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**Paying for the Relocation of Welfare Recipients:
Evidence from the Kentucky Relocation Assistance Program**

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Abstract

In May of 1998, the Relocation Assistance Program (RAP) was introduced in Kentucky as a means of aiding welfare recipients to achieve self-sufficiency by offering lump-sum payments to those who wished to relocate to seek or accept employment. Unlike other relocation assistance programs, this program provides moving assistance to welfare clients rather than to unemployed persons or dislocated workers. We relate this program to other relocation programs as well as to the UI bonus experiments. We also survey the theoretical literature to give some intuition for the effects of the program on earnings and employment for welfare clients. Using program participation to measure the treatment effect is a contentious issue due to program requirements linking employment to participation. Given that advertising would certainly influence participation, we construct an advertising proxy that differs from program uptake/utilization to identify the program's effects. Working with a relatively short, panel administrative dataset, we find that a one standard deviation increase in the RAP proxy is associated with a 20.4 percent increase in employment and a 18.3 percent increase in quarterly unconditional earnings, which is robust to various specification checks.

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1. Introduction

There are a variety of reasons why many welfare recipients do not work, including child care constraints, lack of transportation, low motivation and low wages. On the other hand, it is possible that a significant share do not have employment simply because severe liquidity constraints prevent them from moving to take a job. As an example, Ardington et al. (2007) illustrate how relaxing liquidity constraints can provide earnings-enhancing mobility. In their study of the South African pension system, they find evidence that increases in household resources in the form of government transfers are associated with a rise in labor migration. If liquidity constraints are indeed a deciding factor, the labor force participation of welfare clients will respond to programs that subsidize geographic mobility. The goal of this paper is to see if this is the case.

The More Opportunities for Viable Employment (MOVE) program in Tulare County, California, and the Relocation Assistance Program (RAP) in Kentucky in 1998 are the only two programs in the nation, to our knowledge, that offer payments to welfare clients who wish to relocate to seek or accept employment.² The programs were implemented as a means of aiding welfare recipients to achieve self-sufficiency, a primary objective of any poverty policy. In other words, these programs are essentially targeting individuals on the margin of leaving welfare who would otherwise not do so were it not for the payment of relocation expenses. Perhaps a more illustrative way to consider this is to think about a fixed cost to accepting a job offer.³ The case

² Tulare County also later contracted out its services to Kings, Fresno and Madera County; however, due to severe budget cuts, all three counties have since elected to discontinue the program. Budget cuts in Tulare County have been such as to allow the county to maintain its program, operating on a budget roughly one fourth the size of that five years ago (Telephone interview with Karen Davidson, MOVE Coordinator). As the dataset for the MOVE program was very much incomplete and insufficient for empirical analysis, the focus of this research will be on Kentucky's Relocation Assistance Program.

³ In a similar vein, Charness and Gneezy (2006) acknowledge that there may be such an obstacle in going to the gym. They conduct an experiment wherein monetary payments are granted as an incentive to go to the gym and conclude that such incentives are successful in surmounting this initial fixed cost.

for the expansion of such relocation programs is contingent upon whether they are successful in moving clients off the welfare rolls and into employment at a reasonable cost.

A growing need for such relocation programs, if they are indeed successful, is highlighted in Kaestner et al. (2001). They find that welfare reform has altered the reasons for relocating among welfare recipients. Contrary to seeking higher welfare benefits (the welfare magnet hypothesis), an increasing number of clients are moving for employment. They offer the following: “This suggests that states should adopt welfare policies that facilitate changes in residential location such as transitional housing benefits and relocation expenses” (p.20).

These programs are by no means immune to criticism. Indeed, many observers point out that they are merely a temporary fix, or large bandage. They make no attempt to address the root of the problem, which very plausibly involves a host of issues including minimal human capital skills. Additionally, some worry about potential racial and socioeconomic overtones inherent in such programs.

On the other hand, lower stocks of human capital may be only one facet. In their study of earnings inequality in Appalachia, Ziliak, Troske and Bollinger (2007) pose the question as to whether it is lagging levels of educational and skill attainment or lower returns on human capital that explain lingering poverty in this region. The first is a labor supply issue whereas the second is a labor demand problem and would build the case for RAP like programs. The authors conclude that the skills gap has narrowed over time thus attributing persistent wage inequality in Appalachia relative to the rest of the nation as more due to differences in skill returns rather than differences in skill levels. Nonetheless, they point out that a prominent skills gap still exists at the upper end of the wage distribution.

The remainder of this paper is organized as follows. Section II reviews previous work. Section III describes the Kentucky Relocation Assistance Program (RAP), including eligibility criteria. Section IV outlines some related theory to give some intuition for expected effects of the program on the labor market outcomes of welfare clients. We empirically test the predictions of the theory using a rich, panel, administrative dataset described in Section V. The empirical framework is briefly outlined in Section VI. Results are presented in Section VII with robustness checks in Section VIII. Implications for public policy are offered in the remaining section.

II. Background Literature

A. Relocation Assistance Programs

Although relocation programs in themselves are not new, the emergence of programs targeting welfare recipients is a recent phenomenon. In many ways, welfare relocation programs are similar to subsidized housing programs whereby impoverished families are given vouchers to facilitate a move into a more affluent neighborhood (Katz et. al 2001) or to relocation programs that aid dislocated workers or the unemployed. LeGrande (1983) examined early pilot projects designed to assist dislocated workers. In particular, she noted that blue-collar workers were less willing to relocate than their white-collar counterparts. The same finding holds for older workers. With no control group, little available data and evaluations based on retrospective interviews, the overall effectiveness of the Manpower Development and Training Act, 1965-1972 pilot programs remains unclear. At best, the re-employment effects of the Job Search and Relocation Assistance (JSRA) pilot project 1976-1980 can be rated as a statistically insignificant positive, given the selection bias implicit in the less mobile groups and the ambiguously defined benefit to cost ratios.

Horst (2001) found that 85% of participants in Tulare County's MOVE program were employed six months after relocating. However, this statistic sheds little light on the effects of the program because Tulare's eligibility requirements *actually dictate* that a confirmed job offer is needed if relocating within the state of California.⁴

A preliminary study of the Kentucky RAP program (Barber, 2000, 2002) spells out large returns to employment and earnings after RAP participation (average quarterly earnings reportedly increased from \$1,779 to \$3,422). Unfortunately, it does not appear that this estimate accounts for the basic self-selection problem, nor does it control for demographic characteristics.

B. Reemployment Bonus Experiments

In an effort to alleviate some of the distortionary effects of the UI program, a series of experimental programs were carried out in various states wherein unemployed persons were granted a lump-sum cash benefit if they found a job within a specified length of time. Contrary to the predictions of search theory, these individuals did not seek out lower quality jobs (Borjas, 2005). In the Illinois experiments, UI claimants were randomly assigned to treatment and control groups. Individuals were awarded \$500 for finding a job 11 weeks or less after filing for UI. The program did not diminish reemployment earnings implying that agents chose to focus more on increasing their search intensity rather than reducing their reservation wage in an effort to secure employment more quickly (Meyer, 1996). Meyer (1995) also raises the possibility of a general equilibrium effect, if adopted on a large scale, whereby the program gives individuals an incentive to apply for UI who would otherwise not do so were it not for the bonus, thereby increasing the UI rolls and potentially offsetting or reversing any benefits of the program.

⁴ We contacted the author to inquire as to how he had arrived at these findings. He replied that the Tulare County administering agency reported them.

