say the least” (1988:60).

The importance of timing in general as well as a reasonable consensus among researchers is also emphasized in various other analyses including Aaron (1990:277), Bernstein and Freeman (1975:138), Hanushek (1990:292), Weaver and Stares (2001:25) and Weiss (1978:63). The importance of timing and timeliness is furthered by the importance of policy windows when “problems are coupled with policy solutions at a time when it is politically feasible to implement change” (Soroka, 1999:768; Howlett, 1998; Kingdon, 1984).

Clearly, in the case of poverty research, the political climate and timing mattered, but so did the research results on work incentives and family stability. Although a full-blown negative income tax was never implemented, features of such a program were incorporated into

subsequent policies that tried to strike a balance (identified in the research) between transferring income to the poor while at the same time preserving work incentives for those who can work.

Policy Involvement of Researchers. Haveman (1987) further comments on the importance of the researchers themselves often being involved in the policy making process at some time in their careers. This theme has been echoed by others including Hartle (1981:248). As stated by Lynch (2005:15), a distinguished academic who served a stint as Chief Economist at the U.S. Department of Labor from 1995-1997: “Another way we can influence policy making as economists is to actually spend time in a policy making position.”

Incentive Incompatibility Constraints. The incentive incompatibility that can exist between the constraints of the policy world and academia have been highlighted by numerous commentators. Aaron (1990:279) states: “The paper that wins tenure is not likely to influence any decision maker. The paper that influences a member of Congress is likely to be dismissed as recreational, not professional, by one’s academic colleagues.” Weiss (1978:37) asserts: “If they [researchers] are faculty members of a university, they respond to the academic reward system. Rewards are based on publishing research results in books and journals and hewing close to the mainline interests of the discipline rather than getting enmeshed in interdisciplinary research, which is often derogated as watered down scholarship.” Davis and Salasin (1978:102) conclude: “Framing research in terms of national goals often means that the results will not get published in academic journals and thus earn university promotion.” (Williams, 1971:63) asserts: “The important social scientists who play for the highest stakes in the social science community – particularly peer prestige – do not dirty their hands much with that which is relevant to social policy making. Those who do such work are almost assuredly of lower caste.”

Translating Results. In part to set themselves apart from policy wonks and practitioners, academics often write in their own language.. There are many ways to restrict entry into an exclusive club, and that is one. To say something simply is to risk being taken as simple. An equation can indeed say a thousand words, but it is also true that a few carefully crafted words can often say what is laid out in a formal model. This does not mean that all academics should write in a fashion that is understood by the general public and policy makers. The principle of comparative advantage, however, suggests that some academics should bridge the gap and translate research into policy language, perhaps through synthesis pieces and overviews. Aaron (1990:279), for example, emphasizes the importance of “technical correctness, analytical balance, and clear presentation” as does Weiss (1978:69).

Survey Evidence on Impact. In one of the few studies that attempted to provide evidence on the impact of research, Caplan et al. (1975) interviewed 204 federal decision makers in the U.S. about their use of social science research. Almost all were able to give examples of the use of research; 85 percent felt it could contribute considerably to good policy making; and 87 percent felt that government should use it extensively. The authors conclude that research use in policy making can be described at least as modest.

Knorr (1977: 176) also surveyed government officials and reported that “about two thirds of the respondents (65.5 percent) claimed to have at least slightly changed their opinion about the problem on the basis of the results of the social science project.” Based on a survey of government decision makers in the health area, Weiss and Bucuvalas (1977:222) find that the quality of the research is the most important determinant of its being used in policy making.

Training Examples. With respect to the more recent research that suggested Job Training Partnership Act (JTPA) training for out-of-school youth was largely ineffective relative to JTPA

training for adults, Lynch (2005:7) states: “The policy and budgetary response to this research was rapid and sharp. We saw a significant shift of federal training funds away from youth and towards adults during the 1990s.” She further asserts (p.9) that “evaluation evidence on displaced workers programs relative to training for disadvantaged adults has had a significant impact on policy makers funding priorities [falling for the disadvantaged and rising for the displaced].”

III. Some Detailed Canadian Examples

Macdonald Commission and Free Trade. In Canada, the signature recommendation of the Macdonald Royal Commission of 1985, was for a bilateral free trade agreement between Canada and the U.S. That recommendation led to the Canada-U.S. Free Trade Agreement (FTA), negotiated between 1985 and 1987 and implemented January 1, 1989. The research of the Commission was extensive, involving 280 studies done mainly by 300 different academics in 70 volumes. While the research on trade was only one of seven areas under the economics umbrella, and economics was only one of three areas (the others were law and constitutional issues, and politics and institutions of government), the specific area of trade involved the most extensive analysis, occupying seven of the 70 volumes (See Gomez and Gunderson, 2006 for more details).

