

What Can the ESF Learn from US Evaluations of Active Labor Market Programs?

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My thoughts on the issues discussed in this paper have benefitted from my interactions with a number of scholars over the years, including (but not limited to) Jim Heckman, Dan Black, Michael Lechner, Carolyn Heinrich, Burt Barnow, Lars Skipper and Arthur Sweetman. I am very grateful for those interactions, and for comments from Jessica Goldberg, but, of course, retain all responsibility for the (occasionally provocative) views expressed here.

Introduction

This paper (briefly) addresses three themes related to the evaluation of Active Labor Market Programs (ALMPs), drawing on evidence from the North American¹ experience and contrasting it with current practice in Europe. I begin by making the (measured) case for greater use of random assignment methods in Europe, including both familiar and, I suspect, less familiar, arguments. Second, I make the case for greater (which in many European countries means “any”) use of serious cost-benefit analysis as a component of the evaluation of ALMPs. Third, I discuss the organization of the evaluation “industry” in North America and offer some suggestions about lessons it provides for the organization of evaluation in Europe.

This conference comes at an opportune time given the explosion in non-experimental evaluation work related to ALMPs in Europe. The papers by Kluve (2006) and Card, Kluve and Weber (2009) describe and meta-analyze this work; see also Bergemann and van den Berg (2010). The European Social Fund surely deserves praise for venturing across the pond in search of ways to improve the quality and quantity of this evaluation work (broadly conceived to include performance management). At the same time, I think it well worth noting that the US and Canada have much to learn from the countries at the top of the European evaluation league tables as well. Lessons worth learning include both the general value of rich, well-maintained and relatively accessible (to qualified researchers and with appropriate privacy protections) administrative data and the value of specific data elements such as caseworker ratings of the employability of the unemployed and detailed, complete data on educational qualifications. Though this view may generate some controversy, I read the recent non-experimental evaluations of the US Workforce Investment Act (WIA) program by Heinrich, Mueser, Troske, Jeon and Kahvecioglu (2009) and Hollenbeck (2009) as indicating that existing US

¹ I use “North American” in the Canadian manner to mean the US and Canada but not Mexico.

administrative data systems do not quite have what it takes to provide compelling impact estimates. Perhaps this is not surprising given that the design of current US administrative data systems did not include program evaluation as an objective. On another policy dimension, certain European countries have also done a good job of implementing, documenting and studying regimes of sanctions for benefit recipients not sufficiently inspired by the “carrot” side of activation policies. Recent examples here include Arni, Lalive and van Ours (2009) and Svarer (2007). The US has sanctions in some programs but not, to my knowledge, much in the way of good data on them or (what follows immediately from the lack of good data) much in the way of good studies. A related but different point concerns the sometime conflation in these sorts of discussions of US policy with optimal policy. I make neither the claim that current US policy is optimal in any meaningful sense for the current US context or that all or even most of the good things about current US evaluation policy can easily transfer to Europe. Nonetheless, I will argue for the view that some aspects of US policy and practice suggest reforms worth considering in some (if not all) European countries.

The tremendous heterogeneity among European countries in the current state of research evaluating the performance of ALMPs and, more broadly, the heterogeneity in the relevant political and research institutions and in evaluation capacity also deserve note. Some European countries remain at the very beginning of the process of seriously evaluating their programs, while others have much to teach the North Americans. It (almost!) goes without saying that different aspects of the North American experience have relevance to different countries in Europe, depending on the current state of play in those countries.

Even on the topics directly covered in this paper, much remains unsaid due to space limitations. In addition, I have not considered a variety of other topics closely related to the

evaluation of ALMPs such as recent developments in the literature regarding data and methods for non-experimental evaluations – see e.g. Dolton, Smith and Azevedo (2008), Fredriksson and Johansson (2008), Sianesi (2004) and many others – performance management – see e.g. Radin (2006), Barnow and Smith (2004) and Heckman, Heinrich and Smith (2002) – statistical treatment rules – see e.g. Smith and Staghøj (2009) and the references therein – and the broader issue of the role of caseworkers as gatekeepers, monitors and information providers – see e.g. Lechner and Smith (2007) and Buurman and Dur (2008). These omissions reflect not lack of interest or importance but rather division of labor over time and among authors.²

