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Unemployment insurance and physical activity

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ABSTRACT

Unemployment insurance (UI) reduces the opportunity cost of leisure, but it is unknown whether this additional leisure time is physically active. To obtain unbiased estimates of the effect of UI on physically active leisure participation, I exploit changes in UI program legislation across US states and time. Using nationally representative monthly data between 2003 and 2010 from the Behavioral Risk Factor Surveillance System (BRFSS) and the American Time Use Survey (ATUS), I find evidence that both state UI eligibility expansions and increases in maximum allowable state UI benefits coincide with greater probability of physical activity among the recently unemployed. Based on point estimates, state UI eligibility expansions increased the probability of physical activity participation by 8 to 10 percentage points among the unemployed with less than a high school education, while a 10 percent increase in the maximum allowable state UI benefit increased the probability of physical activity by 0.3 to 0.6 percentage points among the unemployed who have completed high school or some college.

Keywords: Unemployment; physical activity; difference-in-difference-in-difference; Grossman model, labor-leisure tradeoff; BRFSS; ATUS

1. INTRODUCTION

Many studies suggest that job loss has deleterious effects on a variety of health behaviors and conditions and may even increase the risk of premature death (Catalano et al., 2011, Modrek, 2013, Browning and Heinesen, 2012, Sullivan and von Wachter, 2009). However the notion that increases in non-labor time may actually be good for health is supported by several studies that find increases in unemployment to be associated with reductions in overall mortality rates (Tapia Granados, 2005, Ruhm, 1995, Ruhm, 2000, Ruhm, 2003, Ruhm, 2005, Gerdtham and Ruhm, 2006). A common explanation for the latter is that healthy lifestyles are also countercyclical: joblessness increases physical activity among the habitually inactive, as well as weight loss among the severely obese. For example, a one percentage point increase in US state unemployment rates has been associated with a sizeable 1.5% increase in physical activity and a 1.4% decrease in severe obesity at the population level (Ruhm 2005).

In this paper, I test whether unemployment insurance (UI) might explain why some individuals engage in physical activity while out of work. While UI programs are not designed specifically to promote healthy behaviors, it is well established that UI reduces the opportunity cost of leisure (Chetty 2008; Moffitt and Nicholson 1982; Mortensen 1977). Although sedentary and physically active leisure are both subsidized by UI, economic theory posits that demand for time-intensive, health promoting activities will increase as the price of engaging in these activities decreases. This could mean that UI recipients are more likely to participate in physically active leisure compared to non-recipients.

In the US, a key methodological challenge in assessing the causal impact of UI on physically active leisure is the non-random selection into benefit receipt, as not all unemployed people are eligible, or apply for UI. To obtain unbiased estimates of the effect of UI on physical activity, I take two approaches. First, I exploit wide variation across US states in the timing of a policy known as 'Alternate Base Period' (ABP) which allows unemployed workers to file UI claims based on wages earned closer in proximity to the time of job displacement. This policy uniquely expands UI eligibility for unemployed individuals with irregular work history, the vast majority of whom have less than a high school education; however, the policy has not been shown to affect UI receipt among more highly educated groups. In the second approach, I make use of within-state variations in the maximum allowable UI benefit level. Although the dollar value of UI benefits received is individually determined, state laws define the maximum amount and duration of benefits that workers are entitled to receive after job loss, leading to considerable variability across states and time in terms of UI benefit generosity.

I test the effects of these variations in state UI laws on nationally representative monthly data between 2003 and 2010 from both the Behavioral Risk Factor Surveillance System (BRFSS) and the American Time Use Survey (ATUS). I consistently find that UI is associated with greater participation in physical activity. ABP state UI eligibility expansions coincide with increased probability of engaging in physical activity among unemployed people with no high school degree using difference-in-difference models and difference-in-difference-in-difference models (where additional control groups are formed of more highly educated unemployed individuals whose UI eligibility is unaffected by the expansion). Likewise, I find increases in maximum allowable state UI benefit levels are associated with greater probability of participating in physical activity among unemployed high school graduates and those who attended some college. However there are no significant effects among the highly educated unemployed, for whom changes in benefit levels are likely to replace a relatively small share of previous wages, or for low educated unemployed who are unlikely to qualify to receive the maximum allowable UI benefit level.

2. BACKGROUND

2.1 Literature review

The rationale for UI to increase physical activity is tied to the expectation that unemployment benefits lengthen unemployment duration by distorting job search incentives and subsidizing leisure time, with the strongest effects among liquidity constrained households (Chetty 2008; Moffitt and Nicholson 1982; Mortensen 1977). This hypothesis is derived directly from labor supply theory, which proposes a trade-off between deciding whether to engage in labor or leisure to maximize utility. During periods of joblessness, individuals do not engage in wage producing labor, leading to greater consumption of leisure due to reductions in its opportunity cost. The decreased cost of leisure time associated with joblessness is likely to be conditional to some extent on access to financial resources; otherwise, a large portion of time while unemployed must be allocated to job search to preserve consumption levels (Gruber, 1997).

For the unemployed receiving unemployment benefits, as there is no work effort or time required to produce additional income, there is less need to choose between labor, job search[1], and leisure (Besley and Coate, 1992). Leisure time—both sedentary and physically active— is effectively subsidized by unemployment benefits (Holmlund, 1998) with the choice between sedentary and physically active leisure depending to some extent on individual preferences. An important question is therefore whether this additional leisure time attributed to unemployment benefits could inadvertently be health promoting. If individuals with access to unemployment benefits choose to spend some of their newfound leisure time engaging in physical activities, the time off of work could ultimately improve their health.

The canonical Grossman model of demand for health posits that demand for time-intensive health promoting activities will increase as the price of engaging in these activities decreases (Grossman, 1972, Becker, 1965). A utility maximizing unemployed individual with excess free time could be expected to spend some of this time investing in their health by participating in physical activities. With leisure time underwritten by UI, the price of undertaking time consuming healthy activities, such as exercise, diminishes substantially. This temporary increase in income from UI without commensurate work effort is distinct from temporary wage increases requiring labor, which reduce health investment behaviors due to their propensity to encourage additional work hours (Dustmann and Windmeijer, 2000). The increase in income associated with UI could therefore result in increases in active, health producing leisure.

[1] All US states require that the unemployed actively search for work—for example, by signing up for internet employment-search services or keeping a record of weekly work searches—to be UI eligible. Therefore, some amount of time must be allocated to job searching for UI benefit receivers.

