

**The Reemployment Bonus Experiments:  
A Suggested Interpretation**

**Walter Nicholson  
Amherst College**

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## **I. Introduction**

The reemployment bonus experiments represent the most important examples of randomly assigned variations in individual budget constraints since the income maintenance and health insurance experiments of the 1970s. Although the results of these experiments have been extensively summarized elsewhere (Decker, 1994; Meyer, 1995), relatively little attention have been devoted to looking at these experiments from this more general perspective. In this paper I try to address this absence by viewing the results of the reemployment bonus experiments from a theoretical microeconomics perspective. That is, the paper asks specifically how did the experiments change budget constraints and how did the results observed from such changes correspond to the findings from the extensive literature on individual labor supply. I conclude that the results of the bonus experiments are broadly consistent with this literature and that they contribute to it by suggesting a number of avenues of research that might be pursued in the future.

## **II. Conceptualizing Reemployment Bonus**

Two approaches have been taken to modeling reemployment bonuses. Some authors (Woodbury and Spiegelman, 1987 and O'Leary, Spiegelman, and Kline, 1995) suggest that the offer of a reemployment bonus provides workers with an incentive to reduce their reservation wage during the period that the offer is in effect, but they do not provide a theoretical model of that process. Alternatively, Decker (1994, 1997) and Meyer (1995) draw on the earlier model of Moffitt and Nicholson (1982) to develop a labor-leisure model that stresses how the bonus offer raises the opportunity cost of leisure during the qualification period. Although both of these approaches yield similar conclusions (that the bonus offer should reduce compensated unemployment), none of these authors is particularly interested in exploring the more general

consequences of the model chosen, being instead eager to get on with reporting the empirical results of the experiments.

Although, here too, I will refrain from developing a full choice-theoretical model, I do propose what I think might be a different and possibly more informative approach. Specifically, I believe that viewing the offer of a reemployment bonus as a temporary increase<sup>1</sup> in the wage that a worker might receive can yield many insights not available from the other approaches. For example, under this approach, data from the reemployment bonus experiments can be interpreted as part the more general labor supply literature. Similarly, viewing reemployment bonuses as temporary wage increases offers insights into such seemingly unrelated questions as how individuals respond to temporary tax increases or subsidies. The approach seems a bit more natural than either of the other two approaches that have been taken in the literature. It fits better with traditional search theory in which the reservation wage represents the solution to a maximization problem given the distribution of wages by changing what is really exogenous in that model. And, by making clear the connection between the bonus and reemployment (as opposed to, say, simply ceasing to collect unemployment insurance for some other reason), it seems superior to the labor-leisure theory.

Of course, reemployment bonuses are not fully identical to wages. Although receipt of a bonus is contingent on reemployment (and all of the experiments have required about four months on the job before paying the bonus), the bonus itself has usually been a lump-sum payment, not an increment to the per period wage rate. But participants may implicitly engage in a sort of mental pro-rationing of the bonus amount over a relatively short time horizon thereby

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<sup>1</sup> The assumption that the reemployment bonus temporarily increased wage offers for those eligible by the full amount of the bonus implicitly assumes that the demand for labor is infinitely elastic at the prevailing wage – an assumption that may be reasonably valid for small bonus programs. We discuss potential general equilibrium effects of the bonus offer later in the paper.

imparting a wage-like effect. Any computation of this effect is necessarily arbitrary. But a rough estimate might be made by pro-rationing the bonus amount over the four-month re-employment required by the experiments for bonus eligibility. Using this approximation, the \$500 bonus that characterized the Illinois experiment amounts to approximately \$30 per week, or about ten percent of weekly wages earned by the comparison group (Woodbury and Spiegelman, 1987). A similar figure is suggested for the experiments in which the bonus was based on the claimant's weekly UI benefit. Bonuses in the Pennsylvania and Washington experiments varied between two and six times the claimant's WBA (Decker and O'Leary, 1994). Assuming that these states have UI wage-replacement ratios of approximately 50 percent, the bonuses would amount to between one and three extra weeks of wages over a four-month period – that is, the effective wage would be increased by between six and eighteen percent. Again, the median promised increase is probably in the ten percent range<sup>2</sup>.