The importance of academic research to the Commission is also illustrated by the fact that 84 percent of the 1,014 references in the final report are to research studies (67 percent from the academic literature and 17 percent from the background research studies of the Commission which tended to synthesize the academic research). Only 10 percent of the references were to briefs formally presented to the Commission and 6 percent from references to transcripts of the

public hearings (calculations from data in Inwood, 2005:181). The fact that the academic research generally favored free trade while the briefs and public hearings generally involved advocacy positions opposed to free trade, suggests that the research also had a greater impact (Inwood, 1998:18).

The research on trade had a number of important characteristics that likely facilitated its impact on public policy. It was *high quality* research done by *top researchers* in the country and *coordinated* by a prolific and respected trade economist. The computable general equilibrium models were particularly influential, especially because they captured the indirect productivity enhancing effects of the restructuring that would occur because of the economies of scale for producing for a large market. The research of the Commission generally involved a *synthesis* of the *cumulative* stock of existing research, the vast majority of which favored free trade. The near *consensus* perspective favoring free trade is illustrated by the fact that “only one academic could be found to make the anti-free trade case out of the approximately three hundred hired by the Commission” (Inwood, 1998:35). This homogeneity of perspectives within economics and the rigor with which they are advanced made economics prominent as a source of policy advice to the Commission (Simeon, 1987). Brooks and Gagnon (1988:109) conclude that this is a more general phenomenon: “There can be little doubt that economists remain pre-eminent among social scientists in their integration with the policy process.”

The research also had *champions* who made the case for free trade to the Commissioners and to the politicians, and who defended it in the heated public debates that ensued. Trade unions strongly opposed the FTA and organized public forums against it. In countering this, Macdonald (2005:11) acknowledges the important role played by an Industrial Relations academic, John Crispo, for “his robust platform technique which ultimately frightened away the

union leaders from contested meetings where initially it was they who had brandished the verbal brass knuckles.”

There were certainly attacks on the research and on the academic case for free trade. However, the attacks tended to be polemic and based on more nationalistic denunciations of free market economics in general. They tended not to provide alternatives based on different methodologies, and the work was generally simply presented at conferences or published in forums of contemporary opinion as opposed to peer-reviewed academic journals (Inwood, 1998:5).

In his foreword for each of the research volumes, Macdonald commented on the importance of research for his Commission: “We had a wealth of information. We inherited the work of scholars at universities across Canada and we had the benefit of the work of experts across private research institutes and publicly sponsored organisations.” He also commented on the importance of having a stock of solid cumulative research to build the case, and to synthesize and translate that research so as to influence policy: “Our problem was not a shortage of information; it was to interrelate and integrate – to synthesise – the results of much of the information we already had.”

While the Canada-U.S. Free Trade Agreement of 1989 was certainly backed by considerable research support, numerous other factors were at work that may confound the influence of the research. As indicated by (Winham, 2005), other regional trading blocs were forming at the time, and Canada was at risk of not having secure access to a large market. This concern was compounded by the fact that Canada traditionally relied on exporting primary products, but the demand for such products was forecast to be in decline so that a reallocation towards manufacturing was considered desirable. There was also concern with the rising

protectionism in the U.S. that threatened access to Canada's largest market - over 70 percent of Canada's exports went to the U.S.

Freer trade was also regarded as a potentially effective way for the federal government to pressure provincial governments to adopt market-oriented reforms given the substantial control they have over policy initiatives in Canada's system. This was especially the case since there was a backlash against the nationalist and government interventionist policies that prevailed during the 1960s and 1970s, including wage-price controls, energy price fixing, foreign investment restrictions, government procurement policies, and a state trading corporation to assist smaller Canadian firms to sell to centrally planned economies (Chant, 2005:14). Such policies were often regarded as contributing to the worst recession Canada experienced since the Great Depression of the 1930s.

Interestingly, while he was previously in political office, Macdonald himself presided over many of these interventionist strategies including a national oil policy, a state-owned petroleum company, government investment in oil developments that were avoided by the private sector, price controls on uranium exports, and the wage-price control program. He attributes his conversion to free trade and less government intervention to: "My experience in the private sector after my departure from government made it clear that state-controlled programs had failed to achieve the rates of growth to which we all aspire" (Macdonald, 2005:9). This rejection of nationalist-interventionist policies also occurred for Prime Minister Trudeau who had earlier instituted many of the policies in the 1970s. By 1982, he indicated: "Personally, I remain convinced that the primary engine of economic development must be a dynamic private sector and that the marketplace is in most circumstances the best allocator of scarce resources" (Chant, 2005:17). The opposition Conservative party also supported market-oriented reforms

and made free trade a cornerstone of its election platform. After being elected in 1984, they moved quickly on the recommendation of the Macdonald Commission to negotiate the FTA.