There is in fact already evidence already suggesting that UI has a positive effect on health, though no studies have investigated whether there are effects of UI on physical activity or time spent engaging in healthy behaviors (Rodriguez, 2001, Rodríguez et al., 2001, Rodriguez et al., 1997, McLeod et al., 2012a). Most studies linking unemployment benefit programs to health have focused only on the association between actual receipt of unemployment benefits and self-assessed health measures. In general, these studies suggest that unemployed workers receiving benefits are in better health than unemployed workers who do not receive unemployment benefits. A potential caveat of these studies is the strong selection associated with claiming or being eligible for unemployment benefits. Eligibility to receive benefits, as well as the amount of benefits received, is determined based on a worker's career, salary, and reason for job loss; each of these factors is plausibly an independent predictor of health behaviors. Cylus et al provide convincing 'quasi-experimental' evidence that the level of UI generosity can play an important role in health; exploiting variation across states and time in the maximum allowable state UI benefit level, the authors find that more generous UI benefit programs reduce the likelihood of poor self-assessed health among the unemployed (Cylus, Glymour et al. 2015) and slightly moderate the effect of unemployment rates on suicides (Cylus, Glymour et al. 2014). However whether the health effects of UI are driven by changes in income, leisure time, or a combination remains unclear.

The idea that UI could incentivize physical activity is also consistent with literature on the determinants of physical activity participation. In the US, lack of time has been cited as a primary reason for physical inactivity (Brownson et al., 2001). A study using the BRFSS dataset finds that increases in hours of work are associated with less physical activity among the low educated; the author emphasizes that changes in time rather than changes in income drive the results (Xu, 2013). Research also indicates that as wages and the opportunity cost of time increase, the intensity of physical activity increases so that less time is needed to achieve comparable levels of fitness (Meltzer and Jena, 2010). This implies that for UI recipients, whose opportunity cost of time is low, the decision to engage in physical activity may result in relatively less-intensive, more time-consuming leisurely physical activity, such as walking. A recent study from 2003 to 2010 using the ATUS also finds that physical activity increases as a result of unemployment, with effects largely among low-educated men; however the increased physical activity associated with unemployment does not fully substitute for decreases in work-related physical activity (Colman and Dave, 2013).

2.2 Unemployment insurance in the US

The Federal-state UI program in the US was established as part of the Social Security Act of 1935, following years of fragmented and largely unsuccessful attempts at unemployment compensation legislation in various states. A key barrier to creating unemployment benefit programs at the state level was the concern that financing an unemployment benefit program based on employer taxes would lead to variations across states in employer costs, stifling interstate competition. The Social Security Act’s main contribution was therefore not to set up a Federal unemployment benefit program, but rather, the Act made it easier for states to establish their own unemployment benefit programs because it created a Federal unemployment tax to be levied equally across all employers in all states.

As a result, states are responsible for designing and administering their own UI programs based on general principles set by the Federal government. Each state UI program provides qualifying job losers with varying levels of temporary wage replacement for a limited period of time, with the maximum allowable weekly benefit level and the maximum duration of benefit receipt determined by the states themselves. This results in a large degree of heterogeneity in maximum allowable UI levels (weekly maximum benefit level X maximum duration) across states and time. In 2010, the maximum allowable state UI benefit level varied from a low of $6,110 in Mississippi to a high of $28,290 in Massachusetts. From 2003 to 2010, the largest increase in maximum allowable benefits was in New Mexico ($6,474) and the largest decrease was in Washington (-$320); the median state change during that time was in California ($2,080). Four states (Florida, Michigan, New York and Tennessee) made no changes to their maximum UI levels between 2003 and 2010.

Job losers are not guaranteed to receive UI; on the contrary, eligibility, as well as the actual amount of benefit received (subject to the maximum allowable benefit level) is based on complex criteria that job losers must meet, and which differ substantially across states and over time. One of the key impediments to UI eligibility relates to an individual's prior work history (US Department of Labor, 2009a). To receive UI, unemployed individuals must have a minimum level of earnings as determined by each state over a predefined base period; historically, this base period has comprised the earliest four of the previous five completed quarters before job loss (Figure 1, upper panel). The purpose of requiring a minimum level of earnings over a standard base period is to ensure that individuals in receipt of benefits have sufficient attachment to the labor market prior to job loss; the lag between job loss and the base period allows sufficient time for administrative UI eligibility processing. Individuals who do not have adequate earnings during this standard base period are not eligible to receive UI benefits. This largely penalizes individuals with irregular work histories and low wages; research shows that low earners are less likely than high earners to receive UI, underscoring the complications of studying any effects of UI via direct comparisons between UI recipients and non-recipients (Gould-Werth and Shaefer, 2012).

2.2.1 UI modernization: Alternate Base Periods

In an effort to increase UI take-up among marginalized workers, states have progressively been allowing the unemployed to claim UI eligibility using wages earned over Alternate Base Periods (ABP). Under ABP, UI eligibility is not based on earnings during the earliest four of the previous five completed quarters, but rather, the eligibility window is shifted forward by one quarter to comprise the four most recently completed quarters (Figure 1, lower panel). By shifting the base period window to account for more recent earnings, individuals who have unsteady work histories have a greater chance of qualifying for UI. ABP implementation may also increase application rates among individuals who would not have applied otherwise (O'Leary, 2010).

Figure 1. Time periods used to determine eligibility for UI, standard base period vs. alternate base period