In contrast to the optimistic findings of the Illinois Reemployment bonus (Woodbury & Spiegelman, 1987), Decker and O’Leary (1995) found that the follow-up experiments in Pennsylvania and Washington yielded much smaller effects on wages and duration. Coupled with administrative costs and the actual bonus payments themselves, the authors cast doubt on the cost-effectiveness of reemployment bonus programs. Indeed, Meyer (1995) concludes in his review of the experiments, “While the initial reaction of policymakers and academics to the bonus experiment idea was very positive, we believe the initial optimism is fading” (p.110)

C. Earnings Subsidy Experiments for Welfare Clients

Perhaps the most comparable study to the research question posed here is the assessment of the Self Sufficiency Project (SSP) undertaken by Card and Hyslop (2005). In a randomized experiment, welfare recipients were assigned to one of two groups. In the experimental group, participants received a time-limited earnings subsidy for working full-time if they found a job within one year of assignment. Although the short-run effects of the program appeared promising, they were not robust to time and in the long-run the authors could find no discernible effects of the program on either earnings or welfare enrollment. Interestingly, the authors find that some individuals in the treatment group had a greater propensity to return to welfare 15-18 months after program assignment than those in the control group. They take this as evidence of a rather nuanced program incentive to take a low paying job when the eligibility period is nearly up and then soon thereafter quit their position and return to the welfare rolls. Furthermore, unlike the program under study here, participants in the SSP program were required to quit the welfare rolls. In the end, the authors cast doubt on the lasting effectiveness of temporary wage subsidies.

III. Overview of the Kentucky Relocation Assistance Program (RAP)

The Kentucky Relocation Assistance Program (RAP) was introduced in May 1998 as a means of paving the way towards self-sufficiency. RAP is awarded to households, not individuals. Welfare recipients may qualify for up to \$900 in relocation expenses to accept a verified job offer or secure employment obtained within 90 days of the date of request that is ten or more miles from the individual's current place of residence and is at least 30 hours per week at the minimum wage. The minimum wage in Kentucky is equal to the Federal minimum wage at \$5.85 per hour.⁵ Hence, this would work out to a monthly gross wage of \$737.10. Program funds may also be used to obtain more affordable housing due to a rent increase due to gaining employment or to escape a domestic violence situation. For victims of domestic violence, there is no limit for receipt and eligibility is not contingent on employment.⁶ On the other hand, those who seek relocation assistance for employment purposes are limited to two disbursements within a five year period. After January 1st, 2003, this rule changed and households are limited to only one RAP payment.

Kentucky welfare recipients learn about the RAP program as well as all available supportive services via their case management specialist. To apply, the client requests RAP from their caseworker. Applicants have ten days to complete the application and all corresponding documentation. Upon completion of verification, payment is issued. Follow-up case management to assist with the transition is also provided. To qualify for RAP, the household must currently be receiving welfare and not be sanctioned. Applicants who moved prior to requesting RAP are denied. Minors are ineligible to use benefits to move out of their parents' home. Caseworkers are instructed to attempt to recover the RAP funds if the payment was issued in error, the

⁵ <http://www.dol.gov/esa/minwage/america.htm#Kentucky>

household decides not to move or makes inappropriate use of funds or if the employment offer is rescinded (Commonwealth of Kentucky, CHFS, Operation Manual, Service Screening & RAP-Relocation Assistance Program 2007). Interestingly, there is an alternative program for households not receiving welfare, but who meet maximum income requirements for welfare eligibility. The Family Alternatives Diversion (FAD) Plan offers such assistance with moving expenses.⁷

A salient feature of this program is that receipt of RAP does not automatically result in being dropped from the welfare rolls. Whether clients receive RAP to accept or maintain an offer of employment, does not necessarily preclude them from continuing to receive public assistance. That is, it may be possible that even given the employment requirements for RAP (30 hours a week at the minimum wage), they may still have financial eligibility for welfare depending on family size. Additionally, individuals may qualify for a two month work exemption. In this special case, clients continue to receive additional months of welfare benefits despite the lack of financial eligibility. There are four such cases: the recipient is a child under the age of 16; a child 16-19; a teen parent with a child under 12 weeks of age; or a single custodial parent with a child under 1 year of age.

IV. Expected Effects

To illuminate and help interpret the empirical results, some related theoretical models will be presented to explore the potential effects of relocation assistance on the labor market outcomes of welfare recipients. As discussed earlier, program requirements dictate that one must

⁶ While we cannot precisely identify domestic violence victims in the data, estimates are that these clients comprise 5% of all RAP cases. Hence, we would not expect such individuals to greatly confound our estimates.

⁷ Commonwealth of Kentucky, Service Screening, FAD-Family Alternatives Diversion Plan. <http://kycares.ky.gov/ServiceScreening/Resolution.asp?pid=1369>. Accessed 7/9/2007.

already be working or have a confirmed job offer to be eligible for benefits. In this sense, the one-time payment is not a search subsidy, as it is disbursed *ex post* or retroactively in the job search framework. Again, it is relevant to keep in mind that welfare recipients are made aware of this program upon initially meeting with their caseworker. Furthermore, it should be noted that the wage data used in this study are not net of moving costs. The program will be reviewed first within the search framework and then within the static labor leisure model.

A. The Search Framework

That UI programs increase unemployment duration is a standard prediction in the search literature. Contingent on the particular search framework, the effect of UI on post-unemployment wages varies (Classen, 1979). Mortensen (1987) adapts the search model for the reemployment bonus experiments holding constant search intensity. This translates into a diminished reservation wage and shorter unemployment spells *during the qualification period* (Meyer, 1995). RAP, however, differs from a reemployment bonus since there is no time limit attached to finding a job so as to receive the moving bonus (thus, the probability of getting the bonus does not decline over time). As a result, in contrast to the reemployment bonus, the agent does not necessarily have an incentive to find a job faster. The RAP bonus simply adds a constant to the reemployment wage, *whenever* a new job is found. This latter point should be emphasized in that a time-limited eligibility period was a key component of both the UI bonus experiments as well as the earnings subsidy experiment discussed in an earlier section of this paper.

The above analysis assumes the absence of credit constraints. Without access to credit markets, agents are limited in their time spent searching for jobs. Subsequently, reservation wages are lower and unemployment spells are shorter in the presence of borrowing constraints than in their absence. Basic search theory shows that an agent can take two actions to find a job

faster. One, he can reduce his reservation wage thereby increasing the probability of finding a job acceptable. And two, he can increase his search intensity which accelerates the arrival rate of potential offers. In his review of the unemployment bonus experiments, Meyer (1995) finds non-negative effects on reemployment wages, suggesting that agents prefer to focus more on increasing their search intensity rather than reducing their reservation wage in an effort to secure employment more quickly. If we think of RAP as relaxing these liquidity constraints, it should therefore raise reservation wages and the average accepted wage.

Another and perhaps better means of framing the effects of RAP in the search model is to equate RAP to a (mean-increasing) rightward shift in the wage offer distribution, which unambiguously leads to a rise in the reservation wage. Without RAP, the net wage (after moving costs) of a subset of the job offers received is low because they are located some distance away. RAP would raise the net wage from that subset of jobs thereby shifting the distribution of offered wages to the right. The implications for unemployment durations and wages are derived in Burdett and Ondrich (1985). Assuming a logconcave functional form for wage offers, they predict a decline in unemployment duration coupled with higher post-unemployment (accepted) wages. There is, however, a caveat with testing the latter in that, as mentioned earlier, the wages in the dataset used here are not net of moving costs. Thus, in the Burdett-Ondrich framework, RAP does not have an unambiguous predicted effect on the measure of accepted wages that is available in this data set.

B. Implications from the Static Labor Leisure Model

An alternative to the search framework is the Moffitt and Nicholson (1982) static labor leisure framework, which has also been adapted to the reemployment bonus experiments. As

noted, the RAP program may bear a greater resemblance to reemployment bonus programs than UI. Furthermore, unlike UI, the moving bonus does not reduce the cost of being unemployed.