The roughly ten percent wage increases offered by the bonus experiments were only temporary – participants had to choose to take a job during a relatively short “qualification period” to get them. These qualification periods were typically in the 7-11 week range. Hence, the wage changes were very temporary indeed. Some insights on the consequences for behavior of such short-term changes are provided by the literature on the effects of temporary changes in tax or transfer policy. Perhaps the most extensive theoretical framework is provided by Metcalf (1973) in his influential paper on the income maintenance experiments. There the author shows that temporary price changes have predictable differences relative to their longer-term counterparts. Specifically, relative to long-term effects, substitution effects of temporary changes are larger and income effects are smaller. The resulting differences can be quite large – short-term effects may differ from long term effects by a factor of three or more. For the case of

temporary wage changes, the prediction is unambiguous – temporary wage elasticities of supply should exceed their permanent counterparts both because substitution effects are enhanced and because income effects are attenuated. Theoretical consideration of the temporary wage increase feature of the bonus experiments therefore implies that responses should have been relatively large<sup>3</sup> and that they should have been especially large for subgroups of participants for whom substitution effects might have been expected to be the largest.

### III. Summary Results of the Experiments

To see whether this theoretical perspective contributes to interpreting results from the bonus experiments it is useful to develop some “stylized facts” about those results. I choose to highlight five such facts, although I recognize that such a summary does considerable injustice to the many details provided in the reports on the experiments:

- In all of the experiments weeks of UI collection were significantly lower in the participant group than in the control group. The bonus offer seems to have reduced UI weeks by between about 0.3 and 1.1 weeks depending on the location and the specific experimental treatment employed. There is evidence from the experiments that used more complex treatment packages that both more generous bonus offers and more extensive qualification periods tended to have larger impacts (Decker and O’Leary, 1995; O’Leary, Spiegelman, and Kline, 1995, Decker, O’Leary, and Woodbury, 2000). The Illinois experiment (which had a constant<sup>4</sup> but relatively low bonus amount) had the largest impact on total UI weeks, however.

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<sup>2</sup> This computation makes no allowance for the low (approximately 0.50) take-up rate for the bonus offer among eligibles recorded in all of the experiments. Instead, consistent with most of the analysis of the experiments, the focus here is on what was offered.

<sup>3</sup> That is, relative to long-term labor supply elasticities – which are themselves believed to be rather low.

<sup>4</sup> Bonuses in the other experiments were tied to individuals’ UI entitlements and hence were more directly proportional to workers’ wages than were the Illinois bonuses.

- Evidence that reductions in UI weeks actually represented increases in employment was mixed. Although most direct measures of employment seemed broadly consistent with the data on weeks of UI collection, the correspondence was far from perfect (Meyer, 1995). Most evaluations of the experiments tended to assume, however, that reduced weeks of UI collection were synonymous with increases in weeks of employment<sup>5</sup>.
- Take-up rates in the experiments were well below one hundred percent. Among all claimants offered a bonus, actual rates of collection averaged less than 20 percent. Even after taking account of the timing of reemployment, only about 50-60 percent of the claimants who appeared to eligible by virtue of having become reemployed during the qualification period actually completed the paperwork necessary to collect a bonus payment<sup>6</sup> (Woodbury and Spiegelman, 1987; Decker, 1994; O’Leary, Spiegelman, and Kline, 1995)
- According to some authors, a claimant’s recall status appeared to play an important role in experimental outcomes. For example, Anderson (1992) concludes that bonuses had no effect on those expecting recall and Rodriguez-Planas (2000) argues that this “puzzle” can be resolved by considering the effects of asymmetric information on claimants’ job search outcomes.
- Evidence of how various claimant subgroups responded to the bonus offer was ambiguous. In most cases differences among participant subgroups were not found to be statistically significant. However, some of the experiments did report some important possible differences by sex. I review the evidence on subgroups later in the paper.