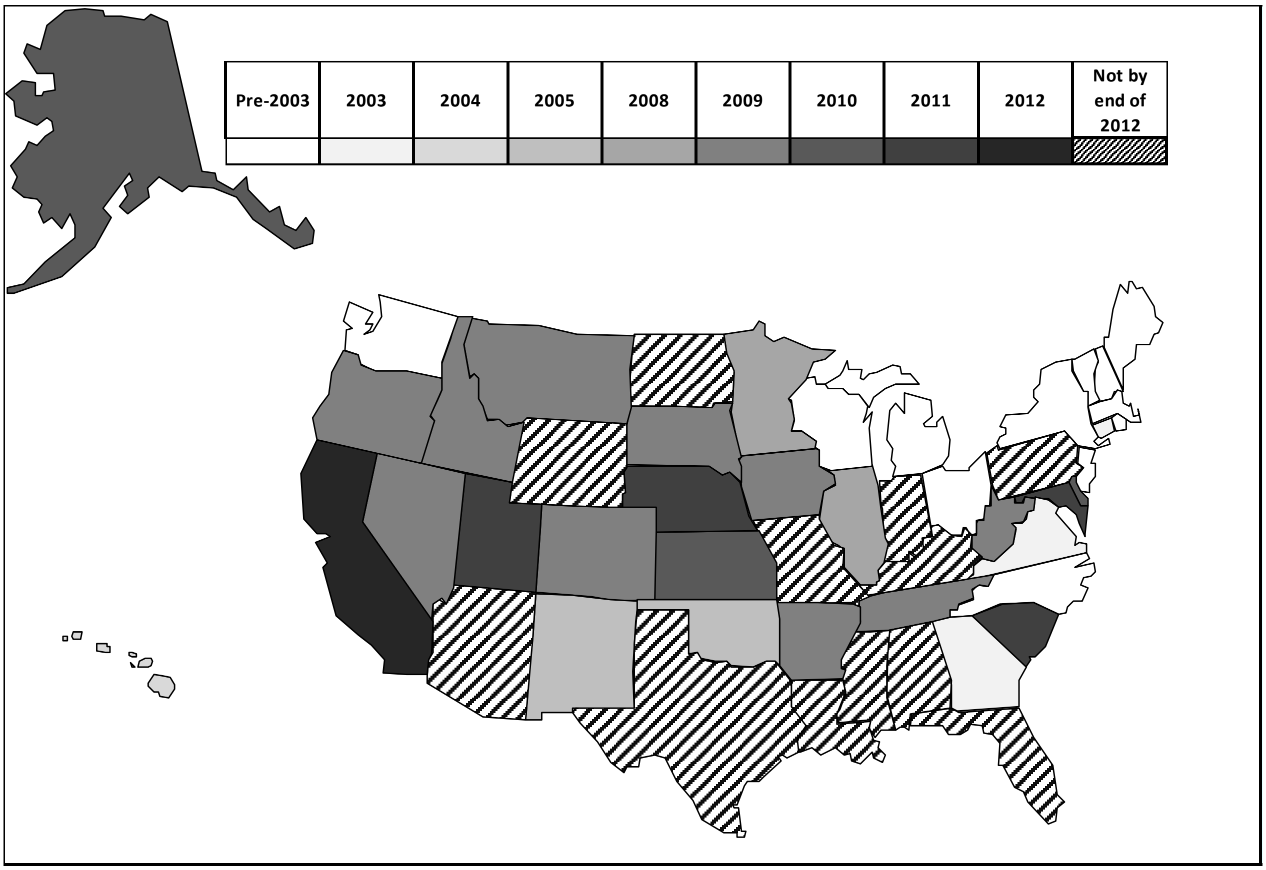
Standard Base Period					
Q1	Q2	Q3	Q4	Q5	Example date of job loss
January-March 2014	April-June 2014	July-September 2014	October-December 2014	January-March 2015	April 15th 2015
Alternate Base Period					
Q1	Q2	Q3	Q4	Q5	Example date of job loss
January-March 2014	April-June 2014	July-September 2014	October-December 2014	January-March 2015	April 15th 2015

Source: Adapted based on Gould-Werth, A., & Shaefer, H. L. (2013)

Note: The grey boxes are the quarters that are used to determine monetary eligibility for UI given April 15th, 2014 as the hypothetical date of job loss. For a worker to be eligible for UI, they must meet State earnings requirements in the quarters highlighted in grey.

The first state to implement ABP was Vermont in 1988; by the end of the 20th century, only 6 more states had followed suit (Rhode Island, Washington, New Jersey, Ohio, North Carolina and New York) followed by Maine, Massachusetts, Michigan, Wisconsin and New Hampshire by 2001 (Figure 2). However between 2003 and 2010, 21 more states plus Washington D.C. enacted legislation for ABP at varying points in time. One of the reasons for such a large increase in ABP is that as part of the American Recovery and Reinvestment Act (ARRA) of 2009, states were given access to special funds totaling $7 billion, conditional on reforms to modernize their UI program. One-third of these funds were made available to states if they had ABP in place, which led 10 states to enact ABP legislation in 2009 followed by 3 more states in 2010 (O'Leary, 2010). These Federal stimulus funds were subsequently transferred into each state's UI trust fund, without any requirement for the funding to pay for the UI modernization reforms themselves.

Figure 2. Year of Alternate Base Period implementation in US states



Source: Based on information from the US Department of Labor website: (http://workforcesecurity.doleta.gov/unemploy/laws.asp)

While ABP legislation increases the share of the unemployed who are eligible to receive UI, there is only limited evidence of the degree to which it has increased UI take-up. A report for the US Department of Labor in 1995 concluded that, based on five of the six states that had enacted ABP policy at that time, the presence of ABP could increase the number of eligible UI claimants by between 6 and 8 percent overall (Vroman, 1995). The study found that, as expected, beneficiaries of the policy were typically low-wage earners, as earnings among ABP eligible individuals were lower than workers who were eligible under the standard base period. Another simulation using data from the Survey of Income and Program Participation (SIPP) also finds that low-wage workers (in the bottom quartile of wage earners) disproportionately gain from ABP (Stettner et al., 2005).

The only nationally representative study of ABP uses data from the Current Population Survey (CPS) and finds analogous evidence that ABP increases UI take-up among low wage earners (Gould-Werth and Shaefer 2013). Despite well-known underreporting of UI receipt in the CPS, the authors conclude that between 1987 and 2011, the unemployed seeking part-time work with less than a high school degree were more likely to receive UI under ABP, but they do not find statistically significant effects on UI take-up among other more highly educated unemployed cohorts. This result is perhaps unsurprising, given that non-high school graduates are likely to be low-wage, part-time and intermittent workers – the target demographic of the policy.

3. METHODS

3.1 Data

The primary data source for this study is the BRFSS, which is a nationally representative repeated cross-sectional dataset and the largest telephone survey in the world (Centers for

Disease Control, 2014). The BRFSS collects data on personal health behaviors and individual characteristics and has frequently been used to study the relationship between unemployment and health (Ruhm and Black, 2002, Ruhm, 2005, Ruhm, 2003, Dee, 2001, Tefft and Kageleiry, 2014, Helliwell et al., 2011).

To supplement the analysis, I also use data from the ATUS (Bureau of Labor Statistics, 2014). Sponsored by the Bureau of Labour Statistics and conducted by the US Census Bureau, the ATUS is a nationally representative repeated cross-sectional dataset comprised of randomly selected individuals from the CPS. Interviewees report detailed information on how they spent their time, minute-by-minute, during the previous day. The ATUS has been used previously to investigate time spent job searching as well as time spent on health promoting activities (Krueger and Mueller, 2010, Cawley and Liu, 2012, Tudor-Locke et al., 2010, Colman and Dave, 2013).

I use data from the 2003 through 2010 waves of both surveys because since 2011, the BRFSS has changed its weighting methodology to iterative proportional fitting, which replaced the previously used post-stratification weighting method[2]. Likewise, the Bureau of Labor Statistics provides state-specific monthly unemployment rates beginning in 2003, which I use as a control variable to proxy economic conditions.

The BRFSS outcome variable of interest is a self-reported yes-no question regarding whether the respondent took part in any leisure time physical activity during the past

[2] Weighting is required to account for unequal probabilities of respondents being included in the survey (Ruhm 2005); weights make the BRFSS data representative of the adult population in the state, allowing me to obtain consistent estimates of average treatment effects (Ruhm & Black, 2002).

month: *During the past month, other than your regular job, did you participate in any physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise?*. Despite the potential for measurement error, research suggests that self-reported measures of physical activity, such as the question used in the BRFSS, are valid, reliable and correlate with objective measures (Aires et al., 2003, Yore et al., 2007). These types of questions are commonly used to measure physical activity (Brownson et al., 2005, Ford et al., 2010, Barker et al., 2011, Mensah et al., 2005, Hackmann et al., 2012, Tekin et al., 2013). While other BRFSS data on self-reported moderate and vigorous physical activity are potentially of interest, they are only available in alternating BRFSS waves (i.e. in odd years) so that there are only 4 years of data between 2003 and 2010. This not only reduces the sample size considerably, but also means that in many instances, there are no observations at, or around the actual time of a state's ABP implementation, as well as fewer within-state changes in UI maximum allowable benefits. As a result, I do not make use of these variables.