Experimentation

As a quick perusal of the *Digest of the Social Experiments* (Greenberg and Shroder, 2004) makes clear, the US has conducted the vast majority (indeed, all but a handful) of social experiments, most (but not all) of them related to active labor market programs, primary and secondary education and the criminal justice system.³ The situation has not really changed since the publication of that volume. In the US, experiments have provided evidence of great value for both policy and for our understanding of social interventions more broadly in areas as diverse as health insurance, electricity pricing, responses to domestic violence, educational interventions related to teachers, schools and curricula and, of course, ALMPs. Widely hailed in the social science community – see e.g. Burtless and Orr (1986) and Burtless (1995) - social experiments have the key advantage that their simple design makes them easy to explain and hard to argue

² For broad general treatments of evaluation methodology, see e.g. Smith (2000) and Smith (2004) for non-technical surveys, Heckman, LaLonde and Smith (1999), Imbens and Wooldridge (2009) and Blundell and Costa-Dias (2010) for somewhat more technical surveys and Abbring and Heckman (2007) and Heckman and Vytlačil (2007a, 2007b) for recent technical overviews.

³ I distinguish here between social experiments and both lab experiments under fully controlled conditions and the small scale field experiments that have taken the development literature by storm over the last decade. For discussions and categorizations see e.g. Levitt and List (2009) and Banerjee and Duflo (2009).

with. This gives them a policy-influencing power not enjoyed by even the cleanest non-experimental designs.

In addition to these direct benefits, experiments have the underappreciated benefit of providing high quality data for other research purposes. In addition to the large literature that uses experimental impact estimates as a benchmark for the study of various combinations of non-experimental estimators and data – see e.g. LaLonde (1986), Fraker and Maynard (1987), Heckman and Hotz (1989), Friedlander and Robins (1995), Dehejia and Wahba (1999, 2002), Smith and Todd (2005a, 2005b) and many others, experiments also have yielded a lot of substantive knowledge, particularly about low income labor markets, and have provided a platform for methodological analyses of heterogeneous treatment effects that avoid the complications associated with first dealing with selection bias – see e.g. Heckman, Smith and Clements (1997), Bitler, Gelbach and Hoynes (2006) and Djebbari and Smith (2008). Experimental data have even helped researchers to learn about structural models (in the sense that economists used that term) as in Todd and Wolpin (2006) and Lise, Seitz and Smith (2009).

The literature documents a variety of limitations of experimental evaluations relative to non-experimental evaluations. These limitations weigh against the advantages just discussed. At a most basic level, technological, political and ethical concerns make it impossible to randomly assign some treatments of great interest, such as gender or family background. Except in unusual circumstances, such as the Progresa evaluation in Mexico where random assignment took place at the level of relatively isolated villages, experimental evaluations capture only the partial equilibrium effects of policies; see Angelucci and Di Giorgio (2009). Depending on the placement of random assignment in the process of treatment receipt and on the availability of substitutes from other sources, both treatment group dropout and control group substitution often

complicate the interpretation of the estimates from experimental evaluations of ALMPs; see the discussions in Heckman, Smith and Taber (1998) and Heckman, Hohmann, Smith and Khoo (2000).

The implementation of random assignment sometimes requires institutional changes that may compromise external validity. In the case of the US National Job Training Partnership Act (JTPA) Study (NJS), the local sites in the experiment were concerned that the requirement of the design that they serve roughly the same number of participants while also filling a control group would mean digging deeper into the pool of potential participants. Depending on the nature of this pool and of the selection process, doing so could mean serving people with lower expected impacts. Some sites reacted to this by changing the nature of their selection process, e.g. reducing the number of visits to the center required to enroll, so as to reduce the extent of attrition during the process. Obviously, such changes compromise the external validity of the results. The scientific and political desirability of using volunteer sites also has implications for external validity. As documented in Doolittle and Traeger (1990), in the US National Job Training Partnership Act (JTPA) Study (NJS), more than 200 of the (approximately) 600 local service delivery areas were contacted, and a substantial amount of money was spent on side payments, in order to induce 16 sites to volunteer to participate, and even then at least one site left the experiment early. This issue often arises in IES evaluations of educational interventions as well. A related but different point is that heterogeneity in the size and organization of local sites may limit the set of sites at which it makes budgetary sense to do random assignment. The presence of random assignment may also alter the behavior of potential participants in ways that less salient and intrusive non-experimental methods might not. For example, it might induce additional selection on risk aversion, or might deter complementary investments. Such changes,

sometimes dubbed “randomization bias” in the literature, are distinct from Hawthorne effects, which result from the mere fact of observation, and pose yet another threat to external validity. Heckman and Smith (1995) and Section 5 of Heckman, LaLonde and Smith (1999) summarize these concerns about experiments.