Chetty (2005) is able to separate income and substitution effects by noting that whereas UI engenders both effects, severance pay only yields an income effect. Furthermore, in his model, the income effect is only present for agents facing credit constraints since in the life cycle model UI is only a small fraction of lifetime income for wealthier agents. In this respect, RAP may be more similar to a severance payment in that it is a one-time payment and prolonging one's job search does not generate additional relocation funds. That is, RAP does not harbor any relative price distortion. On the other hand, it does change the relative rewards to taking a job further away as opposed to close by. This may possibly increase the number of new jobs more than 10 miles from home. Since the moving bonus is paid as a lump-sum following the acceptance of a job offer, it produces only an income effect. Furthermore, unlike the Mortensen (1986) model, there is no uncertainty with respect to finding a job or the new wage (Meyer, 1990).

In sum, the implications of theory for empirical work give rise to three possible scenarios. First, the basic search model predicts that the effect of the moving bonus will be to move both the reservation wage and unemployment duration in the same direction. Since there is no time limit tied to the bonus and the bonus in itself should relax any credit constraints agents may have which force them to become limited in their search, we would expect that RAP, if it has any effect at all, would raise wages and lengthen unemployment duration. A second way to model RAP is as a rightward shift of the wage offer distribution, resulting in a rise in post-unemployment wages (inclusive of the bonus) and a decline in unemployment duration. The third modelling approach is the static labor leisure model, which predicts that the moving bonus

will engender a pure income effect resulting in an unambiguous increase in length of unemployment. In the presence of liquidity constraints, this income effect may also help relax such constraints, thereby providing earnings-enhancing mobility, such as in the case of Ardington et al. (2007).

V. Data

Formally, the sample for this analysis comprises anyone who initiated a welfare spell in Kentucky between quarter 1 of 1996 and quarter 2 of 2004. Three separate administrative data files were made available under approval of the Kentucky Cabinet for Health and Family Services (CHFS). The Kentucky Automated Management and Eligibility System (KAMES) file contains demographic and case file information on all Kentucky welfare clients. The Department of Employment Services (DES) file contains earnings and employment data reported by employers to the state for UI purposes. Unfortunately, DES does not record such data for the self-employed or for those who move out of state. Lastly, the Relocation Assistance Program (RAP) file identifies all RAP clients and their corresponding relocation details. These three files were merged to create a rich panel of observables that also allows one to follow clients' welfare spells, earnings and employment over time.

Starting with the RAP file, which contains records of all clients from May 1998 through May 2004, we appended a 25 percent random sample drawn from the KAMES file (non-RAP welfare group), which is available on a monthly basis and is from January 1996 through May 2004. Finally, we merge in all available DES quarterly earnings data over a somewhat longer period (first quarter of 1995 through the second quarter of 2004) for the individuals in both these samples.

In the raw RAP file, there are 4463 recorded moves. Of these, 471 contain missing relocation details and are dropped. Of the remaining 3,992 records, 2,684 are moves within the same county, 620 are to another county in Kentucky and 688 are to another state (the most popular being Ohio). Figure 1 illustrates the number of inter county moves, whereas Figure 2 depicts net intra county moves. Inspection of the latter reveals that counties experiencing net negative moves are concentrated in the Appalachia region and more generally in more impoverished areas (Figure 3 maps per capita income by county).

If no earnings are reported to DES that quarter, the individual is tallied as not being employed in that quarter. It is possible for an individual to be both employed and still on the welfare rolls as they may still meet the financial eligibility requirements for public assistance. As has been stated in an earlier section of the paper, the self-employed and those who moved out of state are excluded as DES does not retain records for such individuals. The analysis excludes observations with reported real earnings exceeding 10,000 dollars per quarter as well as those with missing SIC codes. Additionally, only clients between the ages of 18 and 65 are included in the sample. Since occupational codes are only non-missing when the individual is currently employed that quarter, occupational codes are endogenous. Instead, we use unconditional earnings in the four quarters prior to receipt of welfare as controls. Summary statistics for basic covariates used in the analysis are presented in Table 1. The “before” period refers to data prior to May 1998. Since the most aggregate level of observation is a person-quarter, the various datasets are merged so that a cell is a person by quarter, thereby representing a total of 31,248 individuals or 662,443 observations in this panel. Since subsidized housing is reported monthly, not quarterly, if at any time during the quarter the client lived in subsidized housing, this variable

is coded as a 1 for that quarter. As can be seen, the bulk of the welfare clients over this period were white women, half of whom were high school graduates.

VI. Empirical Framework

The next step is to fit the data to the model. Given the absence of a time-limited subsidy, it is unclear whether agents have any incentive to find a job faster. On the other hand, it is plausible that the program may lead some clients to seek a lesser paying position so as to reap the benefits of the subsidy and then promptly quit and return to welfare thereafter, a finding substantiated in the earnings subsidy experiment discussed in Card and Hyslop (2005). There are essentially two major concerns in employing receipt of the moving subsidy as the treatment indicator. First, welfare clients were not randomly sampled and assigned to this program. As such, assistance with moving expenses may very well be correlated with unobservables in the error component. Additionally, the program requirement that potential candidates have a confirmed job offer or be employed as a condition to receiving the funds poses problems for the analysis of various labor market outcomes. To avoid both these problems, we implement the research design described below.

A. Construction of Policy Indicator

A fundamental question that naturally arises is which factors influence the decision to participate in the RAP program. It is noteworthy that clients are apprised of the program upon meeting with their case management specialist. To that effect, information is a driving factor in program participation. One would expect that there is heterogeneity among caseworkers in promoting the program. Unfortunately, while we do not have information on clients' caseworkers, we are aware of the local welfare office the client attended. In a similar vein, we

would expect variation in program promotion among welfare offices. This gives rise to the need to construct a proxy for advertising. First, the sample is divided into two parts, one containing all observations before the moving bonus program was introduced, and one containing all those afterward. In the “after” sample, we construct the following ratio for each office: the number of persons who moved to take a job and received the moving bonus divided by the number of persons who moved to take a job. In other words, this proxy is the number of people who received the subsidy as a fraction of those who were actually eligible to take it. This will be used as an indicator of how much the office promotes or advertises the program. Any permanent inter-office differences, such as more attention and focus on clients’ needs, more helpful staff, etc., will be absorbed into office fixed effects. Also, this proxy is not time-varying during the period after RAP was introduced. It should be noted that as calculated, moved is the number of times an individual moved summed over all persons in the county. Similarly, moving and receiving the moving payment is defined as the number of times a moving subsidy was disbursed, thus allowing for multiple disbursements for person. Restricting the dataset to first moves and/or initial disbursements only runs the risk of sample selection bias and masking the true extent of variation in the advertisement of the program.

Finally, we note that an unfortunate limitation of the data available for this study is the fact that, for non-RAP recipients, the subject's location within Kentucky is only available in the KAMES welfare file. Thus, for non-RAP recipients, we only observe whether they moved after a particular date if they reappear in the welfare rolls after that date. This poses a potential problem for our advertising proxy as constructed. Ideally, our proxy should be the ratio of (A) the number of welfare recipients in a county who receive RAP to (B) the total number of welfare recipients in a county who move to take a job. Due to our data limitations, we do not observe

welfare recipients who quit the welfare rolls never to return within the time period of our dataset (permanent leavers so to speak). Thus, (B) is underreported. This measurement error could potentially produce a spurious positive effect of RAP since local welfare offices with a high number of permanent leavers will also have high levels of the dependent variable (employment and earnings). As a check on the bias implied by this measurement error, we construct an alternative policy indicator as follows. Since data limitations prevent us from gathering geographical information on welfare leavers, we redefine (A) to include only individuals who reappear at some time in our data. More specifically, we define a permanent leaver as one who leaves welfare within a month of receiving their RAP payment and never returns. This will adjust for differences in the timing of the payment, we.e. whether the check was issued at the beginning or end of the month. In this way, the policy indicator is consistent in that it excludes successful, permanent welfare leavers in both the numerator and denominator.