While the ATUS does not contain the identical leisure-time physical activity question as the BRFSS, it does contain information on the minutes spent participating in a long list of sporting activities. I limit the analysis to minutes spent walking, running, or engaging in any sporting activity overall because walking and running are commonly reported sporting activities, do not require specific equipment, and can be done without team members or competitors; the any sporting activity category captures all types of physical activities in aggregate. I do not include minutes spent walking or running while traveling from one place to another, as I consider this to be a mode of transport rather than participation in a leisure time physical activity. Although the reported minutes spent exercising in the ATUS could be considered a more objective outcome measure than the self-reported question in the BRFSS, the ATUS itself has a number of drawbacks. Importantly, the ATUS unemployed sample is considerably smaller than the BRFSS. This may be problematic given the small effect of ABP on UI take-up and the fact that I am interested in within-state changes over time, which requires fairly large unemployed samples within each state and time period. Information on employment status collected through the CPS and reported in the ATUS also may not refer to the exact same month as the information collected on time-use, making it difficult to ensure that the time-use data consists exclusively of unemployed individuals.

Other relevant data available and used in both surveys include gender, age group (18-24, 25-34, 35-44, 45-54,55-64), marital status, education level, race (white, black, or other), body mass index (BMI)[3], as well as state of residence, year and month surveyed.

Information on the date of each state's ABP implementation and annual UI maximum allowable benefit levels were taken from the US Department of Labor website. Maximum allowable benefits are disaggregated by the maximum allowable amount per week (in US dollars) and the maximum number of weeks workers were entitled to receive benefits. These two values were multiplied to obtain the total allowable benefit level in a state in a given year.

[3] Body mass index data is missing for most unemployed non-high school graduate respondents in the ATUS and so it is not used in that analysis.

3.2 Empirical strategy

Neither the BRFSS, nor ATUS datasets contain information on whether unemployed individuals actually received UI. However assessing any direct effects of UI receipt on physical activity could produce biased results because of key differences between individuals who are eligible or ineligible, those who qualify or do not qualify, and those who ultimately receive or do not receive UI. Instead, I exploit the wide-variation within and across states over time in UI program legislation to estimate effects on physical activity in an intention-to-treat study design.

My primary approach tests the effect of ABP UI eligibility expansion on the probability of reporting physical activity. I use two main specifications that take advantage of both the state variation in the timing of ABP implementation and the subgroups exposed to and affected by the policy. First, I employ a difference-in-difference (DD) approach exploiting within-state variation in the timing of ABP implementation, and the fact that states introduced the policy at different points over the study period of 8 years (96 months). For this first specification, I restrict the sample to individuals with less than a high school education who became unemployed in the past year, as this group is the most likely to be affected by ABP. The treatment group is therefore recently unemployed individuals with less than a high school education in a state and time period with ABP policy in place, while the control group is recently unemployed individuals with less than a high school education when and where ABP has not yet been implemented. A key benefit of the DD approach is the ability to reduce selection problems inherent when comparing effects of non-random selection into treatment groups. While it is possible that there are changes in the composition of the unemployed population with less than a high school degree over time that could bias the results, it is improbable that such compositional changes would systematically correlate with ABP implementation over time, as there is considerable variation in the year and month of ABP implementation across states. The DD model specification is:

$$PA_{ijmt} = \alpha + \beta_1 ABP + \beta_2 UR_{jmt} + \beta_x X'_i + S_j + Y_t + M_m + \varepsilon_{ijmt}$$

where PA is a binary indicator of whether an individual reports physical activity, S are state fixed effects that control for time invariant state characteristics, Y are year fixed effects, M are month fixed effects which capture seasonal variations, UR are state monthly unemployment rates, and X is a vector of individual characteristics. ABP is an interaction between state and time where ABP=1 beginning in the month following a state's ABP implementation, allowing enough time for ABP eligibility to begin to be processed by state programs (Stettner et al., 2005). Using this specification, the coefficient on ABP is the average treatment effect of the policy on physically active leisure among unemployed individuals, identified for states that implement ABP at some point between 2003 and 2010.

One drawback of this approach is that any other policies or events that correlate with state ABP implementation may also produce an observable effect on the outcome variable. For example, since ABP policy was a requirement for states to receive ARRA UI modernization funds, it is possible that the ABP coefficient may pick up other elements of the ARRA program, such as the Supplemental Nutrition Assistance Program, support for Medicaid, or other measures that have broad effects on unemployed residents of a state, including but not limited to just the unemployed with less than a high school education (Modrek, 2013). Medicaid, for example, affects a wide swath of the US population, having almost 70 million enrollees in 2010—nearly 20 percent of the US population—which is larger than the total adult population that did not graduate high school (approximately 13 percent of the US population) and considerably larger than the unemployed non-high school graduate population (Kaiser Family Foundation, 2014, US Census Bureau, 2012). The DD approach could inadvertently pick up these contemporaneous effects and give an inaccurate estimate of the independent effect of ABP.

To address this, I use difference-in-difference-in-difference (DDD) models, where an additional control group assumed to be unaffected by ABP is included. I primarily use the recently unemployed who have graduated high school but have received no further education, and are unemployed in the same state and time period. This additional control group is arguably a reasonable comparator to the unemployed with less than a high school education in terms of education level, earnings potential, and eligibility for other social programs such as Medicaid as described above, but based on previous research on the effects of ABP, is not likely to benefit from ABP policy. I also run models that use other control groups—either all of the unemployed who have at least graduated high school or the unemployed who have completed some college. Both of these alternative control groups are unlikely to be affected by ABP, but are also unlikely to be affected by other social programs, and may be less comparable to the non-high school graduate demographic in terms of observable and unobservable characteristics in general. The DDD model specification is:

$$PA_{ijmt} = \alpha + \beta_1 ABP + \beta_2 UR_{jmt} + \beta_x X'_i + \beta_3 E_i + S_j + Y_t + M_m + \beta_4 (E_i * ABP) + (E_i * S_j) + (E_i * Y_t) + (E_i * M_m) + \varepsilon_{ijmt}$$

where E refers to the population eligible for ABP, in this case, the unemployed with less than a high school education. I use separate state, year, and month fixed effects for the eligible and non-eligible populations, which is a conservative modeling approach. The coefficient of interest in this model is ABP*E, which estimates the average effect of ABP policy on the target population.