Clearly, various circumstances were coming together to foster the free trade initiative. While the precise importance of research in that process is difficult to establish, the fact remains that the case for the policy initiative was backed by solid cumulative research, done by top scholars who were in general agreement. Furthermore, the research was synthesized, translated, and championed into the public realm. Whether the research would have had the same impact if it did not have these characteristics or the interaction with the other circumstances that were occurring is an open and interesting question.

Other Recommendations of the Macdonald Commission. While free trade was the signature recommendation of the Macdonald Commission, numerous other recommendations were made backed by labor and social policy research. As Riddell (2005) indicates, many of these recommendations were implemented into policy, including unemployment insurance reforms; active adjustment assistance policies; income supplements to the working poor; national testing of student achievement; and deemphasizing minimum wages.

The Commission recommended reductions in the generosity of unemployment insurance benefits (discussed in more detail later) since they involved passive income support that encouraged people to remain in declining sectors and regions. Instead, they recommended active adjustment assistance to facilitate reallocating labor from declining sectors and regions to expanding ones through such programs as training, wage subsidies, mobility grants, and early retirement plans. These recommendations appear to have led to policy changes in reduced generosity of unemployment insurance and more emphasis on what became known as Employment Benefit and Support Measures (EBSMs) involving support for job search, labor

market information, training, and wage subsidies – programs that would facilitate rather than work against market adjustments.

As Riddell (2005:63) points out: “There was a substantial increase in the measurement of student achievement at both the elementary and secondary levels, including the introduction of the Student Achievement Indicators Program, which provide data that are comparable across provinces and over time, as recommended by the Commissioners.” The intent was that such information would provide incentives for schools to improve their performance and for parents and students, as consumers, to make informed choices.

In his overall assessment, Bradford (1999/2000:158, 159) concludes: “The Macdonald Commission report remains the essential component reference point for the host of era-defining policy innovations, ranging from continental free trade to restrictions on unemployment insurance and retrenchment of the federal role in social assistance, legislated between 1985 and 1997 by successive Conservative and Liberal governments.”

There were areas where the recommendations of the Commission were not followed, although features of the recommendations were often incorporated into subsequent policy initiatives. In order to deal with the adjustment consequences of its market-oriented reforms including free trade, the Commission recommended not only active adjustment assistance programs but also a form of guaranteed annual income (termed the Universal Income Security Program, UISP) with a clawback rate of 20 percent over and above the normal income tax rate and applied to all income. The UISP was to replace other income support programs. That recommendation was not based on its own research since only one of its background studies considered this approach and that study recommended that the UISP not be pursued, largely

because it was not well targeted to those most in need and because the cost and extensive redistributive consequences would make it politically infeasible (Kesselman, 2005:74).

This could indicate that research was very important in the policy process. That is, the research of the Commission backed free trade (and the policy was implemented) and opposed a more universal income support program (which was not implemented in spite of the recommendation of the Commission). More realistically, the universal income support was not implemented because of the reasons outlined by the research – its poor targeting and high cost. The Commission itself may have realized the political infeasibility of a universal income support program but recommended it in any case because doing so made their more market-oriented recommendations more palatable by dealing with the adjustment consequences. However, the universal income support recommendation was not completely ignored: Important features of that program were incorporated into subsequent programs such as the National Child Benefit System of 1998 (Kesselman, 2005:74; Riddell, 2005:63)

Unemployment Insurance. In 1971, Canada's unemployment insurance scheme underwent major reforms that made it one of the world's more generous systems. Elements that made it so included: near universal coverage (93 percent of the paid labor force); eligibility based on only eight weeks or more of insurable earnings; easier qualify periods and longer benefit periods for regions of high unemployment; a relatively high benefit replacement rate; and the continued coverage of self-employed fish harvestors (in spite of the moral hazard problem that they could lay themselves off to collect UI). Those reforms were based on the recommendations of a 1970 White Paper on Unemployment Insurance (HRSDC, 2004) although they were not backed by extensive research. The rationale appeared more political – to assist depressed regions and the

fishing industry – the only sector where the self-employed were eligible. The reforms were part of the more interventionist views that were prominent at the time, as discussed previously.

