Cover Sheet

EVALUATING THE WORKER PROFILING AND REEMPLOYMENT SERVICES SYSTEM USING A REGRESSION DISCONTINUITY APPROACH

Dan Black
University of Chicago, NORC, and Syracuse University
1155 E. 60th Street
Chicago, IL 60637
Phone: 315-443.9046
Fax: 315-443.1081
black-dan@norc.org

Jose Galdo
McMaster University and IZA
Address: KTH Building 426, Hamilton, ON, L9H2P7
Phone: 905-525.9140x26095
Fax: 905-5218232
galdojo@mcmaster.ca

Jeffrey Smith
University of Michigan, NBER, IFS, IZA, PSI and ZEW
Address: 238 Lorch Hall, 611 Tappan Street
Ann Arbor, MI 48109
Phone: 734-764.5359
Fax:734-764.2769
econjeff@umich.edu

Corresponding author: Jeffrey Smith

Session Title: Social Insurance Programs: Good for Workers? Good for the Labor Market?

Session Chair: RAJ CHETTY, University of California-Berkeley and NBER

Discussants: CHRIS BOLLINGER, University of Kentucky
THOMAS LEMIEUX, University of British Columbia
AMY FINKELSTEIN, Massachusetts Institute of Technology
OLIVIER DESCHENES, University of California-Santa Barbara

EVALUATING THE WORKER PROFILING AND REEMPLOYMENT SERVICES SYSTEM USING A REGRESSION DISCONTINUITY APPROACH

By Dan Black, Jose Galdo, and Jeffrey Smith*

Economists have long recognized the incentives the unemployment insurance (UI) system provides for workers. The system motivates claimants to extend their unemployment spells by subsidizing both additional job search and leisure; see Bruce D. Meyer (1995) for a review of the empirical evidence. At the same time, the literature does not fully recognize that modest changes in the attractiveness of UI can potentially lead to sizeable changes in worker behavior and, as a result, substantively important changes in UI expenditures.

Dan A. Black, Jeffrey A. Smith, Mark C. Berger, and Brett J. Noel (2003) provide experimental evidence of a large impact of the U.S. Worker Profiling and Reemployment Services (WPRS) system in the state of Kentucky. The WPRS system imposes the requirement that claimants receive low intensity (in terms of both the state's money and the claimant's time) re-employment services early in their spells in order to continue receiving benefits. Black et al. (2003) find that imposing this requirement leads to a substantial increase in mean earnings as well as reductions in the mean amount and duration of UI benefits received. However, as we describe in detail in the next section, the nature of the experimental design utilized in Black et al. (2003) limits the generalizability of their findings.

This paper provides non-experimental estimates that generalize to a broader claimant population while relying on the same data on Kentucky's WPRS program from June 1994 to October 1996 as in Black et al. (2003). Our estimates exploit a series of sharp discontinuities in the assignment of the requirement to receive re-employment services inherent in the implementation of the WPRS program. In our context, the nature of the institutions makes this design particularly credible. Our evidence leads to conclusions similar to those reached by Black et al. (2003) regarding the effects of the WPRS program for the broader population we consider but also demonstrates the sensitivity of discontinuity-based estimates to econometric details in samples of moderate size.

I. The WPRS Program

In November 1993, President Clinton signed into law the Unemployment Compensation Amendments of 1993, which required states to launch worker profiling and reemployment systems. These systems seek to reduce the duration of unemployment spells for claimants with a relatively high probability of exhausting their 26-week entitlement to UI benefits by identifying such individuals among new claimants using a statistical profiling system. Claimants predicted to have long spells must participate in reemployment services such as job training and job search workshops early in their spells in order to continue receiving benefits.

Under WPRS, each state develops its own statistical profiling model. The Kentucky model, widely viewed as the most sophisticated of the state models, includes individual characteristics, information on past employment and UI benefit receipt, and information on local economic conditions.[1] The model produces a predicted value of the fraction of his or her UI benefit entitlement that each new claimant will collect. These values are then collapsed into a discrete score ranging from 1 to 20. Claimants predicted by the model to exhaust 95 to 100 percent of their UI benefits receive a score of 20, while claimants predicted to exhaust 90 to 95 percent receive a 19, and so on. Each local employment office assigns claimants to the WPRS treatment in descending order by profiling score until it fills the number of slots available for that office in a given week. In most offices in most weeks, either the available slots suffice to treat all of the new claimants or else the last profiling score treated exactly exhausts the number of available slots. In the remaining cases, where the number of claimants with the marginal profiling score (the last one treated in a given office in a given week) exceeds the number of slots, random assignment allocates the marginal claimants to the remaining slots. Black et al. (2003) call these 286 sets of claimants "Profiling Tie Groups," or PTGs.