I conduct many robustness checks, including inclusion of state-specific time trends and demographic interactions. I also run the analysis after collapsing the monthly data into state-year observations. As an additional sensitivity analysis, I test the effects of ABP on the natural log of height, for which there is no reason to expect that variations across states and repeated cross-sections will be associated with implementation of ABP policy in the short-term.

One of the key assumptions of DD and DDD is the common trend assumption. This stipulates that physical activity participation is essentially indistinguishable between the treatment and control groups prior to the implementation of ABP policy. If the probability of physical activity among the treatment and control groups had already been diverging prior to ABP implementation, the models may inaccurately attribute effects to the policy. This would be the case even after controlling for observable characteristics. With nearly half of the 50 states plus Washington DC implementing ABP at various points in time over the study period, it is difficult to visually confirm that there is no difference in trends prior to ABP. However, prior studies have utilized a test, where a bogus policy is created to see whether there is a statistically significant difference between treatment and control groups in the time period leading up to the policy (Gregg et al., 2012, Bertrand et al., 2004). I use two bogus policies: the 24 months or 36 months leading up to the actual implementation of ABP and test these for DD and DDD specifications, where a non-significant association validates the common trend assumption.

For the second approach, I exploit variation in the legislated maximum allowable state UI benefit level across states and time. Importantly, changes in state laws are presumed uncorrelated with state physical activity participation, demographics or other state characteristics. Prior research also suggests that changes in unemployment benefit generosity are unrelated to changes in the generosity of other state programs (Fishback et al, 2010) enabling identification of UI effects through changes in state UI maximum allowable benefits. The model specification, similar to the DD model above is:

$$PA_{ijmt} = \alpha + \beta_1 \ln(MAXUI_{jt}) + \beta_2 UR_{jmt} + \beta_x X'_i + S_j + Y_t + M_m + \varepsilon_{ijmt}$$

where MAXUI is the maximum allowable state UI benefit in a state and year. State fixed effects control for all time-invariant differences across states and use only within-state variation over time to identify the impact of benefits on physical activity. I use the natural log of benefit levels to calculate the effect of a proportional increase in maximum benefit levels. Because low educated job losers are unlikely to be eligible to receive the maximum allowable UI benefit level if they have poor work history, I run the analysis stratified by education.

All regressions are linear probability models with standard errors that are robust to unobserved heteroscedasticity, allowing for intragroup correlation by clustering at the state-year-month level for the ABP analysis (since ABP variation is at that level) and at the state-year level for the maximum allowable benefit analysis (since maximum UI benefit variation is at that level). Results are comparable using logistic regressions instead of linear regression.

4. RESULTS

4.1 Descriptive statistics

The BRFSS contains 9,062 18 to 65 year olds with less than a high school degree who had been unemployed for less than one year at the time of survey. 42.3 percent (n=3,833) were exposed to ABP policy. For the main DDD models, where unemployed high school graduates are the additional control group, 25,812 respondents were recently unemployed high school graduates with no further education, with 11,869 of those exposed to ABP but whose UI eligibility was not likely affected by the policy. The ATUS contains 1,178 18 to 65 year olds with less than a high school degree who report being unemployed; 40.8 percent (n=481) were exposed to ABP.

Table 1 contains weighted descriptive statistics from the BRFSS for recently unemployed individuals exposed to ABP and not exposed to ABP, disaggregated by those with less than a high school degree and those with a high school degree but no further education (the control group in the main DDD). The percentages of ABP-exposed unemployed with less than a high school education (the treatment group) that were male (61.9%), non-white (44.0%), or unmarried (62.3%) are slightly higher than the respective percentages for the control groups in the main DDD analysis (i.e. the unemployed with less than a high school education but not exposed to ABP, unemployed high school graduates exposed to but not affected by ABP, and unemployed high school graduates not exposed to or affected by ABP).

ABP-exposed and non-exposed unemployed non-high school graduate respondents in the ATUS have similar demographic characteristics to those in the BRFSS. 18.4% of unemployed ABP-exposed non-high school graduate respondents reported any minutes of all sports activities, 5.0% reported any minutes of walking, 1.7% reported any minutes of running and 11.1% reported any minutes of job search; 15.0%, 4.7%, 0.8% and 13.8% of the unemployed non-ABP exposed non-high school graduate control group reported participation in these activities, respectively.

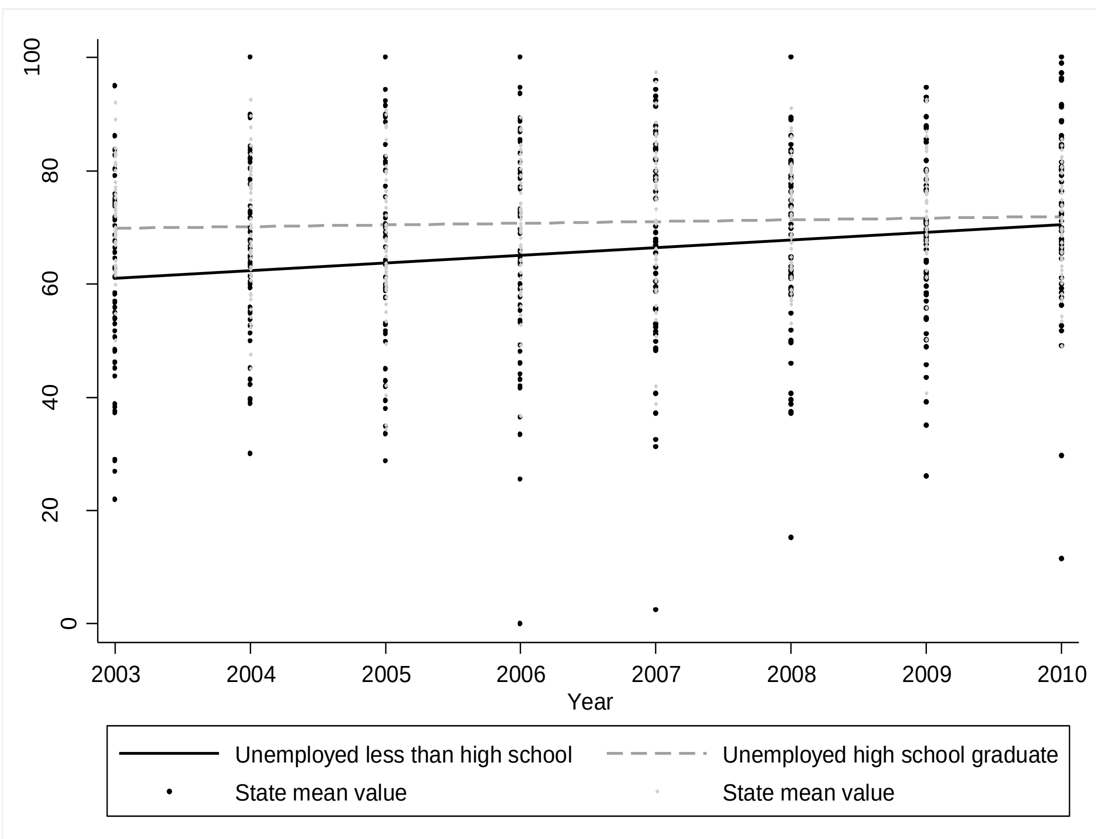
Table 1. Descriptive statistics of ABP vs non-ABP exposed unemployed individuals in the BRFSS, sample weighted