In addition to these real issues, policymakers and program administrators sometimes offer up “ethical” objections to random assignment. In my experience, these objections nearly always represent a cover for simply not wanting to know the answer. Experiments often provide compelling evidence that treatments do not work at all or do not work well enough to pass a cost-benefit test. Educational researchers have dubbed the What Works Clearinghouse, a formal compendium of quality-rated evidence on the impacts of educational treatments funded by the Institute of Education Sciences and operated by Mathematica Policy Research, the “Nothing Works Clearinghouse”.⁴ This usage illustrates the very real empirical pattern that many, maybe most, programs fail when subjected to serious evaluation. Programs that deliver ineffective treatments, and so do not benefit their participants, still benefit important constituencies, such as the workers and agencies or firms that provide the treatments. Indeed, one sometimes suspects that it is these constituencies, and not the population served, who represent the real reason for the program’s existence in the first place. These constituencies have an interest in the production of low quality (and sometimes deliberately manipulated) non-experimental evaluations and misleading performance measures in place of compelling experimental (or even non-experimental) evidence.

One way to confront these specious ethical arguments is to point out what they miss, namely the problematic ethical position of taking money by force from the long-suffering

⁴ The What Works Clearinghouse can be found at <http://ies.ed.gov/ncee/wwc/>.

taxpayer to pay for programs without any serious evidence that they pass cost-benefit tests when such evidence could easily be produced. Such “speaking truth to power” provides the warm glow of righteous satisfaction, and carries some sway with stakeholders not completely in the service of their own narrow interests, but does not always carry the day.

Variants of random assignment that do not require the complete denial of service to any potential clients constitute another response to the phony ethical arguments offered up against random assignment, as these arguments typically revolve around concerns about service denial. In contexts where some eligible individuals would not receive service anyway, advocates of serious evaluation can (and do) frame random assignment as an equitable way to allocate scarce resources. In contexts where resource constraints do not bind, variants of random assignment that do not assign anyone to a no-services control group can help to derail malicious objections.

The literature offers three variants of random assignment that (more or less) avoid a no-treatment control group. One rather obvious variant consists of random assignment with multiple treatment arms but no control arm. For example, in the WIA context one might randomly assign some clients to only core and intensive services, while excluding them from training services. Another variant consists of a randomized encouragement design, as in Hirano, Imbens, Rubin and Zhou (2000). Here eligible individuals get randomly assigned an incentive to participate. Thus, no one is excluded, but the incentive, when properly designed – learning about the impact of the incentive represents a side benefit of the design – induces exogenous variation in treatment status. The design identifies what the literature calls the local average treatment effect (the LATE) rather than the average treatment effect on the treated. Put less technically, this design identifies the mean impact on those induced to participate by the incentive, but not the mean impact on all participants. Whether or not this parameter merits attention depends on the

particular policy context. The final design consists of randomization at the margin, as in Black, Smith, Berger and Noel (2003). This design does create a no-treatment control group, but only of individuals on the margin of participation. In the case of the Kentucky Worker Profiling and Reemployment Services System analyzed in Black et al. (2003), the margin consists of individuals whose predicted durations of benefit receipt put them in the last cell of treated individuals in a given local office in a given week. The state was willing to randomize these individuals but not those with long predicted spells. Like the randomized encouragement design, this design does not identify the average treatment effect on the treated, but does identify the average impact of treatment for individuals at the margin of treatment. This parameter answers a different policy question of what the mean impact would be on individuals brought into the program by an increase in the number of slots. As with the randomized encouragement design, this parameter might have greater or lesser policy importance than the average treatment effect on the treated.