It should be emphasized that the above constructed advertising proxy differs substantially from a program utilization or uptake rate. The latter runs the risk of being endogenous as high (or low) uptake rates may be concentrated in more economically disadvantaged areas. To fix ideas, consider Figures 3 and 4. Figure 3 maps per capita income in 1999 by county whereas Figures 4 and 5 map regional variation in the original and alternative advertising proxy. This rough illustration indicates that the policy indicator does not have a strong correlation with income.⁸ That is, the highest rates of advertising are not concentrated in the poorest areas. Complete advertising rates are listed in Tables A1 and A2 of the appendix. The original (alternative) policy indicator (always zero prior to program implementation) ranges from zero to 0.369 (0.226) in Menifee County.

B. Model Specifications

An obvious question that arises is whether in fact the program itself increases the number of welfare participants who move. As already discussed, moves are only observed for individuals actually receiving welfare. Thus, we cannot specifically test for labor migration enhancing mobility since we have no information on whether individuals move once they leave the welfare rolls. We subsequently choose to focus on other labor market variables that do not suffer from such measurement issues.

The objective is hence to determine the effect of this proxy for treatment status on various outcomes pertaining to the entire welfare population, namely employment and earnings. Additionally, both office and time fixed effects are included as the policy indicator changes by different amounts in different offices when the program is introduced. Thus, the goal is to estimate θ in the equation

$$Y_{iot} = \alpha + \theta T_{ot} + \mathbf{X}_{iot}' \beta + \sum \gamma_o + \sum \lambda_t + \varepsilon_{iot} \quad (1)$$

where Y_{iot} is the labor market outcome of interest for individual i in office o at time t , T_{ot} is the advertising proxy and always takes on a value of zero prior to May 1998, \mathbf{X}_{iot} is a vector of demographic characteristics, γ_o are office fixed effects that control for time invariant office characteristics; and λ_t are time fixed effects that control for statewide trends. Since the source of key variation and hence, identification is drawn from local welfare offices, standard errors are clustered by local office.

⁸ In a simple office level regression of the policy indicator in the after period on per capita income and a constant, the coefficient was on the order of magnitude of 10^{-7} . The overall regression had an F-statistic of 0.09. The t-statistic on per capita income was -0.31.

Non-employed welfare recipients are included in the sample. Moreover, individuals are coded as not employed between welfare spells if the client had no reported earnings. While the latter may introduce some measurement error from clients leaving the state of Kentucky, it should pose much less of a concern than to treat such observations as missing. In this way, the outcomes of interest are those that occur after the first welfare spell. Thus, earnings information prior to the first spell are used only as control variables as discussed earlier.

VII. Results

All real earnings are adjusted to April 2007 dollars. Standard errors are clustered by local welfare office. The dataset is sufficiently large to allow examination of the effect of the relocation program on subgroups of the population (with the exception of white men). Detecting differential program effects is particularly important from a policy maker's perspective as it may provide clues as to whether the program itself should be amended in some way. Moreover, as the sample composition is roughly 90 percent female and 80 percent white, examining such subsamples may allow us to focus on program effects in the absence of "outliers", whose presence may make it more difficult to elicit the true treatment effects. Furthermore, policy effects are computed as elasticities for ease of interpretation.

A. Employment

The estimated effects of RAP availability on current employment status are reported in Part A and Part B of Table 2 for the original and modified policy indicators respectively. The complete employment equation estimates using the original proxy are available in Table A3 in the appendix.⁹ The proxy for the RAP program has a statistically significant positive impact on

⁹ For the sake of space, the complete estimates using the original proxy are available in Table A3 in the appendix. They are, however, very similar to the original estimates, yielding slightly higher point estimates of the policy indicator.

quarterly employment across all sample specifications. For the full sample, the calculated elasticity suggests that a 10 percent increase in the RAP proxy is associated with a 2.68 percent (or 1 percentage point) increase in employment. Using the original proxy, such a 10 percent increase would roughly be the equivalent of the local office in Adair County increasing the fraction of movers eligible for the subsidy to that of the local office in McLean County. This elasticity varies significantly depending upon the group sampled with the highest elasticity for white men and the lowest for non-white women. Inspection of Table A3 reveals that white men have the lowest employment rates and non-white women the highest. This evidence would suggest that those who are the most disadvantaged in terms of employment stand the most to gain from the RAP program.

B. Unconditional Earnings

The second rows of Part A and B of Table 2 spell out the elasticities of earnings. Note that unconditional earnings include earnings of zero in quarters where the individual had no reported earnings. Complete estimates using the original proxy are reported in Table A4 in the Appendix. It should be emphasized that these are interpreted within the context of mean accepted wages. The full sample estimate in column (1) means that a 10 percent increase in the policy indicator would yield a 2.41 percent increase in unconditional earnings. Once again, white men and non-white women have the highest and lowest elasticities respectively and some of the lowest and highest unconditional wages.

C. Earnings Conditional on Employment

The third rows of Parts A and B of Table 2 show the effect of RAP when the sample is restricted to earnings conditional on employment, that is, earnings exceeding 100 dollars per

quarter.¹⁰ Theory suggests that RAP should increase post-employment wages. Inspection of the estimated elasticities suggests that while RAP's effect on conditional earnings may not be a hard zero, the magnitude is so small as to conclude that its effects are negligible. Reading from column (3) in Part A of Table 2, a 10 percent increase in the policy indicator would be associated with a 0.18 percent increase in quarterly conditional earnings for white women. Stated differently, a one standard deviation increase in the RAP proxy would generate a 1.4 percent increase in quarterly conditional earnings. Furthermore, it should be noted these estimates naturally suffer from selection bias implicit in the probability of being employed.

VIII. Robustness Checks

As a test of the robustness of our findings, we restrict our sample so as to exclude individuals who initiated a welfare spell after the RAP program was implemented. To ensure our model is being correctly identified, the results should be roughly comparable to those for the full sample of welfare clients. The concern here is that potentially there was an influx of individuals to the welfare rolls around 1998 in response to the program's implementation. Such a general equilibrium effect was raised by Meyer (1995) and discussed in an earlier section of the paper. If RAP created a perverse incentive to sign up for welfare just to receive the moving bonus, then any benefits from RAP could be lessened or potentially even reversed. Summary statistics comparing the restricted and excluded samples are reported in Table 3 alongside the complete sample. The estimated elasticities are reproduced in Table 4. Complete estimation results are

¹⁰ For the sake of brevity, complete regression results are omitted here, but available from the author upon request.

available from the author upon request. These results are very similar to those in Table 2, which is the preferred specification.¹¹

IX. Conclusions

The empirical findings of this paper suggest that relaxing liquidity constraints, by subsidizing geographic mobility, may raise the labor force participation of welfare clients. A one standard deviation increase in the RAP proxy, effectively, is associated with a 20.4 percent (or 7.56 percentage point) increase in employment and a 18.3 percent (or \$183) increase in quarterly unconditional earnings, which is robust to various specification checks. These findings are not consistent with most simple search and labor supply models, which predict slower re-employment. Instead, our positive estimates are more consistent with a liquidity constrained model such as Ardington et al's. (2007). That said, we are somewhat surprised by the magnitude of our estimated beneficial effects: Given that only a minority of welfare clients actually receive the moving subsidy, it must be that a large share of the job starts subsidized by the program are marginal (i.e. would not have occurred in its absence) or that the availability of the program has indirect effects even on persons who, *ex post*, do not receive a moving subsidy.