In 1996 major reforms were instituted, backed by considerable research, that reduced the system's generosity and tried to restore its original insurance principles rather than a passive income maintenance scheme. The reforms were instituted because of concerns about the adverse incentive effects: Some workers and employers structured their employment relationship to take advantage of the UI system, and UI discouraged unskilled workers especially in seasonal industries from improving their skills and encouraged them to be repeat users who relied on UI as a way of life.

As outlined in Gray (2004), the main reforms in 1996 involved: a reduced benefit rate; eligibility based on insurable hours rather than weeks worked; higher eligibility requirements for new entrants and re-entrants so as to foster insurance principles of paying into the system before drawing from it and to discourage youths from starting off drawing UI and others from developing a cycle of repeat use; an intensity rule or experience rating applied to individual workers whereby the new benefit rate would be reduced for repeat UI users and the provision of incentives to work more than the minimum qualifying period.

Part II of the new legislation also established the Employment Benefit Support Measures (EBSMs) designed to provide active labor market adjustment policies in such areas as job search, training, wage subsidies, and self-employment assistance.

The research behind the 1996 reforms was extensive and of high quality. The Macdonald Commission (1985), criticized the UI system as contributing to the duration of unemployment, encouraging temporary lay-offs, reinforcing the concentration of temporary and unstable jobs in high-unemployment and low-wage regions, and subsidizing, and therefore, encouraging repeated

unstable employment. Among other things, the Commission recommended more experience rating (where employer premiums would be based on the UI claims of their workers in the recent past) and eliminating regionally extended benefits. As well, it recommended returning UI to its original insurance principles with reduced benefits and more stringent eligibility criteria. As stated by Riddell (2005:62): “In some cases, such as UI reform, one can point to a fairly direct line from the Report’s diagnosis and recommendations to those ultimately implemented in the 1990s.”

The Macdonald Commission, which dealt with UI only as one part of its mandate, also recommended the establishment of a commission to focus more directly on UI reforms. This was done, and the Forget Commission reported in 1986. The report generally followed the Macdonald Commission recommendations except for experience rating and recommended that eligibility be based on an hours requirement rather than the 10-week requirement.

As indicated, the 1996 reforms came after extensive, high quality research that highlighted the adverse incentive effects of UI (studies for this section are cited in Benjamin et al., 2002:561-66; Gunderson, 2004; Gunderson and Riddell, 2001). These adverse effects occurred in various forms: increasing the incidence and duration of unemployment; entering the labor market and working just long enough to be eligible for UI; accepting a job just when UI eligibility expires; sustaining “occurrence dependence” whereby each spell of UI leads to a longer subsequent spell, as users get to know how to benefit from the system; sustaining seasonal and cyclically sensitive employment; and, discouraging labor mobility out of high unemployment regions.

The recommendation for employer experience rating was also supported by considerable research from the U.S. where experience rating is prominent and where the research shows that

experience rating reduces layoffs and unemployment (Gunderson, 2004). It does so by ensuring that firms with low layoff rates do not cross-subsidize those with high layoff rates.

In spite of the strong theoretical and empirical support for experience rating of firms, such a policy was never adopted likely for political reasons as it would disproportionately affect firms in high unemployment regions and especially the Atlantic provinces –the federal government is sensitive to such regional pressures. As indicated, a form of individual experience rating was adopted in 1996 by reducing the benefits for repeat users. Although empirical evidence indicated this was having its intended impact of reducing repeat use such individual experience rating was abolished in 2001, also for political reasons – its disproportionate impact on Atlantic Canada. In the area of experience rating, political pressure seems to have trumped research in influencing policy.

The 1996 reforms to reduce the generosity of UI and expand active adjustment assistance by facilitating job search, training and mobility also were consistent with the research evidence recommending a reallocation from passive income maintenance programs like UI towards more active adjustment assistance measures to foster the reallocation of labor from declining to expanding sectors and regions. While often critical of the overall effectiveness measures like publicly supported training programs, much of that evaluation literature emphasized specific aspects of active adjustment assistance that tended to be incorporated into the Employment Benefit Support Measures (EBSMs), including: low-cost job search assistance for those who can be matched to jobs; early identification of individual needs followed by early intervention with individualized action plans targeted to the needs of specific groups with appropriate combinations or bundles of programs delivered through a single-wicket or one-stop shopping network; utilizing the private sector for on-the-job training and delivery, and downplaying public

training and job creation; requiring job search or training to maintain eligibility; and, providing positive incentive bonuses to persons on UI to encourage them to leave the system (Gunderson, 2004).

Overall, the increased generosity of Canada's UI system, especially through the reforms of 1971, seems to have occurred for political reasons, but subsequent retrenchments especially in 1996 reflected the research results documenting the adverse incentive effects of UI. Nevertheless, the failure to adopt firm-level experience rating and the abolishing of individual experience rating, in spite of research results favoring such policies, highlights that political pressures can trump research if the pressures are sufficient.