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<sup>5</sup> For example, several authors point out that the (large) confidence intervals on the estimated effects of the bonus offer on earnings contain values that would be consistent with an employment interpretation (see for example, Decker and O’Leary, 1995, page 545).

## V. Labor Supply Interpretation

The most basic results of the experiments are broadly consistent with a generalized labor supply interpretation. Two strands of literature are helpful in making this point. First, with regard to the results based on weeks of UI collection, the empirical findings are quite close to the consensus estimates for the disincentive effects of UI benefits. For example, Decker (1997, page 296) concludes that "... the lengthening in average unemployment spells is likely to be in the range of 0.5 to 1.5 weeks for every 10 percentage-point increase in replacement rates". Since a temporary 10 percent increase in wages translates into approximately a 5 percentage point decline in the wage-replacement ratio (assuming that the ratio is in the 0.5 range), a decline in weeks of UI collection of between 0.25 and 0.75 might have been expected – a range remarkably close to what was actually observed.

Three factors make it more difficult to compare the experimental results to the more general literature on labor supply: (1) The experimental results for time employed are much more ambiguous than are the results for weeks of UI; (2) The time frame for calculating increases in labor supply is not obvious; and (3) Responses to temporary wage changes have seldom been studied in the more general literature. Still, with some heroic assumptions, certain similarities are apparent. For example, if the time frame is taken to be the four-month period of employment required for bonus receipt and if weeks employed are assumed to have increased by, say, 0.5 weeks for those eligible for a bonus, then the implied (uncompensated) wage elasticity of supply can be calculated<sup>7</sup> as about +0.29. This figure is considerably higher than the uncompensated elasticity estimates for males summarized by Pencavel (1986), but tends to fall in the middle of the (very broad-ranged) estimates for women quoted in Killingsworth and Heckman (1986). Given the expectation that

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<sup>6</sup> It should be pointed out that this take-up rate is not out of line with take-up rates for other social programs,

uncompensated wage elasticities should be larger with temporary wage increases relative to estimates made for more permanent ones, this implied supply elasticity estimate seems quite reasonable.

If the labor-supply interpretation of the basic results from the UI bonus experiments is accepted, many of the other results from the experiments fall into place. For example, perhaps the most puzzling comparative result from the experiments is the much larger impact in Illinois despite the relatively small bonus offered in that state. A variety of explanations have been offered for this anomaly including such possibilities as weaker labor markets in Illinois (Meyer, 1995), existence of extended benefits in Illinois (Davidson and Woodbury, 1993), or the sharp discontinuity that the Illinois bonus introduced into the budget constraints of claimants (Decker, 1994). Although all of these explanations may have some validity, a far simpler explanation is that the Illinois sample had a greater representation of low-wage, younger workers for whom the *fixed* \$500 bonus amount represented a large proportional increase in the wage. Two pieces of evidence are consistent with this conjecture. First, the fraction of workers in the Illinois sample who were younger than 35 (Decker, 1994, reports this number as 62 percent) appears to have exceeded by a substantial margin the fraction in the other bonus experiments<sup>8</sup>. Similarly, average base period earnings appear to have been much lower in the Illinois sample<sup>9</sup>, though there is no published combined data set for the four experiments with which to make a formal comparison. It is the fixed nature of the Illinois bonus that yields its large proportional effect on low-wage workers. This effect would have been attenuated in the other experiments because, in those, the bonus amount

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especially those of short duration.

<sup>7</sup> The increase in labor supply is 2.9% (0.5weeks/17weeks) in response to a temporary 10% increase in the wage.

<sup>8</sup> Although it is difficult to get comparable samples, my quick perusal of published information suggests that the fraction of workers less than 35 were 30 percent in New Jersey, 49 percent in Pennsylvania, and 41 percent in Washington.