			Male	Age	Married	White	Black	Asian	Other race	Leisure physical activity	BMI	Height
Less than high school	No ABP	Mean	0.58	33.8	0.48	0.65	0.18	0.01	0.16	0.64	27.53	4.20
		SD	0.49	12.6	0.50	0.48	0.38	0.11	0.36	0.48	5.90	0.11
	ABP	Mean	0.62	32.8	0.38	0.56	0.24	0.01	0.19	0.66	27.13	4.20
		SD	0.49	13.0	0.48	0.50	0.43	0.11	0.39	0.47	5.98	0.08
	Total	Mean	0.60	33.4	0.44	0.62	0.20	0.01	0.17	0.65	27.39	4.20
		SD	0.49	12.7	0.50	0.49	0.40	0.11	0.37	0.48	5.93	0.10
High school graduates	No ABP	Mean	0.59	33.2	0.40	0.66	0.21	0.02	0.12	0.72	27.35	4.21
		SD	0.49	12.8	0.49	0.47	0.41	0.13	0.32	0.45	5.89	0.08
	ABP	Mean	0.59	34.1	0.42	0.68	0.22	0.01	0.09	0.71	27.41	4.21
		SD	0.49	13.1	0.49	0.47	0.41	0.11	0.29	0.45	5.94	0.06
	Total	Mean	0.59	33.6	0.41	0.67	0.21	0.02	0.11	0.72	27.37	4.21
		SD	0.49	12.9	0.49	0.47	0.41	0.12	0.31	0.45	5.91	0.07

Note: SD=Standard deviation; height is reported as the natural log of inches.

Fitted lines in Figure 3 reveal that the share of the unemployed with less than a high school education who reported physically active leisure in the BRFSS increased between 2003 and 2010 (solid line), but that there were no changes of note among unemployed high school graduates (dotted line). While this increase in physical activity participation among non-high school graduates coincides with the incremental increase over time in the number of states implementing ABP, it is not possible to attribute these changes to ABP based on this Figure, since I cannot establish whether increased physical activity is occurring within states as they implement ABP, or whether something else entirely is driving the change.

Figure 3. Fitted lines of the percentage of unemployed reporting physically active leisure in the BRFSS, by US state, 2003 to 2010



4.2 Main results

Before proceeding with the DD and DDD models, I check to ensure that the common trend assumption holds using the BRFSS data. To do this, I run the full DD and DDD model specifications, but replace ABP with bogus policies covering the 24 or 36 months prior to ABP implementation (Table 2, Columns 1 and 2). In all instances, unemployed non-high school graduates are not predicted to have statistically significant differences in their probability of reporting physical activity leading up to ABP implementation relative to the control groups. This provides confirmatory evidence that the treatment and control groups had non-diverging physically active leisure trends prior to ABP.

Table 2. Testing common trend assumptions using bogus policies 24 and 36 months prior to ABP implementation, BRFSS

	Leisure physical activity		**Leisure physical activity**	
	DD	**DDD**	**DD**	**DDD**
	(1)	(2)	(3)	(4)
	Using 24 months prior as the common trend test			
ABP 24 months prior	-0.0327	0.00599	0.0277	0.0336**
	(0.0325)	(0.0268)	(0.0315)	(0.0171)
ABP 24 months prior* Less than high school		-0.0301		-0.0109
		(0.0514)		(0.0315)
	Using 36 months prior as the common trend test			
ABP 36 months prior	0.00527	0.0124	0.0161	0.0277*
	(0.0317)	(0.0251)	(0.0286)	(0.0160)
ABP 36 months prior* Less than high school		0.0248		-0.0155
		(0.0492)		(0.0291)

Note: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. Models include gender, age group, marital status, education level, race (white, black, or other), state, year and month, as in other DD and DDD specifications. If a coefficient is statistically significant it indicates that there was a trend in the outcome variable prior to ABP policy implementation. Data used for columns 1 and 2 is at the individual level; data used for columns 3 and 4 are collapsed to the state-year level.

DD models reveal the average treatment effect of ABP among the unemployed with less than a high school education based on within-state variation in the timing of ABP (Table 3). The basic model including no controls other than state, year and month fixed effects finds that ABP implementation is associated with an increased probability of physical activity participation in the BRFSS (Beta=0.0798, p<0.1, Column 1); controlling for state monthly unemployment rates slightly increases the magnitude and preciseness of the estimate (Beta=0.0851, p<0.05, Column 2). After controlling for all covariates, ABP policy implementation remains associated with increased probability of engaging in physical activity (Beta=0.085, p<0.05, Column 3). The effect of ABP on the natural log of height is not statistically significant in any models (Columns 5-8).

Table 3. Estimates of the effect of ABP on physical activity and the natural log of height, BRFSS

	Leisure physical activity				**Natural log of height (in inches)**			
	DD			**DDD**	**DD**			**DDD**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No controls	UR control only	Fully adjusted	Fully adjusted	No controls	UR control only	Fully adjusted	Fully adjusted
ABP	0.0798*	0.0851**	0.0850**	-0.0103	-0.00957	-0.00904	-0.00787	-0.00239
	(0.041)	(0.0413)	(0.041)	(0.0268)	(0.00911)	(0.00923)	(0.00792)	(0.00364)
ABP*Less than high school				0.0921*				-0.00514
				(0.0472)				(0.00835)
Observations	9,048	9,048	8,854	34,355	9,062	9,062	8,866	34,399

Note: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. Fully adjusted models include gender, age group, marital status, education level, race (white, black, or other), state, year and month.

As discussed, DD models may produce biased estimates of the effect of ABP if some other policy or event that influences physical activity coincides with ABP implementation. Using the DDD specification, I find again that unemployed non-high school graduates exposed to ABP are at a higher probability of reporting physical activity (Column 4). The magnitude of the effect is 0.0921 ($p<0.1$), comparable in both size and preciseness to the DD estimate. There is no discernible effect of ABP on the probability of physical activity among the high school graduate control group based on the non-significant main effect of ABP. There are also no effects on the log of height (Columns 5-8).

I run many additional models to test the robustness of these results (Table 4). First, to ensure that the DDD estimate is not biased because of the choice of control group, I run alternate models where the control group is the entire unemployed population that has at least graduated from high school, or where the control group is the unemployed population that has completed some college education only (Columns 2 and 3, respectively). In both cases, ABP policy is again associated with higher probability of physical activity among those with less than a high school degree at $p<0.05$ (Beta for all unemployed model=0.0912; Beta for some college unemployed model=0.103). I next add state-specific linear time trends (Column 4) and state-specific quadratic time trends to the main DDD model (Column 5), with negligible effect on the ABP coefficient.