For our non-experimental analysis, we augment the PTG sample in two ways. First, each PTG potentially yields two discontinuities, one at the upper end obtained by using the control observations from the PTG and adding treated observations with higher scores, and one at the lower end obtained by using the treated observations from the PTG and adding untreated observations with lower scores. To maximize the sample size available for the analysis, we instead create a single "fuzzy" discontinuity from each PTG by setting the discontinuity point in the underlying continuous prediction variable at the midpoint of the marginal profiling score associated with the PTG. We call the sample associated with each such discontinuity a Regression Discontinuity Group (RDG). We gain a large number of additional RDGs from office-week pairs without a PTG, but with both treated and untreated claimants. For example, a particular office-week pair might have three treated claimants with profiling scores of 15 and two untreated claimants with scores of 14. These RDGs embody a "sharp" discontinuity. As a result, while we have 286 PTGs with 1,981 claimants, we use at least 994 RDGs and 5,167 claimants for our non-experimental estimates.

Using the sample of RDGs rather than the sample of PTGs to construct our estimates implies three major changes relative to Black et al. (2003). First of all, we move from experimental estimates to non-experimental estimates. Second, we greatly increases the range of profiling scores, weeks, and offices used in the estimation, even though at most 40.4 percent of the 2,734 office-week pairs in our data have an RDG. Third, unless we assume a common treatment effect among all claimants, it changes the nature of the average treatment effect we estimate. We discuss identification in more detail in the next section.

II. Empirical Approach

We implement two alternative econometric estimators, one parametric and one non-parametric. Both exploit the discontinuities in treatment status associated with the RDGs. We first consider a simple non-parametric Wald estimator. To construct the Wald estimate, we calculate the mean differences in outcomes between claimants above and below the discontinuity that defines each RDG and then take the weighted average of these differences, using as weights the proportion of all treated observations in the sample in each RDG. The Wald estimator consistently estimates the corresponding population average treatment effect on the treated.

To implement the Wald estimator we have to decide how wide a window to consider on each side of each discontinuity. As usual, we face a tradeoff between bias and variance in making this choice. A wider window decreases the variance of the estimates by increasing the number of observations used to construct them but (except in special cases) increases the bias. We present three window widths: 0.05 (one profiling score), 0.10 (two profiling scores) or, for comparison purposes, the full sample. Because the distribution of treated units among the RDGs changes with the window width, different widths imply not only using different observations to construct the mean outcomes but also different weights on the RDGs.

The parametric estimator we consider follows that employed in Wilbert van der Klaauw (2002). We posit the following model for the outcome for claimant "*i*" in RDG "*j*":

(1) $$Y_{ij} = \delta T_{ij} + g(P_{ij}) + \eta_j + \varepsilon_{ij} ,$$

where Y_{ij} denotes the outcome, $T_{ij} \in \{0,1\}$ indicates treatment status, $g(P_{ij})$ denotes a continuous function of the predictions P_{ij} from the profiling model used to define the scores, the $\eta_j, j = 1,..., J$ denote RDG fixed effects and ε_{ij} indicates the error term.[2] The assumption of continuity of the conditional mean of the outcomes in the underlying continuous predictions P_{ij} implies that receipt of the WPRS treatment is the only source of any discontinuity in outcomes. The assumption of a common $g(P_{ij})$ for all of the office-week pairs with RDGs means that the particular office-week pair in which a claimant starts a spell affects conditional mean outcomes only through the RDG fixed effect, η_j. Attempts to allow $g(P_{ij})$ to vary by office and/or by week failed due to the modest sample sizes of many of our RDGs. In a common effect world, OLS estimation of (1) consistently estimates the common effect. In a heterogeneous treatment effect world where the heterogeneity does not depend on the included covariates, OLS estimation of (1) consistently estimates the mean impact of treatment on the treated for all of the treated units in the sample (a somewhat different parameter than that estimated by the Wald estimator).

We minimize the importance of our parametric functional form assumptions in two ways. First, we impose a caliper on the data we use to estimate the parametric model. Second, for each outcome and each value of the caliper, we select the order of our polynomial approximation to the population $g(P_{ij})$ function using a penalty function proposed by Hortugu Akaike (1970). In almost all cases, this results in a linear or quadratic function using calipers of 0.05 or 0.10.

III. Results

Panels A and B of Table 1 present estimates from the parametric estimator and the Wald estimator, respectively. Within each panel, the rows correspond to different dependent variables: weeks receiving UI benefits, amount of UI benefits received, and annual earnings, all measured over the 52-week period starting in the first week of the UI claim. The first three columns of estimates show the effect on both estimators of varying the caliper/window width used to define the estimation sample. The final column presents experimental estimates that use only the PTGs taken from Table 2 of Black et al. (2003). Except in a common effect world, even if the identifying assumptions underlying our discontinuity estimators hold exactly, those estimators converge to a different treatment effect parameter than does the experimental estimator.