Workers' Compensation. In Canada, the workers' compensation systems of the various provinces have been under intense scrutiny in part because of escalating costs in spite of dramatic reductions in accident rates. Many of the higher costs are associated with the rapid increase in diseases and syndromes that are difficult to monitor as being legitimate claims or arising out of workplace accidents.

As was the case with UI, the research in this area generally emphasizes the incentive effects of workers' compensation (see Gunderson and Hyatt, 2003, and other reviews cited therein). The research, for example, finds that experience rating reduces injuries and claims as well as the severity of injuries and duration of claims, although the severity and duration results are not always robust. Limited evidence also suggests that longer waiting periods before benefits can be claimed (akin to a deductible in insurance policies) also lead to fewer injuries and claims. More generous workers' compensation systems lead to a substitution into those programs and away from other income support programs as individuals engage in "forum shopping" for the more generous program. Importantly, higher benefit income replacement rates (often 85 to 90

percent of lost earnings and perhaps more when other benefits are included) have adverse incentive effects in various dimensions: increased injuries and claims; increased duration of claims; increased fatality rates; and, reduced probabilities of returning to work. The importance of incentive effects is further highlighted by the fact that claims duration is increased the most for claims that are harder to diagnose as legitimate or not.

While determining the exact influence of the research is difficult, most jurisdictions in Canada are reducing the generosity of their workers' compensation benefits as well as engaging in more aggressive claims management and expanding experience rating.

Labor Relations Legislation. The Woods (1968) Task Force was "the first in-depth study of Canadian labor management relations" (Abbott, 2000:30) which was backed by an extensive research program of 88 studies designed to fill the gap from the fact that "the research vacuum that has existed has been detrimental to the development and formation of appropriate policy in industrial relations" (Woods, 1968:242). "A comprehensive research program was essential if they were to be in a position to formulate analysis and recommendations based on more than informed insight into the country's industrial relations environment, structure, procedures and problems" Woods (1968:240).

The background studies were of high quality and done by reputable researchers. As Adams (1991:145) states: "The names of the authors who undertook studies for the Task Force constituted the then "Who's Who" of Canadian labour and employment policy and their studies reflected a wide-ranging consensus over the content and direction of Canadian labour market regulatory policy."

Knowledgeable observers have commented on the report having a substantial impact. Adams (1991:146) indicates: "Not only was the Canada Labour Code enacted in line with many

of the recommendations of the Task Force, but almost every collective bargaining statute in Canada was subsequently amended to pursue the course chartered by the Woods Task Force Report.”. Abbott (2000:30) asserts that “a number of provinces adopted legislation that was directly derived from the Report ... virtually all of the 1972 reforms [of the federal legislation] derived from recommendations in the Woods Report.” Weiler (1980:11) also indicates: “Since that report was released in 1968, there has been an explosion of labour law reform right across the country.”

IV. *Concluding Observations*

A direct *causal* connection between academic research and labor and social policy is difficult, if not impossible to establish, especially because there is evidence of reverse causality whereby the policy process understandably drives much of the research. As well, research can be utilized or circumvented at each of the various stages of policy making: *problem identification, legislative action, policy adoption, and evaluation.*

Nevertheless, the literature suggests there is some effect. The polar extremes of zero and everything, suggested at outset are invariably not correct. But that leaves a big range. If forced to pick a modifier for the impact, I would chose something between “moderate” and “substantial.” Research is neither a necessary nor sufficient condition for policy change. It is not a necessary condition because policy changes have clearly been instituted without research or that go against the research findings. It is not a sufficient condition because research on its own will not be enough to change policy no matter how solid the research.

The examples and conclusions of the studies I analyzed herein highlight a number of key ingredients for research to affect policy. Research is more likely to have an influence if it is of

high quality and done by *reputable researchers*. They, or others, must *synthesize* and *translate* it into a language that is understood by policy makers, the general public, and the media. Its impact is enhanced if it has *credible champions* who will broker and defend it, in the political process or in the public realm, especially against vested interest groups. *Timeliness* can be important given the lags in the research process can lead to research that is prompted by a current policy concern hitting the public realm only when the policy interest has dissipated. *Political acceptability* is also key. There are simply times and conditions when the political process will not be altered by research results, and other times when it will be open to change that can be affected by such research. While pin-pointing the precise impact of research on policy is not feasible (beyond some, likely ranging from moderate to substantial) more confidence can be placed on these key ingredients for research to have an impact.

NOTE

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