In addition to examining whether such a subsidy actually helps clients, it is also worthwhile to investigate whether such a relocation program is also cost effective. Due to the relatively short time span of the particular panel used, whether this program saves the state money in the long-run remains to be seen.

¹¹ We also divided up the sample by education to determine if the policy had differential effects by skill attainment. This made little difference in terms of the results.

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Figure 1: Inter County Moves with RAP

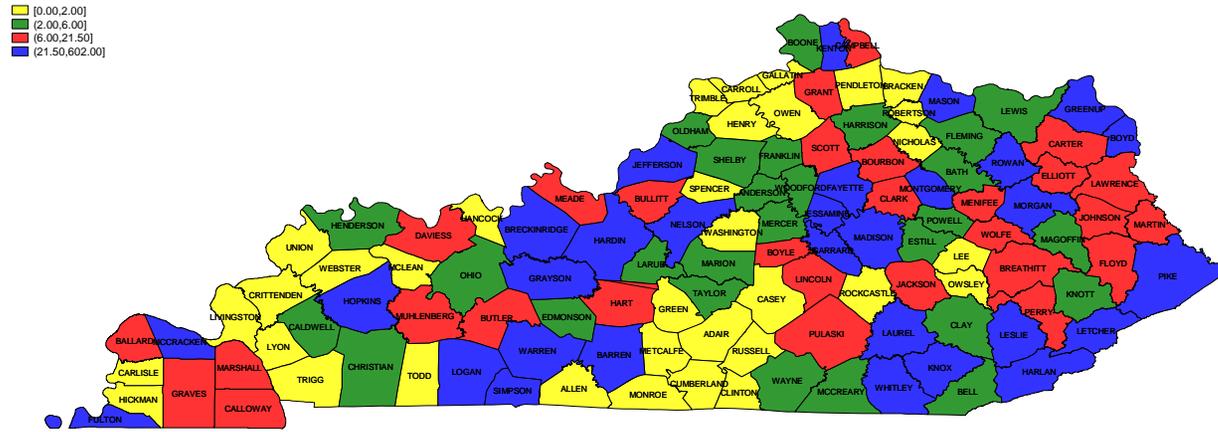


Figure 2: Net Intra County Moves with RAP

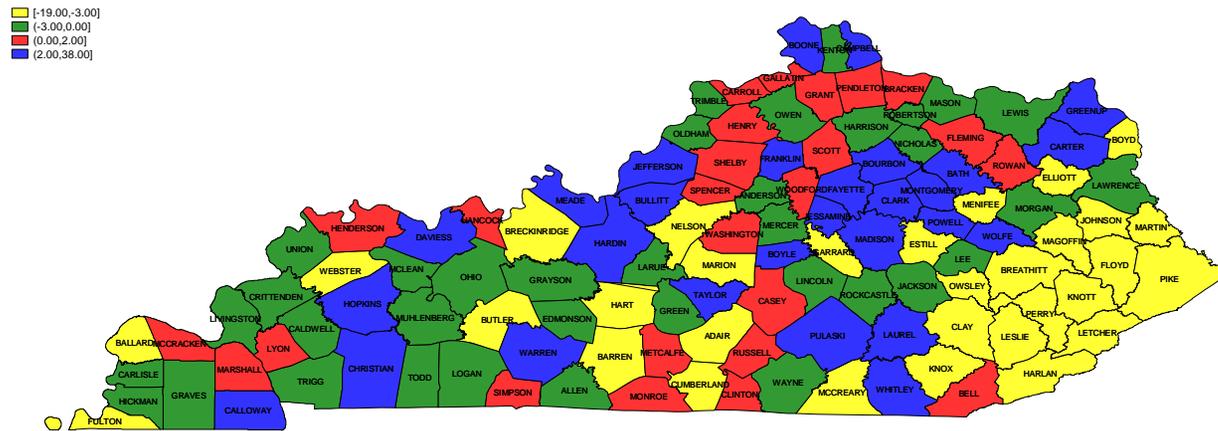


Figure 3: Kentucky Per Capita Income by County



Figure 4: Regional Variation in Policy Indicator

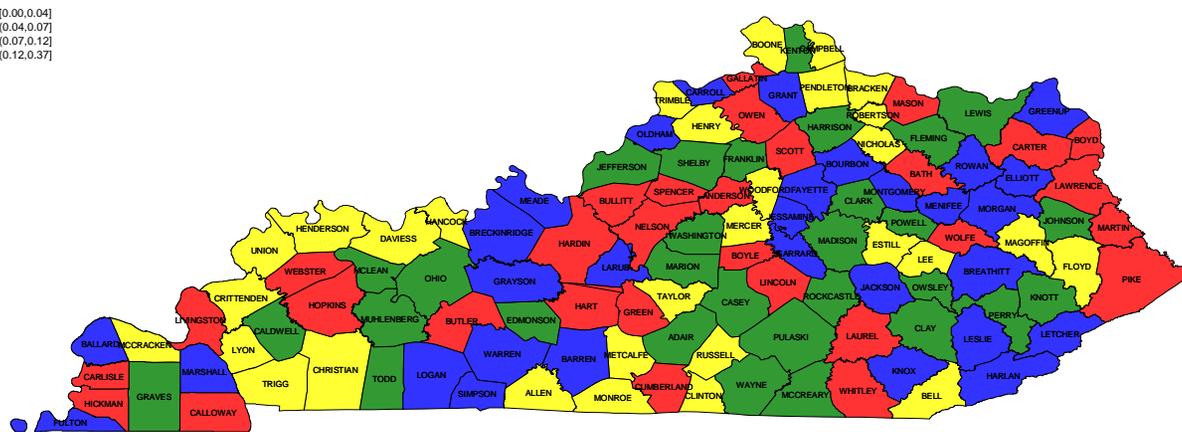


Table 1: Summary Statistics: Before and After Relocation Assistance Program Implemented May 1998

| | <i>Before</i> | | <i>After</i> | | <i>Entire Sample</i> | |
|--|---------------|--------------------|--------------|--------------------|----------------------|--------------------|
| | Mean | Standard Deviation | Mean | Standard Deviation | Mean | Standard Deviation |
| Subsidized Housing | 0.073 | 0.261 | 0.061 | 0.240 | 0.063 | 0.244 |
| Household Size | 2.817 | 1.009 | 2.806 | 0.985 | 2.808 | 0.990 |
| Age | 29.900 | 8.077 | 30.785 | 7.979 | 30.627 | 8.004 |
| Female | 0.912 | 0.284 | 0.908 | 0.289 | 0.909 | 0.288 |
| White | 0.799 | 0.400 | 0.775 | 0.418 | 0.779 | 0.415 |
| Married | 0.373 | 0.484 | 0.373 | 0.484 | 0.373 | 0.484 |
| Divorced | 0.171 | 0.377 | 0.150 | 0.357 | 0.154 | 0.361 |
| High School Graduate | 0.470 | 0.499 | 0.518 | 0.500 | 0.509 | 0.500 |
| Some College or above | 0.071 | 0.257 | 0.078 | 0.269 | 0.077 | 0.267 |
| Pre-welfare Quarter 1 Earnings | 494.413 | 1202.621 | 753.812 | 1535.499 | 707.358 | 1484.729 |
| Pre-welfare Quarter 2 Earnings | 455.278 | 1138.757 | 713.587 | 1473.967 | 667.328 | 1423.216 |
| Pre-welfare Quarter 3 Earnings | 422.323 | 1075.655 | 669.522 | 1405.098 | 625.253 | 1355.332 |
| Pre-welfare Quarter 4 Earnings | 375.087 | 967.597 | 586.634 | 1264.224 | 548.749 | 1219.132 |
| Quarterly Employment Rate | 0.244 | 0.429 | 0.399 | 0.490 | 0.371 | 0.483 |
| Quarterly Real Earnings (<i>not conditional on employment</i>) | 441.4339 | 1057.083 | 1120.642 | 1907.901 | 999.008 | 1804.477 |
| Number of observations | 118,632 | | 543,811 | | 662,443 | |
| Quarterly Real Earnings (<i>conditional on employment</i>) | 1,707.473 | 1,469.872 | 2,696.574 | 2,123.543 | 2,578.383 | 2,081.269 |
| Number of observations | 30,670 | | 225,997 | | 256,667 | |

Note: An observation is a person-quarter. This panel represents 31,248 persons. Formally, the sample includes anyone who initiated a welfare spell between quarter 1 of 1996 and quarter 2 of 2004.