At the end of the day, the push for random assignment evaluations of ALMPs (and other policies as well) has great value. For example, the zero (and sometimes negative) impact estimates for youth in the NJS led to large budget cuts in that program, budget cuts an order of magnitude larger than the cost of this (quite expensive) evaluation; see the discussion in Heckman and Krueger (2003). The experimental findings from the National Job Corps Study presented in Schochet, Burghardt, and McConnell (2008), which include positive impacts that fade out and so fail to pass a cost benefit test given the high cost of the program, have led to some serious thinking about that popular and, prior to the evaluation, essentially untouchable program. Some of the IES experimental evaluation results, such as those for the Teach for America Program (Glazerman, Mayer, and Decker, 2005), abstinence-only sex education

programs (Trenholm, Devaney, Fortson, Clark, Quay and, Wheeler, 2008), reading and mathematics software (Campuzano, Dynarski, Agodini, and Rall, 2009), and intensive teacher mentoring programs (Isenberg, Glazerman, Bleeker, Johnson, Lugo-Gil, Grider, and Dolfen, 2009), have had real impacts on expenditures and on the course of policy innovation and research. The Europeans can, and should, get in on this worthwhile game.

Cost-benefit analysis

Cost-benefit analysis combines impact estimates with information on programs costs to produce a direct policy conclusion. In the case of impact estimates that capture the average effect of treatment on the treated, a comparison of the impacts with the average cost of the program provides a clear and direct message about the value of program to the taxpayers who fund it. Historically, many US evaluations have included at least rudimentary cost-benefit analyses. The cost-benefit analysis associated with the US National Job Corp Study presented in Schochet, Burghardt and McConnell (2006) represents a particularly fine example.

In contrast, one can look pretty hard and not find very many European ALMP evaluations that include serious cost-benefit analyses. The paper by Munch, Skipper and Jespersen (2008) provides a notable Danish example. The (otherwise quite nice) paper by Aderonke (2009) shows a more common situation, with only a very rudimentary comparison of costs and impacts. More generally, and despite these counter-examples, the modal European ALMP evaluation, at least in my experience, contains no cost-benefit analysis at all.

One hears a number of reasons given for the absence of cost-benefit analysis in European evaluations of ALMPs. The most common answer concerns the European focus on employment impacts, rather than earnings impacts, mainly for political reasons. This focus on employment

has led to a lack of good administrative data on earnings in some countries, which makes cost-benefit analysis more challenging, as the researcher (or the literature more broadly) must come up with a compelling way to translate employment impacts into monetary units. In contrast, impacts on earnings, the most common case in North America, fit easily into a cost-benefit framework. Another reason sometimes given for the absence of cost-benefit analyses in Europe relates to the fact that the estimated employment impacts often turn out negative or zero or, in the bright and sunny cases, positive but small enough to make the negative result that would emerge from a serious cost-benefit analysis obvious in advance. This is the “why bother when the programs do not really work anyway” argument, and it has some sense to it.

The lack of good cost data also poses a barrier to serious cost-benefit analysis in many European contexts (and some North American ones as well). Ideally, one would have detailed data on both average and marginal costs for each service offered, broken down geographically in cases where costs varied substantially by, for example, location in a large city, a small city or a rural area. Instead, researchers often have available little more than the program budget and the total number of persons served. This lack of good cost data has always puzzled me, and represents a major difference between the public and private sectors. When this author worked at Farrell’s Ice Cream Parlor Restaurant⁵ (a small chain with less than \$100 million in annual sales) back in the late 1970s and early 1980s (a time of largely paper-based accounting), he was impressed to learn that the management knew the cost and the profit margin on every single menu item, including the free glasses of ice water that customers received upon reaching their table. Surely the public can expect government programs spending hundreds of millions or even billions of Euros a year to do as well as a lowly chain of ice cream parlors? This seems like a

⁵ Interested readers can learn about Farrell’s at www.happyitis.biz and www.farrellsusa.com.

basic fiduciary responsibility of public sector managers; their private sector counterparts would lose their jobs for neglecting it.

I do not want overstate the simplicity of coming up with good cost numbers. For example, both JTPA and WIA have attempted performance standards measures that included a cost component. These have faced real difficulties in assigning costs shared by JTPA or WIA and other programs, as when a variety of programs, often each having multiple funding sources, all share a common physical location as a “one stop” center. These common cost allocation issues (and others) are real and challenging, and carry over directly from performance measures to the problem of creating meaningful cost information for use in cost-benefit calculations. At the same time, private firms face similar difficulties and a large literature and equally large body of empirical practice in accounting lays out reasonable ways to deal with them.