Table 4. Estimates of the effect of ABP on physical activity, robustness checks I, BRFSS

	Testing different control groups for DDD			Testing inclusion of state trends	
	Main DDD (Control: high school graduates)	Alternative Control: All unemployed that have at least graduated high school	Alternative Control: Some college unemployed	State linear time trends	State quadratic time trends
	(1)	(2)	(3)	(4)	(5)
ABP	-0.0103	-0.011	-0.0216	-0.0285	-0.0284
	(0.0268)	(0.0155)	(0.0238)	(0.0311)	(0.0307)
ABP*Less than high school	0.0921*	0.0912**	0.103**	0.0864*	0.0840*
	(0.0472)	(0.0427)	(0.0479)	(0.0466)	(0.0464)
Observations	34,355	69,016	27,943	34,316	34,316

Note: *** p<0.01, ** p<0.05, * p<0.1; robust standard errors in parentheses. Models contain all control variables that are included in other DDD models.

Analogous to the Ruhm (2005) study that finds effects of unemployment rates on physical activity, I add demographic interactions age*sex, age*race, sex*race, sex*marriage, and sex*education to the DDD model (Table 5, Column 1) with no material differences in the results.

Table 5. Estimates of the effect of ABP on physical activity, robustness checks II, BRFSS

	Interactions between all demographic variables included	BMI as control	Collapsing to weighted state-years
	(1)	(2)	(3)
ABP	-0.0115	-0.00706	-0.0457*
	(0.0265)	(0.0272)	(0.0243)
ABP*Less than high school	0.0920*	0.0813*	0.0864*
	(0.0471)	(0.0481)	(0.0450)
Body mass index		-0.00333***	
		(0.00088)	
Observations	34,355	33,042	814

Note: *** p<0.01, ** p<0.05, * p<0.1; robust standard errors in parentheses. Models contain all control variables that are included in other DDD models.

To confirm that effects of ABP are not due to differences in BMI across cohorts, I control for BMI in the original DDD model (Column 2). While higher BMI is associated with a statistically lower probability of engaging in physical activity ($p<0.01$), the positive effect of ABP implementation on physical activity among non-high school graduates remains (Beta=0.0813, $p<0.1$). Lastly, because of the potential for bias due to small numbers of observations at the state-year-month level, I collapse the main DDD individual level data into state-year level observations. The collapsed state-year data pass the common trend tests for unemployed non-high school graduates (Table 2, Columns 3 and 4) and the results remain significant for the DDD (Beta=0.0864; $p<0.1$) (Column 3).

Next, I replicate the main DD and DDD model specifications using the ATUS unemployed sample. I find that the binary outcome variables of whether any minutes were spent walking, running, or engaging in a sporting activity pass both the 24 months and 36 months prior common trend tests for both the DD and DDD model specifications, however the any minutes spent job searching outcome variable does not (Table 6, Columns 1-8). In the 24 and 36 months leading up to ABP, there is a statistically significant lower probability of unemployed non-high school graduates spending any time job searching (Columns 7 and 8); this prohibits further analysis to compare time-spent searching for work with time spent engaging in physical activities.

Table 6. Testing common trend assumptions using bogus policies 24 and 36 months prior to ABP implementation, ATUS

	Any walking		Any running		Any sporting activity		Any job search	
	DD	DDD	DD	DDD	DD	DD D	DD	DDD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Using 24 months prior as the common trend test								
ABP 24 months prior	-0.0000714	-0.00696	-0.00453	-0.00584	0.000644	0.0222	-0.0890**	0.0202
	(0.0434)	(0.0196)	(.00558)	(.00859)	(0.0614)	(0.038)	(0.0432)	(0.0385)
ABP 24 months prior*Less than high school		0.00706		0.00141		-0.0131		-0.116**
		(0.0462)		(0.0102)		(0.0693)		(0.0572)
Using 36 months prior as the common trend test								
ABP 36 months prior	0.00243	-0.00354	-0.0007	-0.00084	-0.0122	0.0183	-0.0990**	0.0234
	(0.0339)	(0.0172)	(0.00815)	(0.00899)	(0.0482)	(0.0312)	(0.0443)	(0.032)
ABP 36 months prior*Less than high school		0.00637		0.00166		-0.0112		-0.126**
		(0.0379)		(-0.012)		(0.0557)		(0.0527)

Note: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. Models include gender, age group, marital status, education level, race (white, black, or other), state, year and month, as in other DD and DDD specifications. If a coefficient is statistically significant it indicates that there was a trend in the outcome variable prior to ABP policy implementation. All data is at the individual level.

Nevertheless, using the DD approach, I find that based on the point estimates, ABP is associated with higher probability of reporting any walking in the ATUS (Table 7, Beta=0.0751, Column 1); the effect size is comparable to those found using the BRFSS. However, perhaps due to the relatively small number of unemployed respondents that did not complete high school (n=848), the estimated confidence intervals are wide. Due to this potential small sample size issue, for the DDD I use all unemployed who have *at least* finished high school as the control group, rather than just the unemployed who have only graduated high school; this increases the sample size to n=4,306 unemployed people. I find that unemployed non-high school graduates exposed to ABP have an increased probability of spending any time walking (Beta=0.107; p<0.1, Column 2). I do not find any statistically significant effects of ABP on the probability of engaging in any sporting activities overall or on running (Columns 3 and 4).

Table 7. Estimates of the effect of ABP on any time spent walking, running, and all sports participation, ATUS

	DD	**DDD**		
	(1)	(2)	(3)	(4)
VARIABLES	Any walking	Any walking	Any running	Any sports participation
ABP	0.0751	-0.0364	-0.0111	-0.0449
	(0.0534)	(0.024)	(0.0115)	(0.0404)
ABP*Less than high school		0.107*	0.0129	0.110
		(0.0573)	(0.019)	(0.0878)
Observations	848	4,306	4,306	4,306

Note: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. Models contain all control variables that are included in other DD and DDD models.

Lastly, I investigate whether changes in state UI maximum allowable benefit levels have an effect on physical activity. I find that within-state increases in maximum UI benefits are associated with a higher probability of reporting physical activity in the BRFSS among unemployed high school graduates and unemployed with some college, and lower probability among non-high school graduates and college graduates, though confidence intervals are wide in all instances (Table 8). This is believable particularly in the case of the latter two groups, as non-high school graduates are unlikely to have sufficient work history to qualify for maximum UI benefit levels, as mentioned, whereas college graduates may not benefit substantially from relatively small changes in maximum UI benefit levels if these replace trivial shares of their prior wages. However combining high school graduates and some college into a single group (whilst controlling for educational attainment), I find a statistically significant higher probability of physically active leisure in Table 8 Column 5 (Beta=0.0282, p<0.1). This is corroborated using the ATUS, where this same demographic is predicted to have greater probability of any participation in sporting activities as maximum UI benefits increase (Beta=0.0628, p<0.05). I do not find statistically significant effects of maximum allowable UI for walking or running in the ATUS (not shown).