Table 2: Effect of Relocation Assistance Program

| <i>A. Policy Indicator</i> | | | | | | | | |
|--|---------|---------|--------------|---------|-----------|---------------|-----------------|-----------|
| | All | Female | White Female | White | White Men | Non-White Men | Non-White Women | Non-White |
| <i>Elasticity</i> | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Employment | 0.268** | 0.261** | 0.304** | 0.311** | 0.375** | 0.291** | 0.145** | 0.149** |
| Unconditional earnings | 0.241** | 0.230** | 0.282** | 0.292** | 0.358** | 0.215** | 0.114** | 0.119** |
| Earnings conditional on employment | 0.004 | 0.004 | 0.018** | 0.017* | 0.00133 | -0.01443 | -0.018** | -0.019** |
| <i>B. Alternative Policy Indicator</i> | | | | | | | | |
| Employment | 0.266** | 0.257** | 0.305** | 0.313** | 0.381** | 0.279** | 0.138** | 0.143** |
| Unconditional earnings | 0.242** | 0.231** | 0.286** | 0.297** | 0.372** | 0.203** | 0.108** | 0.112** |
| Earnings conditional on employment | 0.005 | 0.005 | 0.020** | 0.020** | 0.010 | -0.016 | -0.018** | -0.020** |

Notes: * p<0.05, **p<0.01 Robust standard errors are clustered by office. Also included are office, and time fixed effects.

Table 3: Summary Statistics: Initiated a Welfare Spell Before and After Relocation Assistance Program Implemented May 1998

| | <i>Before</i> | | <i>After</i> | | <i>Entire Sample</i> | |
|--|---------------|--------------------|--------------|--------------------|----------------------|--------------------|
| | Mean | Standard Deviation | Mean | Standard Deviation | Mean | Standard Deviation |
| Subsidized Housing | 0.086 | 0.281 | 0.016 | 0.125 | 0.063 | 0.244 |
| Household Size | 2.853 | 1.010 | 2.715 | 0.940 | 2.808 | 0.990 |
| Age | 31.349 | 7.848 | 29.137 | 8.115 | 30.627 | 8.004 |
| Female | 0.918 | 0.275 | 0.890 | 0.312 | 0.909 | 0.288 |
| White | 0.775 | 0.418 | 0.790 | 0.408 | 0.779 | 0.415 |
| Married | 0.363 | 0.481 | 0.395 | 0.489 | 0.373 | 0.484 |
| Divorced | 0.169 | 0.375 | 0.123 | 0.329 | 0.154 | 0.361 |
| High School Graduate | 0.498 | 0.500 | 0.531 | 0.499 | 0.509 | 0.500 |
| Some College or above | 0.082 | 0.274 | 0.067 | 0.251 | 0.077 | 0.267 |
| Pre-welfare Quarter 1 Earnings | 585.740 | 1299.170 | 958.127 | 1782.189 | 707.358 | 1484.729 |
| Pre-welfare Quarter 2 Earnings | 545.420 | 1240.688 | 918.695 | 1712.983 | 667.328 | 1423.216 |
| Pre-welfare Quarter 3 Earnings | 512.359 | 1181.758 | 858.033 | 1632.327 | 625.253 | 1355.332 |
| Pre-welfare Quarter 4 Earnings | 457.936 | 1075.744 | 736.000 | 1453.519 | 548.749 | 1219.132 |
| Quarterly Employment Rate | 0.378 | 0.485 | 0.357 | 0.479 | 0.371 | 0.483 |
| Quarterly Real Earnings (<i>not conditional on employment</i>) | 1030.598 | 1828.423 | 933.871 | 1752.279 | 999.008 | 1804.477 |
| Number of observations | 446,095 | | 216,348 | | 662,443 | |
| Quarterly Real Earnings (<i>conditional on employment</i>) | 1,707.473 | 1,469.872 | 2,811.548 | 2,140.924 | 2,618.673 | 2,082.306 |
| Number of observations | 30,670 | | 144,894 | | 175,564 | |

Note: An observation is a person-quarter. This panel represents 31,248 persons. Formally, the sample includes anyone who initiated a welfare spell between quarter 1 of 1996 and quarter 2 of 2004. The panel of individuals initiating a welfare spell in the before period represents 14,897 persons, while that for the after period includes 16,351 persons.

Table 4: Effect of Relocation Assistance Program, Excludes Persons Initiating a Welfare Spell after Program Implementation

| | All | Female | White Female | White | White Men | Non-White Men | Non-White Women | Non-White |
|---|---------|---------|--------------|---------|-----------|---------------|-----------------|-----------|
| <i>Elasticity</i> | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Employment | 0.255** | 0.247** | 0.295** | 0.302** | 0.368** | 0.196* | 0.126** | 0.131** |
| Unconditional earnings | 0.237** | 0.225** | 0.282** | 0.293** | 0.369** | 0.159* | 0.095* | 0.103* |
| Earnings conditional on employment | 0.004 | 0.004 | 0.021** | 0.019** | 0.006 | -0.010 | -0.025** | -0.024** |
| <i>B. Alternative Policy Indicator</i> | | | | | | | | |
| Employment | 0.251** | 0.243** | 0.294** | 0.302** | 0.371** | 0.185* | 0.120** | 0.125** |
| Unconditional earnings | 0.236** | 0.223** | 0.283** | 0.295** | 0.376** | 0.150* | 0.089* | 0.096* |
| Earnings conditional on employment | 0.005 | 0.004 | 0.022** | 0.021** | 0.014 | -0.013 | -0.026** | -0.025** |

Notes: * p<0.05, **p<0.01 Robust standard errors are clustered by office. Also included are office, and time fixed effects.