In addition to its value at informing decisions about keeping or dropping programs, cost-benefit analysis has the further benefit of encouraging thinking about important aspects of program design and evaluation, and of public policy more generally. First, it encourages thinking about the outcomes an ALMP will affect. A focus on outcomes other than just earnings, in particular on crime, represents one of the notable aspects of the Job Corp cost-benefit analysis highlighted earlier. Not only do impacts on crime account for much of the gross impact of the program, particularly in the short term, their presence tells us a lot about how the program works, and suggests other possible treatments that might well pass a cost-benefit analysis.

Thinking about outcomes and about the behavioral theory that links treatments to outcomes also leads to a salutary focus on the possible general equilibrium effects (which include spillovers or displacement effects) of programs. Johnson (1980) and Calmfors (1994) are classic references; see Lise, Seitz and Smith (2009) and the citations it contains for pointers to

the more recent (and still much too small) literature. While difficult to estimate, they deserve a place in cost-benefit analyses, if only in the form of a sensitivity analysis using informal estimates drawn from the broader literature.

Thinking about cost-benefit analysis in a serious way also highlights the importance of learning about the duration of program impacts. Most evaluations of ALMPs provide only a year or two of follow-up. The available evidence on longer-term impacts suggests that sometimes impacts remain remarkably steady over time for years after an intervention, as in the National Supported Work Demonstration (Couch, 1992) and the National JTPA Study (US GAO 1996), other times they fade out, as in the National Job Corps Study (Schochet, Burghardt and McConnell, 2008) and the California GAIN program (Hotz, Imbens and Klerman, 2006), and other times they appear only belatedly, as in the evaluation of German training programs by Lechner, Miquel and Wunsch (2004). The absence of both a clear general empirical pattern and compelling theory on when estimates should persist and when not suggests the value of more frequently undertaking long-term follow-up, so as to minimize the impact of extrapolation of the sort described in Heckman, LaLonde and Smith (1999).

Finally, paying attention to cost-benefit analysis focuses policy and research attention on two important parameters: the discount rate and the marginal social cost of public funds or “excess burden”. Having a well-justified social discount rate for use in government budgeting and investment decisions represents a basic task of public finance economists. As noted in e.g. Heckman, LaLonde and Smith (1999), the discount rate employed to bring future net impacts (and costs, if applicable) forward in time to the present can affect the outcome of a cost-benefit analysis. Also important, and routinely ignored in North American cost-benefit analyses (including otherwise exemplary ones like that from the Job Corp evaluation), is the fact that a

dollar of government budget for ALMPs costs society more than a dollar, both because the operation of the tax system directly consumes real resources (all those cheery Internal Revenue Service agents have to get paid) and because all developed countries rely on distortionary tax systems. While estimates of the marginal social cost of public funds vary widely in the literature even for specific countries, and we would expect them to vary across countries due to differences in tax systems and tax rates and other institutional features, the estimates never equal zero and often reach magnitudes that suggest the policy importance of incorporating this factor into cost-benefit analyses and thereby into decisions about program existence and funding.⁶

In sum, cost-benefit analysis represents a useful tool, both in a direct sense via its role in clarifying and systematizing decisions about program existence, expansion or contraction and indirectly via its direction of policy and research attention to important, but often neglected, issues of program design and impact and of public finance more broadly.

Organizing evaluation

Surprisingly little research seeks to document and explain differences in the quantity and quality of ALMP evaluation across countries. I am aware of Riddell (1991) and not much else. Given the heterogeneity in both quality and quantity obvious even to the most casual observer, this gap in the literature comes as a surprise. Filling the gap represents a worthy task for researchers.

Because of this gap, my remarks here rely mainly on my own observations as a scholar studying evaluation methods, a provider of evaluation short courses to graduate students at various locations in Europe, a referee and editor handling academic evaluations and an occasional evaluation consultant as well as on discussions with friends in the academic and policy worlds.

The lack of quantitative evidence on national variation in quality and quantity necessitates the

⁶ See, e.g. Auerbach and Hines (2002) for a survey.

following caveat: I am well aware that low quality research, such as PriceWaterhouseCoopers (2004), with its smiley faces and confusion of outcome levels and impacts (see e.g. page 15) or Gregory (2000) with its distinctive “sites of oppression matrix” evaluation tool, appear everywhere, including the US and Canada, because of the universal demand for evaluation reports that promote the views of interested parties while providing an appearance of technical understanding and objectivity sufficient to fool the reading public.