Table 8. Estimates of the effect of maximum allowable state UI benefit levels on physical activity (BRFSS) and any sports participation (ATUS)

	BRFSS					ATUS
	Physically active leisure					Any participation in sporting activities
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	No high school	High school only	Some college	College graduate	High school and some college	High school and some college
Maximum UI benefit (natural log)	-0.00259	0.0176	0.0424	-0.0153	0.0282*	0.0628**
	(0.0306)	(0.0219)	(0.0319)	(0.0324)	(0.0157)	(0.0298)
Observations	8,843	25,473	19,060	15,547	44,533	2,319

Note: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. Models contain all control variables that are included in DD models.

5. DISCUSSION

Unemployment benefits have many rationales and effects, but to date, no research has examined whether they cause changes in time-consuming health behaviors, such as exercise. Although the image of an unemployment benefit-receiving ‘couch potato’ may be ubiquitous, this study suggests that UI recipients are likely to spend some of their newfound leisure time participating in physical activity. Analysis using two separate datasets and two distinct methodological approaches produces consistent estimates that are of the same sign and statistical significance.

Point estimates suggest ABP implementation resulted in an 8-10 percentage point increase in the probability of physical activity. While this implies that the effect of the ABP treatment on the treated population – actual UI receivers – is quite large, the wide confidence intervals prohibit definitive conclusions regarding the precise magnitude of effects; 95% confidence intervals from the main DDD model, for example, indicate that the increased probability of reporting physically active leisure following ABP adoption ranges from near 0 to 18.5%. This lack of precision may be due in part to small numbers of individuals in some state-year-month cohorts, resulting in instances where there is either 0 or 100% participation in physical activity in an entire state-year-month. However as noted, the effect remains positive and significant even after aggregating the data to the state-year level (Table 5, Column 7) (Appendix Figure 1). The imprecise estimates may also be due to the intention-to-treat study design, which as noted, is used to account for possible selection into UI. While unlikely, the estimated effect size could also be large if ABP leads to changes in social norms regarding physical activity, which might cause spill-over effects among non-UI recipients within the same low-educated demographic (Berkman and Glass 2000).

Nevertheless, the finding of an analogous relationship using state UI maximum allowable benefit levels seems to at least support the estimated direction of effects. Using maximum allowable UI benefit levels, the models suggest that a 10% increase in maximum allowable benefits increases the probability of physically active leisure by 0.3 percentage points in the BRFSS and the probability of any sports participation by 0.6 percentage points in the ATUS. Between 2003 and 2010, the median annual change in maximum allowable benefit levels was just -2.8%, suggesting in practice that variations in maximum benefit levels have a very small absolute effect on physical activity. The magnitude of decline in maximum allowable benefits in the median state-year would imply a reduction in the probability of reporting physically active leisure of 0.08 percentage points in the BRFSS and 0.18 percentage points in the ATUS.

The notion that incremental changes in state maximum benefit levels might have a smaller absolute effect on physical activity participation than expansions of UI eligibility is not surprising, as the monetary gains afforded to new UI recipients is quite substantial in comparison to receiving no UI benefits at all. Additionally, for low wage earners impacted by ABP, the amount of labor hours needed during their base period to have high enough earnings to qualify for UI is comparatively greater than for higher wage earners. This means that for low wage individuals, leisure would have been consumed at a premium while employed. The perceived decrease in the cost of leisure associated with joblessness and UI would appear substantial to such an individual and could explain the relatively large estimated effects associated with ABP.

The main underlying mechanism linking UI to physical activity may be either that (1) individuals receiving UI benefits feel less pressure to search for work, which gives them additional time that can be spent engaging in physical activities or (2) individuals receiving UI benefits are able to afford costly physical activities, such as gym memberships. I am unable to explore whether individuals substitute physical activity in lieu of job search in the main analysis because the outcome variable of whether an individual engaged in any job search does not pass the common trend test: in the 24 and 36 months prior to ABP implementation, non-high school graduates were already statistically less likely than the control groups to spend any time searching for a job. Nevertheless, given the finding from the ATUS that there is greater probability of time spent walking but not of other, potentially more expensive sporting activities, the former explanation appears most likely.

There are a number of limitations to this analysis. As noted, using the BRFSS or ATUS I am unable to identify whether individuals actually receive UI. Therefore, I cannot observe whether there are changes in UI take-up directly attributable to ABP or whether there are variations in the amount of benefits actually received when maximum allowable UI benefits are altered. In the case of ABP, I rely on the existing literature to infer the effects on UI take-up among a nationally representative sample. The results presented would be biased if take-up patterns differed substantially among the BRFSS or ATUS survey samples, although this seems unlikely given the consistency in the estimates across the two datasets.

Additionally, the BRFSS question on leisure time physical activity, though commonly used in the literature, is vague and may capture various behaviors or suffer from measurement error. However, the alternative, to fit individuals with accelerometers, is not feasible on this scale. It is also reassuring that the more objective data from the ATUS provide confirmatory results, despite the notably smaller sample size. Other datasets such as the National Health and Nutrition Examination Survey (NHANES) that have more detailed data on physical activity have too few observations in each state and time period to conduct this sort of analysis.

Finally, I am unable to observe changes in exercise within unemployed individuals over time due to the non-panel nature of both the BRFSS and ATUS surveys. Future research should assess whether leisure-time subsidies including, but not limited to UI, affect more objective measures of physical activity among unemployed individuals with otherwise poor access to financial resources, as well as whether such leisure time subsidies have an effect on objective health outcome measures.

Physical inactivity is an important determinant of poor health. The finding that UI increases physically active leisure is consistent with theory that reductions in the opportunity cost of time will lead individuals to engage in time-consuming leisure activities, such as exercise. Possible long-run health effects of leisure time physical activity include better weight management, lower risk of chronic disease, and reduced risk of death (Warburton et al., 2006, Ruhm, 2005, Chaput et al., 2011, Abu-Omar and

Rutten, 2008, Lindstrom et al., 2001, Clays et al., 2014, Johnsen et al., 2013, Naci and Ioannidis, 2013). UI also provides workers with an opportunity to increase their health capital during periods of unemployment, which has been found to contribute to increases in worker productivity upon return to employment (Acemoglu, 2001, Acemoglu and Shimer, 2000, Brown and Kaufold, 1988). This is consistent with evidence that job applicants who engage in leisure sports activities have higher call-back rates from prospective employers as well as higher wages and earnings (Rooth, 2011, Lechner, 2009). Overall, the evidence suggests that UI can prepare the unemployed to re-enter the workforce by increasing their health capital and ultimately contribute to better health.

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Appendix Figure 1. Distribution of physically active leisure at the state-year level among non-high school graduates, BRFSS