Appendix Table A1: Policy Indicator by County after Program Implementation

| | | | | | |
|---------------------------|-------|------------|-------|------------------------------------|-------|
| Adair | 0.048 | Grant | 0.150 | McLean | 0.053 |
| Allen | 0.000 | Graves | 0.043 | Meade | 0.193 |
| Anderson | 0.075 | Grayson | 0.201 | Menifee | 0.369 |
| Ballard | 0.256 | Green | 0.075 | Mercer | 0.038 |
| Barren | 0.183 | Greenup | 0.149 | Metcalfe | 0.000 |
| Bath | 0.083 | Hancock | 0.021 | Monroe | 0.000 |
| Bell | 0.012 | Hardin | 0.084 | Montgomery | 0.150 |
| Boone | 0.041 | Harlan | 0.151 | Morgan | 0.175 |
| Bourbon | 0.130 | Harrison | 0.052 | Muhlenburg | 0.053 |
| Boyd | 0.122 | Hart | 0.109 | Nelson | 0.110 |
| Boyle | 0.073 | Henderson | 0.008 | Nicholas | 0.000 |
| Bracken | 0.037 | Henry | 0.023 | Ohio | 0.060 |
| Breathitt | 0.135 | Hickman | 0.093 | Oldham | 0.154 |
| Breckinridge | 0.308 | Hopkins | 0.085 | Owen | 0.083 |
| Builitt | 0.071 | Jackson | 0.155 | Owsley | 0.059 |
| Butler | 0.117 | Jefferson | 0.046 | Pendleton | 0.030 |
| Caldwell | 0.067 | Jessamine | 0.132 | Perry | 0.063 |
| Calloway | 0.107 | Johnson | 0.065 | Pike | 0.070 |
| Campbell | 0.038 | Kenton | 0.050 | Powell | 0.050 |
| Carlisle | 0.111 | Knott | 0.068 | Pulaski | 0.052 |
| Carroll | 0.130 | Knox | 0.213 | Robertson | 0.000 |
| Carter | 0.068 | Larue | 0.125 | Rockcastle | 0.047 |
| Casey | 0.043 | Laurel | 0.111 | Rowan | 0.144 |
| Christian | 0.011 | Lawrence | 0.108 | Russell | 0.010 |
| Clark | 0.058 | Lee | 0.036 | Scott | 0.115 |
| Clay | 0.053 | Leslie | 0.218 | Shelby | 0.061 |
| Clinton | 0.028 | Letcher | 0.152 | Simpson | 0.230 |
| Crittenden | 0.041 | Lewis | 0.048 | Spencer | 0.100 |
| Cumberland | 0.093 | Lincoln | 0.095 | Taylor | 0.043 |
| Daviess | 0.012 | Livingston | 0.071 | Todd | 0.065 |
| Edmonson | 0.065 | Logan | 0.157 | Trigg | 0.037 |
| Elliott | 0.196 | Lyon | 0.000 | Trimble | 0.036 |
| Estill | 0.019 | Madison | 0.067 | Union | 0.028 |
| Fayette | 0.202 | Magoffin | 0.029 | Warren | 0.191 |
| Fleming | 0.053 | Marion | 0.050 | Washington | 0.050 |
| Floyd | 0.033 | Marshall | 0.124 | Wayne | 0.054 |
| Franklin | 0.047 | Martin | 0.095 | Webster | 0.076 |
| Fulton | 0.173 | Mason | 0.119 | Whitley | 0.089 |
| Gallatin | 0.088 | McCracken | 0.038 | Wolfe | 0.107 |
| Garrard | 0.215 | McCreary | 0.056 | Woodford | 0.032 |
| Unweighted mean | 0.088 | | | Population weighted mean | 0.090 |
| Unweighted std. deviation | 0.067 | | | Population weighted std. deviation | 0.064 |

Appendix Table A2: Alternative Policy Indicator by County after Program Implementation

| | | | | | |
|---------------------------|-------|------------|-------|------------------------------------|-------|
| Adair | 0.040 | Grant | 0.090 | McLean | 0.053 |
| Allen | 0.000 | Graves | 0.040 | Meade | 0.070 |
| Anderson | 0.075 | Grayson | 0.122 | Menifee | 0.226 |
| Ballard | 0.179 | Green | 0.045 | Mercer | 0.028 |
| Barren | 0.150 | Greenup | 0.093 | Metcalfe | 0.000 |
| Bath | 0.056 | Hancock | 0.021 | Monroe | 0.000 |
| Bell | 0.012 | Hardin | 0.059 | Montgomery | 0.097 |
| Boone | 0.007 | Harlan | 0.089 | Morgan | 0.124 |
| Bourbon | 0.070 | Harrison | 0.041 | Muhlenburg | 0.033 |
| Boyd | 0.091 | Hart | 0.072 | Nelson | 0.083 |
| Boyle | 0.055 | Henderson | 0.003 | Nicholas | 0.000 |
| Bracken | 0.037 | Henry | 0.023 | Ohio | 0.047 |
| Breathitt | 0.099 | Hickman | 0.070 | Oldham | 0.115 |
| Breckinridge | 0.182 | Hopkins | 0.070 | Owen | 0.083 |
| Builitt | 0.058 | Jackson | 0.069 | Owsley | 0.029 |
| Butler | 0.097 | Jefferson | 0.034 | Pendleton | 0.030 |
| Caldwell | 0.067 | Jessamine | 0.078 | Perry | 0.058 |
| Calloway | 0.054 | Johnson | 0.047 | Pike | 0.051 |
| Campbell | 0.030 | Kenton | 0.035 | Powell | 0.035 |
| Carlisle | 0.037 | Knott | 0.046 | Pulaski | 0.034 |
| Carroll | 0.043 | Knox | 0.143 | Robertson | 0.000 |
| Carter | 0.041 | Larue | 0.071 | Rockcastle | 0.019 |
| Casey | 0.022 | Laurel | 0.078 | Rowan | 0.095 |
| Christian | 0.009 | Lawrence | 0.060 | Russell | 0.010 |
| Clark | 0.033 | Lee | 0.036 | Scott | 0.088 |
| Clay | 0.021 | Leslie | 0.148 | Shelby | 0.053 |
| Clinton | 0.028 | Letcher | 0.106 | Simpson | 0.190 |
| Crittenden | 0.014 | Lewis | 0.032 | Spencer | 0.050 |
| Cumberland | 0.070 | Lincoln | 0.078 | Taylor | 0.034 |
| Daviess | 0.006 | Livingston | 0.054 | Todd | 0.032 |
| Edmonson | 0.043 | Logan | 0.108 | Trigg | 0.037 |
| Elliott | 0.152 | Lyon | 0.000 | Trimble | 0.036 |
| Estill | 0.019 | Madison | 0.053 | Union | 0.014 |
| Fayette | 0.151 | Magoffin | 0.025 | Warren | 0.155 |
| Fleming | 0.035 | Marion | 0.043 | Washington | 0.050 |
| Floyd | 0.021 | Marshall | 0.083 | Wayne | 0.040 |
| Franklin | 0.016 | Martin | 0.058 | Webster | 0.067 |
| Fulton | 0.151 | Mason | 0.083 | Whitley | 0.065 |
| Gallatin | 0.029 | McCracken | 0.029 | Wolfe | 0.089 |
| Garrard | 0.162 | McCreary | 0.051 | Woodford | 0.032 |
| Unweighted mean | 0.061 | | | Population weighted mean | 0.064 |
| Unweighted std. deviation | 0.046 | | | Population weighted std. deviation | 0.046 |