I will argue that differences in the quality and quantity of evaluation research across countries result from much more than simply differences in the industrial organization of the evaluation industry, but those differences play a role, and make a good place to start my discussion. The evaluation industry in the US combines government, private for-profit firms, private non-for-profit firms and academia in remarkable and complex ways that differ across program types. For ALMPs, both not-for-profit and for-profit firms, operating on contract to the US Department of Labor, have undertaken many of the evaluations of large programs such as JTPA, WIA, the Job Corps, the Trade Adjustment Act and so on. Additional evaluation work is performed by academics operating with research funding from places like the National Science Foundation or private foundations; this work often uses data from the original DOL-funded evaluations, as with the long series of papers by Heckman and various co-authors using the data from the NJS; see Heckman, Ichimura, Smith and Todd (1988) for an example. Other evaluation work, including process evaluation work and impact evaluation work on private firms, is also often contracted out to a somewhat wider set of firms than the small number of large firms (e.g. Abt, Mathematica, MDRC etc.) with the capacity to undertake large evaluations. These firms compete in both the product market and the labor market; in regard at least to economists, they compete for the same newly minted doctorates as academic economics departments just outside

the top twenty. Some evaluation work is also done in-house at USDOL, whose staff includes people trained in economics at the doctoral level. A similar pattern holds in the education world, though probably with more academic involvement in the actual performance of the evaluation work, as opposed to simply advising or undertaking secondary analyses using the data generated by evaluation conducted by others.

What makes the European evaluation market different from the North American ones? First, some European countries have an important player in their markets that is absent in the US in the form of (mostly or entirely) government supported research institutes devoted to labor market policy and evaluation that operate (more or less) at “arms length” from the government itself. I have in mind here the IFAU in Sweden and the various institutes in Germany (e.g. the ZEW in Mannheim, the IZA in Bonn, the DIW in Berlin, the RWI in Essen and so on). My understanding is that these institutes have both base funding and do work on contract. They maintain a remarkable degree of independence, in the sense that they routinely report evaluation results indicating that ALMPs have zero or even negative impacts (and other more humorous but still somewhat embarrassing-to-the-government findings such as paternal leave being more common during hunting season and such like).

Neither the US nor Canada has any direct analog to these institutes. The US Government Accountability Office (formerly the General Accounting Office) does some work along the lines of process and implementation evaluation but not much in the way of econometric impact evaluation.⁷ The closest analogue in Canada, the Auditor General, is even less like the European Institutes. The US Congressional Research Service largely confines itself, as I understand it, to literature surveys. While I could imagine the Canadians setting up something like the IFAU, I

⁷ For an exception, see US GAO (1996), which presents long-term impact estimates for the JTPA experiment using administrative data.

find it hard to imagine the US doing so, in part because it would present real competition to the various DC “think tanks”. I do think these institutes represent a valuable component of the European scene, and countries that do not have them, and where they would “fit” politically, institutionally and culturally, ought to think about having them.

Size represents a second important contrast between the evaluation market in the US and that in Europe (and in Canada, for that matter). Size has two relevant dimensions here. The first is the simple magnitude of evaluation research going on. The US spends quite a lot of money on evaluation in a number of policy areas, including for programs that it funds in developing countries. To the extent that evaluation firms, whether for-profit or not-for-profit, have economies of scale over some range, a larger market can support more firms and thus allow more competition between firms. The second dimension of size concerns the number of potential clients for evaluation research firms. My sense is that evaluation firms in the US face many more potential clients both at the national level (where they might deal with the departments of labor, education, housing and urban development, health and human services, homeland security, transportation, agriculture, and so on and in some cases even separate parts of particular departments) as well as the development banks, states and larger cities and private foundations. This diversity of potential clients reduces the dependence of the firm on repeated interactions with a single client and thus, I think, reduces the potential costs associated with catering to the truth rather than to the client agency. Firms in smaller European countries with highly centralized governments and no private foundations may face a much, much smaller number of potential clients and so face much stronger pressure to bend to the client’s wishes of the moment.