Our estimates provide strong evidence that the Kentucky WPRS program affects the behavior of UI claimants. Consider first the parametric estimates with a 0.05 caliper. They indicate average treatment effect of 2.26 fewer weeks of UI benefits received, $175 less in benefits received and an earnings increase of about $636.[3] Expanding the caliper to 0.10 does little to change the point estimates but does reduce the estimated standard errors. Using all of the data from office-week pairs with an RDG in the third column results in an odd positive impact for the amount of benefits but does not affect the other estimates much. In our view, operating without a caliper asks too much of our parametric model.

The Wald estimates in Panel B tell a story that differs only marginally from that implicit in the parametric estimates. With a window width of 0.05, we obtain similar estimates for weeks of benefits and amount of benefits, but a noticeably smaller, but still negative, estimate for amount of benefits. As expected, moving to a non-parametric estimator leaves us with larger estimated standard errors. Increasing the window width to 0.10 leads to another anomalous positive impact estimate on amount of benefits received, but leaves the other two estimates largely unchanged. Implementing the Wald estimator using all of the observations in the week-office pairs with an RDG reduces the estimated standard errors quite a bit, but yields an implausible large and positive estimate on amount of benefits received. Overall, the Wald estimates imply substantive inferences similar to those of the parametric estimates; methodologically, we find that, as expected, the performance of the Wald estimator declines fairly rapidly with the window width. Although they correspond to somewhat different population parameters, the similarity of our discontinuity based estimates to the experimental estimates in Black et al. (2003) suggests that the common effect assumption does not go too far wrong, at least within these groups.

IV. Conclusion

This paper exploits the sharp discontinuities in treatment generated by the Kentucky profiling model to estimate the effects of the WPRS on three labor market outcomes. Application of both parametric and non-parametric estimators that exploit these discontinuities reveals that the Kentucky WPRS program accomplishes its aims of shortening the duration of UI claims, reducing total benefits paid, and raising annual earnings. For an extended analysis that includes additional parametric and non-experimental estimators as well as further sensitivity analysis, see Black, Jose Galdo and Smith (2006).

REFERENCES

Akaike, Hirotugu. “Statistical Predictor Identification” *Annals of the Institute for Statistical Mathematics* 22(2) pp. 203-217.

Black, Dan A., Jeffrey A. Smith, Mark C. Berger and Brett J. Noel. “Is the Threat of Reemployment Services More Effective than the Services Themselves? Experimental Evidence from the UI System.” *American Economic Review*, 2003, 93 (4), pp.1313-1327.

Black, Dan A., Jose Galdo, and Jeffrey A. Smith. “Evaluating the Selection Bias of the Regression Discontinuity Design Using Experimental Data.” Unpublished Manuscript, 2006.

Meyer, Bruce D. “Lessons from the U.S Unemployment Insurance Experiments.” *Journal of Economic Literature*, 1995, 33, pp. 91-131.

Van der Klaauw, Wilbert. “Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach.” *International Economic Review*, 2002, 43(4), pp. 1249-1287.

---

[*] Black: NORC / Harris Graduate School of Public Policy Studies, 1155 E. 60th Street, Chicago, IL 60637, USA; Galdo: Department of Economics, McMaster University, KTH Building, Room 426, Hamilton, Ontario L9H2P7, Canada; Smith: Department of Economics, University of Michigan, 238 Lorch Hall, 611 Tappan Street, Ann Arbor, MI 48109, USA.

[1] The Kentucky model does not include age, race, sex or other prohibited variables.

[2] We also estimated the model with a set of covariates, but they did not improve the precision of the estimates.

[3] Footnote 10 of Black, et al. (2003) discusses the apparent disjunction between a large impact on weeks of benefits and a relatively small impact on amount of benefits. Those authors attach more weight to the impact on weeks for reasons having to do with the construction of the administrative data.

Table 1- Regression Discontinuity Estimates from KWPRS, October 1994 to June 1996

	+/- 0.05 caliper	+/-0.10 caliper	No caliper	Experimental estimates
Panel A: Parametric Estimates				
weeks receiving	-2.26**	-2.27**	-1.81**	-2.24**
benefits	(0.37)	(0.31)	(0.17)	(0.51)
amount of	-175**	-148**	92**	-143
benefits	(74)	(61)	(35)	(100)
annual earnings	636	694**	717**	1,054*
	(422)	(350)	(186)	(588)
Panel B: Wald Estimates				
weeks receiving	-2.05**	-1.90**	-0.40**	-2.24**
benefits	(0.40)	(0.28)	(0.16)	(0.51)
amount of	-10	131*	758**	-143
benefits	(82)	(52)	(33)	(100)
annual earnings	682	648**	880**	1,054*
	(445)	(312)	(181)	(588)
N (RDG / PTG)	740	929	1,107	286
N (claimant)	4,679	9,815	36,487	1,981

Notes: "**" indicates statistical significance at the 5-percent level and "*" indicates statistical significance at the 10-percent level.