Appendix Table A3: Effect of Relocation Assistance Program on Employment

| | All | Women | White Women | White | White Men | Non-White Men | Non-White Women | Non-White |
|---------------------------------------|---------------------|---------------------|---------------------|---------------------|--------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Mean Quarterly Employment Rate</i> | 0.371 (0.483) | 0.380 (0.485) | 0.345 (0.475) | 0.336 (0.472) | 0.258 (0.438) | 0.416 (0.493) | 0.497 (0.500) | 0.493 (0.500) |
| Policy Indicator | 1.370** (0.174) | 1.373** (0.181) | 1.425** (0.133) | 1.413** (0.131) | 1.250** (0.150) | 1.743** (0.434) | 1.063** (0.354) | 1.088** (0.364) |
| Subsidized Housing | 0.317** (0.009) | 0.315** (0.009) | 0.331** (0.007) | 0.333** (0.007) | 0.346** (0.037) | 0.236** (0.034) | 0.287** (0.009) | 0.287** (0.009) |
| Household Size | -0.001 (0.002) | -0.001 (0.002) | -0.001 (0.002) | -0.001 (0.002) | 0.004 (0.005) | 0.023 (0.015) | -0.002 (0.003) | -0.001 (0.003) |
| Age | 0.008** (0.001) | 0.008** (0.001) | 0.007** (0.002) | 0.008** (0.001) | -0.005 (0.004) | 0.021* (0.008) | 0.012** (0.003) | 0.011** (0.003) |
| Age squared | -0.000** (0.000) | -0.000** (0.000) | -0.000** (0.000) | -0.000** (0.000) | -0.000 (0.000) | -0.000** (0.000) | -0.000** (0.000) | -0.000** (0.000) |
| Female | 0.052** (0.009) | | | 0.053** (0.009) | | | | 0.038 (0.021) |
| White | -0.056** (0.010) | -0.055** (0.011) | | | | | | |
| Married | -0.018** (0.005) | -0.021** (0.004) | -0.019** (0.004) | -0.017** (0.005) | 0.020 (0.022) | 0.007 (0.051) | -0.023** (0.009) | -0.021 (0.012) |
| Divorced | -0.016** (0.005) | -0.018** (0.005) | -0.018** (0.005) | -0.018** (0.005) | 0.006 (0.019) | 0.144** (0.040) | -0.024 (0.015) | -0.013 (0.014) |
| High School Graduate | 0.055** (0.004) | 0.053** (0.004) | 0.056** (0.004) | 0.057** (0.004) | 0.064** (0.010) | 0.062 (0.045) | 0.044** (0.005) | 0.044** (0.005) |
| Some College or above | 0.056** (0.010) | 0.054** (0.011) | 0.067** (0.012) | 0.069** (0.010) | 0.064* (0.030) | 0.006 (0.073) | 0.019 (0.016) | 0.020 (0.014) |
| Pre welfare quarter 1 earnings | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | 0.000 (0.000) | 0.000** (0.000) | 0.000** (0.000) |
| Pre welfare quarter 2 earnings | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | -0.000 (0.000) | 0.000** (0.000) | 0.000 (0.000) | 0.000** (0.000) |
| Pre welfare quarter 3 earnings | 0.000 (0.000) | 0.000 (0.000) | 0.000 (0.000) | 0.000 (0.000) | -0.000 (0.000) | -0.000 (0.000) | 0.000* (0.000) | 0.000* (0.000) |
| Pre welfare quarter 4 earnings | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | 0.000** (0.000) | 0.000* (0.000) | 0.000** (0.000) | 0.000** (0.000) |
| Constant | 0.102** (0.036) | 0.151** (0.036) | 0.063* (0.031) | 0.011 (0.028) | 0.247** (0.078) | -0.092 (0.220) | 0.208** (0.044) | 0.188** (0.046) |
| Number of observations | 662,443 | 602,047 | 462,864 | 516,362 | 53,498 | 6,898 | 139,183 | 146,081 |
| R squared | 0.132 | 0.132 | 0.117 | 0.116 | 0.114 | 0.163 | 0.129 | 0.128 |

Notes: * p<0.05, **p<0.01 Robust standard errors are clustered by office. Also included are office, and time fixed effects.

Appendix Table A4: Effect of Relocation Assistance Program on Unconditional Earnings

| | All | Female | White Female | White | White Men | Non-White Men | Non-White Women | Non-White |
|--|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|----------------------------|---------------------------|---------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Mean Quarterly Unconditional Earnings</i> | 999.008 (1804.477) | 1004.231 (1783.965) | 901.977 (1701.742) | 900.921 (1729.399) | 891.788 (1952.407) | 1374.799 (2267.173) | 1344.274 (1996.310) | 1345.715 (2009.917) |
| Policy Indicator | 3,308.367** (340.917) | 3,196.662** (324.999) | 3,450.408** (320.733) | 3,555.019** (335.983) | 4,121.520** (600.237) | 4,251.298** (1,179.471) | 2,272.864** (814.577) | 2,364.691** (863.766) |
| Subsidized Housing | 1,116.939** (36.096) | 1,121.958** (37.794) | 1,142.185** (42.425) | 1,141.828** (42.549) | 1,129.960** (190.317) | 439.715** (162.022) | 1,101.022** (62.344) | 1,088.910** (58.833) |
| Household Size | 9.076 (8.894) | 4.349 (9.012) | 2.766 (9.395) | 6.718 (8.651) | 24.751 (21.795) | 184.257** (52.277) | 6.268 (15.805) | 16.844 (16.095) |
| Age | 69.725** (3.769) | 70.475** (4.474) | 62.951** (4.996) | 63.792** (4.399) | 29.622 (20.046) | 142.459** (28.644) | 100.549** (9.766) | 94.056** (8.393) |
| Age squared | -0.925** (0.060) | -0.929** (0.073) | -0.804** (0.074) | -0.832** (0.065) | -0.483 (0.268) | -1.883** (0.376) | -1.438** (0.127) | -1.328** (0.115) |
| Female | -11.686 (43.129) | | | -16.309 (39.814) | | | | 1.754 (123.130) |
| White | -103.667** (31.305) | -107.466** (31.463) | | | | | | |
| Married | -73.362* (29.674) | -96.082** (17.829) | -94.546** (16.976) | -76.752** (22.274) | 129.235 (140.329) | 433.443 (341.144) | -92.930* (41.531) | -39.376 (72.389) |
| Divorced | -80.267** (21.048) | -85.691** (18.737) | -80.288** (20.004) | -80.945** (20.373) | -14.132 (98.244) | 579.248** (164.138) | -145.269* (64.430) | -95.816 (64.585) |
| High School Graduate | 288.190** (22.410) | 279.347** (21.905) | 268.523** (16.199) | 280.951** (17.806) | 354.214** (57.352) | 195.627 (239.404) | 322.822** (44.079) | 321.460** (37.469) |
| Some College or above | 514.318** (35.412) | 508.740** (35.431) | 513.958** (37.073) | 521.130** (37.173) | 464.638** (147.651) | 77.755 (466.451) | 503.747** (84.608) | 498.506** (72.833) |
| Pre welfare quarter 1 earnings | 0.062** (0.008) | 0.063** (0.008) | 0.050** (0.012) | 0.054** (0.010) | 0.067** (0.020) | 0.044 (0.073) | 0.095** (0.009) | 0.088** (0.012) |
| Pre welfare quarter 2 earnings | 0.049** (0.009) | 0.055** (0.009) | 0.061** (0.013) | 0.049** (0.011) | -0.004 (0.019) | 0.156 (0.078) | 0.040** (0.014) | 0.048** (0.017) |
| Pre welfare quarter 3 earnings | 0.025* (0.012) | 0.036** (0.014) | 0.023 (0.016) | 0.011 (0.013) | -0.032 (0.022) | -0.050 (0.064) | 0.068** (0.016) | 0.063** (0.014) |
| Pre welfare quarter 4 earnings | 0.126** (0.010) | 0.123** (0.011) | 0.125** (0.015) | 0.131** (0.013) | 0.138** (0.023) | 0.025 (0.072) | 0.114** (0.012) | 0.109** (0.010) |
| Constant | -1,049.012** (71.786) | -1,056.023** (91.317) | -1,053.117** (84.375) | -1,056.279** (74.667) | -520.172 (412.597) | -3,011.080** (593.857) | -1,496.509** (131.630) | -1,434.578** (149.207) |
| Number of observations | 662,443 | 602,047 | 462,864 | 516,362 | 53,498 | 6,898 | 139,183 | 146,081 |
| R squared | 0.119 | 0.127 | 0.114 | 0.106 | 0.097 | 0.132 | 0.135 | 0.129 |

Notes: * p<0.05, **p<0.01 Robust standard errors are clustered by office. Also included are office, and time fixed effects.