<sup>9</sup> Decker, 1994, reports that average base period earnings in Illinois were \$12,800 versus \$18,000 in New Jersey.

was more nearly proportional to the claimant's actual wage. I have not been able to find any sub-sample results for the experimental evaluations that focus specifically on low-wage workers<sup>10</sup> nor, apparently, did any of the evaluations utilize random coefficients models in their statistical work. Hence, although I report a few simple supportive regressions in the Appendix to this paper, the possibility that the Illinois results might be explained by greater heterogeneity in responses induced by its fixed bonus structure must remain only a conjecture.

The relationship between effects of the reemployment bonuses and the recall expectations of claimants has also been subject to a variety of interpretations. In general, this relationship is a complex one. All of the experiments specified that claimants recalled to their pre-UI jobs would not be eligible for a bonus on the basis of that job although in some cases recalls following other employment were acceptable<sup>11</sup>. Some of the experiments<sup>12</sup> also specified that workers with definite recall dates were not eligible for participation. Still, many of the remaining participants in the bonus experiments expected to be recalled to their prior employment<sup>13</sup>.

For workers expecting recall the labor-supply interpretation of the experiments makes a relatively robust prediction that the offer of a bonus should increase new job taking and reduce recalls. Because recalled workers could not generally collect a bonus, the bonus offer (temporarily) tilts relative wages toward taking a new job. The size of this effect is determined by the worker's subjective probability of recall – which may change over his or

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<sup>10</sup> The Pennsylvania experiment did find that younger workers had larger effects from the bonus offer than did older workers, though this pattern was not found in New Jersey.

<sup>11</sup> The situation in Washington was perhaps the most complex. Whereas the other experiments defined a "recall" as a return to the pre-UI employer, Washington defined it as a return to the pre-UI *job*. Workers in Washington who returned to their prior employer but at a "different job" were eligible for a bonus.

<sup>12</sup> For example, the Pennsylvania experiment barred participation by workers with a definite recall date within 60 days of their UI application (Corson and Decker, 1992)

her unemployment spell. If subjective probabilities of recall decline over time, the impact of the bonus on new job taking should increase, though the bonus qualification period may be too short to detect such changes in expectations. Whether those who expect recall should respond to the bonus offer more significantly or less significantly than those who do not expect recall is a complex question which would seem to depend on a variety of factors that may change during the unemployment spell.

The ambiguities of the existing analyses of the experimental data shed relatively little light on these theoretical predictions. The New Jersey experiment's final report (Corson and Decker, 1989) provides subgroup estimates that suggest that those expecting recall had much larger reductions in weeks of UI than did those not expecting recall<sup>14</sup>. The Pennsylvania results (which may have been affected by the experiment's eligibility screens) suggest precisely the opposite<sup>15</sup>. Patterns reported in the widely-quoted paper by Anderson (1992) are similarly perplexing. In this paper the author used a large number of bonus-expected recall interaction terms in a competing risks model in which the outcomes of interest were taking a new job or returning to the pre-UI employer. None of the interactions was statistically significant in the recall equation. The interactions were also not statistically significant in the "New Job" equation during the bonus period although the author does note an anomalous significant negative pre-bonus-period interaction<sup>16</sup>. The author reads this as

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<sup>13</sup> In New Jersey the percent of participants expecting recall was 40 percent. Because of the eligibility rules in Pennsylvania this fraction was much lower (11 percent). The other experiments appear to have values between these two extremes.

<sup>14</sup> Table V.4, for example reports that the impact of the bonus (plus job search assistance) on weeks of UI was -1.48 weeks for those expecting recall versus -0.37 weeks for those not expecting recall.

<sup>15</sup> Table VII.6 reports that the bonus offer reduced UI weeks by -0.41 weeks for those expecting recall versus -0.84 weeks for those not expecting recall.