One easy way to increase the size of the European evaluation market is for that market to become truly European, rather than national. At present, I am aware of very little evaluation

work that happens across boundaries in Europe. Transforming small national markets into a much larger European market would allow greater competition between providers and would give firms more freedom to avoid clients seeking a particular answer rather than necessarily the correct answer. I think entry by the major US firms into the European market would aid in these developments. This has happened in a very limited way in the UK, with MDRC playing a role in the experimental evaluation of the Employment Retention and Advancement Demonstration (Miller, Bewley, Campbell-Barr, Dorsett, Hamilton, Hoggart, Homonoff, Marsh, Ray, Riccio, and Vegeris, 2008). More activity on this front would, in my view, bring great benefits.⁸

The absence of institutions like APPAM in Europe represents another difference of note. The key feature I have in mind here concerns the interaction between academics, government consumers and producers of evaluation research, evaluation firms and policy people interested in the results of evaluations. Bringing these groups together, both via the annual meetings and via APPAM's publications and other activities, represents an important contribution not duplicated, to my knowledge, by any European organization. Efforts to replicate APPAM in Europe, with some linkages and occasional joint conferences as with the Society for Labor Economics in North America and its younger European compatriot the European Association of Labor Economists, would add value.

Finally, you have to want it. At a narrow level, this means having at least some people in government who care about evidence more than they care about the party line or about their narrow bureaucratic imperatives of budget increase and career advancement. It needs to encompass both the levels of administration that change at election time and those that do not. It also means that some people at both levels have to understand enough about evaluation to know what to ask for and to evaluate what gets produced in response. I think the US practice of having

⁸ This same point applies to Canada as well.

serious academics spend brief stints in the national administration, say as chief economist at the US DOL or on the Council of Economic Advisors, plays an important role in the (very much relative) success the US has had on this dimension, and commend such institutions to European governments. The temporary nature of the appointments matters here precisely because you do not want the academics to assimilate into the bureaucratic culture. Rather, you want them to maintain their outsider perspective and their academic devotion to getting the right answer (helped along by the threat of ridicule from their university friends and colleagues if they sell out).

The Bush II administration provides a useful illustration here. At the labor department, evaluation research became a low priority during this administration. More broadly, the department had such a poor reputation in regard to its interest in evidence that it could not manage to fill the chief economist position with a serious academic economist (for eight years!). Contrast this to the distinguished list of chief economists under Clinton, which included Larry Katz and Alan Krueger. In contrast, less than a mile (or two kilometers, for metric system readers) away, the US Department of Education – in particular the Institute for Education Sciences under Russ Whitehurst – made a serious run at transforming the entire field of educational policy evaluation through a program of experimental and high-quality non-experimental evaluations as well as the funding of a training grant program to create a generation of new, quantitative, serious education policy evaluators with disciplinary roots at least partially outside of traditional schools of education; see the discussion in IES (2008). How do you create more places like IES? I must confess that I do not have a good answer here, but we should be thinking about it, because doing so has a very high payoff indeed.

More broadly, the demand for serious program evaluation has to come from somewhere. It can come from leaders within government. It can come from actors outside government, such as the media and public intellectuals. It can come from the general public. But it must come from somewhere. Casual empiricism suggests a link at the country level between the quality and quantity of evaluation and the imprint of neoclassical economics. Countries with long neoclassical traditions, including the UK, the Netherlands, and the Nordic countries, are pretty much the same as those with long traditions of serious research within the neoclassical tradition. Looking within countries, Germany has gotten serious about empirical evaluation research only in the last 15 years or so, a time period that coincides with the triumph of neoclassical economics within academic economics in that country. This observed link between the demand for evaluation and neoclassical economics might reflect a causal relationship. Alternatively, both demand for serious policy evaluation and the dominance of neoclassical economics may reflect broader and deeper differences across countries in individualism, deference to authority, the importance of social class, average education and so on. Regardless of whether the current relationship reflects causality or not, one might argue that increasing the number of individuals trained in economics, particularly a practical version of economics rather than just high theory or theoretical econometrics, at both the undergraduate and graduate levels might represent a long term strategy for increasing the demand for quality policy evaluation as well as the ability to supply it with domestic labor. Who knows, it might even improve European agricultural policy as well!

Concluding remarks

This short paper has touched on three important issues where the ESF can learn from the North American experience in evaluating ALMPs. I have argued that current European practice lies very far from the point where the marginal value of additional experimental evaluations would equal their marginal cost. I have also argued that Europe would benefit from much greater attention to careful cost-benefit analysis following evaluation. Such analyses would allow the evaluation results to provide more guidance to policy and, more broadly, would increase our understanding of how policy works and so aid in the design of future policies. Finally, I have argued that much room remains for improving the organization of evaluation in Europe. The European environment includes distinctive and valuable aspects not present in North America, but could usefully incorporate aspects of the North American experience as it seeks to improve the overall quality of European evaluations.

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