<sup>16</sup> One explanation given for the result is that it simply a failure of random assignment with a "disproportionate number of those receiving it [the bonus offer] earlier than week 7 did not expect recall". A more behavioral-based explanation might be that those for whom the new job/recall wage ratio was close to wait for the bonus to shift that ratio toward taking a new job.

supporting the hypothesis that “the bonus offer by itself had no effect on those expecting recall” though this reading does not seem completely consistent with the reported results.

Overall then I conclude that it is impossible to say exactly how the bonus offer affected workers who expected to be recalled to their prior employment. There are reasons to expect that the offer might have encouraged them to consider new jobs more actively or to adopt more realistic recall expectations. Such an impact would have both desirable and undesirable social consequences. But a definitive study of the size and direction of such effects remains to be done.

With regard to subgroup estimates from the experiments, I believe that one of the most important findings has been widely overlooked – that women responded much more significantly to the bonus offer than did men. Table 1 records the results by sex that I have been able to find easily<sup>17</sup>. Clearly, in all of these cases, the response of women to the bonus offer was much greater than were those of men, though none of the reports provide an explicit test of the statistical significance of the difference.

Table 1 : Reported Effects of Bonus Offer on Weeks of UI, by Sex, in Three Experiments

	Experiment			
	New Jersey	Pennsylvania	Illinois - White	Illinois - Black
Male Effect	-0.47	-0.32	-1.10	-0.52
Female Effect	-1.09	-0.82	-1.60	-0.98

These differences by sex are fully consistent with the labor supply interpretation developed above. Because women are believed to have greater labor supply elasticities than

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<sup>17</sup> I omit the results from Washington here. These results tended to show that men responded more significantly than women to the bonus offer. But these may have been affected in indeterminate ways by the complex situation with regard to recalls in Washington.

men and because substitution effects are expected to be exaggerated by the short duration of the bonus offer, responses by women should have been greater than those for men. In this regard the results of the experiments provide relatively convincing support for the findings from the broader labor supply literature. The relatively narrow focus on the experiments on measuring *the* impact of the bonus offer may have obscured this interesting result.

## **VI. Lessons for Evaluation Design and Analysis**

These observations suggest a general lesson for the design of social experiments. Early experiments, such as the New Jersey Income Maintenance Experiment or the Rand Health Insurance Experiment tended to be designed with structural behavioral models in mind. Experimental treatments were then designed so as to provide measures of some of the parameters in these models. This approach made it possible, in some cases, to extrapolate beyond the treatments actually tested and, more generally, to learn something about the determinants of economic behavior. A problem with the structural modeling approach, however, was that the results often could not be interpreted as simple experimental-control differences and could therefore not be easily explained to policy-makers and the general public.

Beginning with the Supported Work evaluations and the many work-welfare initiatives the focus of experimental designs began to move away from structural modeling and toward simpler treatment-comparison approaches. This yielded experimental results that were both easier to understand and less reliant on pre-specified behavioral models. But a major disadvantage of this is, as Heckman et al. (1999, page 2084) report, “samples generated under the new model for social experiments produce evidence that does not accumulate in the same way as evidence accumulated under the old model...”. That is, the evidence from these new “black box” experiments cannot readily be compared across evaluations with differing

treatments, nor can the evidence be compared to the more general econometric literature.

Hence, the interplay between behavioral models and econometric techniques that has been so important to the development of quantitative labor economics during the past thirty years is thwarted.

With a few notable exceptions<sup>18</sup> researchers have treated the bonus experiments in the black box manner. There are few explicit theoretical models of how bonuses are likely to affect claimant behavior (Anderson, 1992 provides a search-based model whereas Decker, 1994 offers a labor-supply-type model) and the empirical results from the experiments have not generally been used to provide parameters to those models that do exist. Hence, it is difficult to extrapolate results from the bonus experiments to other contexts. I believe that taking a more structural approach to the bonus issue would have benefited the research in at least five ways:

1. Such an approach would have provided better guidance about the relevant parameters to be varied in the experiments. For example, a focus on how bonuses temporarily affect the wage would have suggested a focus on purposely varying this wage rather than (as in three of the experiments) adopting a bonus that was nearly proportional to claimants' wages;
2. A structural approach may have clarified issues about the proper eligibility rules both for participation and for receipt of the bonus. The current confused situation with regard to layoff expectations might have been

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<sup>18</sup> For example, O'Leary, Spiegelman, and Kline (1995) compute a "price" effect and a "duration" effect from the results of the Washington experiment. They find that both effects are statistically significant on some outcomes though the price effects are larger. This is consistent with the labor supply interpretation of the experiments presented in this paper. However, the authors make no attempt to link the elasticities implied by their estimates to those from the more general literature. The Appendix to this paper reports my own attempts to make a few such simple estimates using data from the Pennsylvania experiment. These results, though far from definitive, are generally consistent with the labor supply interpretation provided in the body of this paper.

clarified by additional theoretical research on the bonus/recall connection, for example;

3. A structural approach would have given better guidance about what subgroup analyses might have been most interesting. The virtual absence of research on the major male-female differences in the experiments is perhaps the best illustration of this possibility;
4. More generally, a structural approach may have provided better suggestions of where to turn next in looking for policies to speed claimants' return to work. The finding that, in general, bonuses do not appear to pass a cost-benefit test from the perspective of the UI system nor from a social perspective has caused policy interest in such schemes to fade from the horizon with few obvious replacements. Perhaps a more structural approach would have helped to identify policy levers that might be more effective in speeding the return to work.
5. Finally, a focus on structural parameters in individual labor supply decisions would make easier the development of general equilibrium models for evaluating bonus schemes. To date such models have tended to have a partial equilibrium character (for example, Davidson and Woodbury, 1993) in which relatively stringent assumptions are made about the demand side of the labor market. Incorporation of bonuses into such a model proceeds by examining how eligibility affects workers' entry into the job queue. Relatively significant displacement effects are implicitly assumed from the start in such a model. An alternative, general equilibrium approach would likely show less displacement because of

demand effects generated by bonus-induced changes in wage rates.

Incorporation of bonuses into a standard CGE model would, however, require greater attention to the underlying behavioral labor supply parameters.

Overall then, the UI bonus experiments have been characterized by a careful attention to experimental implementation and have yielded a large number of interesting results. Further work on the underlying theoretical structure of the experiments might also be expected to be similarly productive in yielding information relevant to the development of UI policy.

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## Appendix

## Some Illustrative Estimates

This appendix presents a few illustrations of how the data from the reemployment bonus experiments might be used to estimate conventional labor supply elasticities. As in the body of the paper, making such estimates requires a number of heroic assumptions. Hence, there is no implication that any of these estimates are in any way definitive.

The basic data from which labor supply elasticities might be derived is, of course, quite sparse consisting mainly of data related to unemployment insurance claims together with a few demographics. To derive labor supply elasticities therefore I made the following three key assumptions:

- That any experimental impact on weeks of UI collected in fact represents weeks of additional employment;
- That claimants use a four-month time horizon in evaluating the wages that a bonus-eligible job promises;
- That expected wages upon reemployment can be inferred from the individual's UI weekly benefit amount by assuming a 50 percent wage-replacement rate for workers not at the state maximum.

Given these assumptions, the expected proportionate increase in wage promised by the offer of a reemployment bonus is given by:

$$\text{DeltaWage} = \text{Bonus Amount}/34\text{WBA}^{19}$$

And the implied labor supply elasticity is:

$$\text{Elasticity} = -([\text{Change in weeks of UI}]/17)/\text{DeltaWage}.$$

Table A1 reports the results of such a calculation for data from the Pennsylvania Reemployment Bonus Experiment<sup>20</sup>. All estimates are based on simple OLS regressions for claimants with weekly benefit amounts less than the Pennsylvania maximum. When estimated over the entire sample, the estimated labor supply elasticity of 0.22 is considerably higher than reported in most of the literature, but this may be only the result of the way I have chosen to measure it. Still, the results offer convincing evidence that this approach may provide some useful insights.

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<sup>19</sup> The coefficient “34” here is used to approximate the wage paid on a job paying twice the WBA over a 17 week period.

<sup>20</sup> These data were kindly provided by Paul Decker.

Table A1: Implicit Labor Supply Elasticity Estimates based on Pennsylvania Experiment

	Whole Sample	No Recall Expectation		Recall Expectation	
		Male	Female	Male	Female
Estimated Elasticity	+0.22***	+0.25**	+0.22*	+0.22	-0.07
Sample Size	11017	5196	4576	664	581

\*\*\* Elasticity significantly different from zero at .01 level

\*\* Elasticity significantly different from zero at .05 level

\* Elasticity significantly different from zero at .10 level

All estimates based on all experimental participants with weekly benefit amounts less than \$250. Estimates made using OLS regressions that controlled for Race/ethnicity, Age, and Potential UI Duration.

Disaggregation of the Pennsylvania estimates by sex and by initial expectations of recall yielded somewhat paradoxical results. For workers with no recall expectations estimated supply elasticities were virtually identical for men and women. This finding is somewhat in conflict with the reported data for the Pennsylvania experiment that suggest that the response of women exceeded that for men by a factor of more than two. Perhaps the use of the WBA to approximate expected reemployment wages is a better estimate for men than for women (so that the female elasticity is underestimated), but I have no direct evidence to support that hypothesis.

For workers expecting recall the male/female differences are quite startling. For men, the implied supply elasticity is about the same as it is for those not expecting recall (though the coefficient is not significantly different from zero in this case). This implies that the bonus offer probably had a substantial effect of luring those expecting recall to take different jobs. For women expecting recall, however, the estimated effect of the bonus offer is essentially zero. In this case there was no substitution of new jobs for old ones. I have no convincing explanation for this observed difference by sex, but it does reinforce the need to sort out the precise connection between recall expectations and bonus effects highlighted in the body of the paper.

Table A2 reports a similar set of estimates based on data from the Illinois experiment<sup>21</sup>. Because this experiment did not gather data on recall expectations it was not possible to provide the sort of disaggregation provided in the previous table. Still, two findings stand out in these simple estimates. First, the estimated elasticities are all much higher in the Illinois than in the Pennsylvania sample. Hence, the very favorable results of the Illinois experiment arise not only because the experiment promised larger wage changes to low-wage workers<sup>22</sup>. Rather, the responses seem more elastic on an across the board basis. Second, the female elasticities in

<sup>21</sup> These data were kindly provided by Stephen Woodbury.

<sup>22</sup> The data do show, however, that implicit wage changes were, on average, 2-3 percent higher for workers less than 25 years old than for other workers. Assuming that the UI weekly benefit amount may offer a higher wage-replacement fraction for this group suggests that the actual proportional wage increment provided by the experiment may have been even larger. Some supporting evidence is provided by separate estimates of supply elasticities for workers less than 25 years old which show that these are nearly twice as large as those for older workers.

Illinois seem to be somewhat larger than the male elasticities – a finding more consistent with theoretical expectations than those reported in table A1.

Table A2: Implicit Labor Supply Elasticity Estimates based on Illinois Experiment

	Whole Sample	Males Only	Females Only
Estimated elasticity	+0.42***	+0.36***	+0.48***
Sample Size	7944	3749	4195

\*\*\* Elasticity significantly different from zero at .01 level

\*\* Elasticity significantly different from zero at .05 level

\* Elasticity significantly different from zero at .10 level

All estimates based on all experimental participants with weekly benefit amounts less than \$160. Estimates made using OLS regressions that controlled for Race/ethnicity and Age.

Of course, all of these econometric estimates are made using very simple specifications and none allows for heterogeneity in worker responses. The results do suggest, however, that focusing on behavioral parameters may make it somewhat easier to compare results across experiments.