The Dynamic Effects of Obesity on the Wages of Young Workers*

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October, 2012

Abstract

This paper considers the effects of an individual's body mass on his or her wages in the years following labor market entry. In addition to considering current body mass, past body mass and wages are allowed to affect current wages. Furthermore, the preferred dynamic panel specifications remove individual fixed effects and account for the endogeneity of body mass. The estimation results suggest that being morbidly obese in the previous year has a large negative effect on the current wages of white men. White women face a penalty for being overweight at all in the previous year, with an additional penalty for past morbid obesity. The only evidence consistent with an effect of current body mass suggests a penalty for women who exceed the threshold for morbid obesity.

Keywords: BMI, obesity, wages, dynamic panel data model

JEL codes: J31, J7

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This paper considers the effects of an individual's past and current body mass on wages in the years following labor market entry. Panel data from the National Longitudinal Survey of Youth 1997 (NLSY97) are used to estimate an autoregressive model of wages in which measures of current and lagged body mass are allowed to be endogenous. This approach removes bias due to individual fixed effects, while still allowing a worker's history in the labor market to affect wages. The panel data also provide a natural set of instrumental variables that can be used to correct for any remaining endogeneity.

Previous work on the effects of body mass on wages has repeatedly discussed the potential for bias due to correlation with unobserved individual-specific effects, reverse causality or other forms of endogeneity. Cawley (2004) provides a thorough discussion of various ways in which body mass might be correlated with the errors in a wage regression. He then estimates separate regressions using each of the three main approaches to endogeneity in this literature: He uses a seven-year lag of either body mass index (BMI), weight or BMI categories in place of the current measure to remove possible bias from reverse causality. He considers effects of current body mass using fixed-effects regressions. And he uses a sibling's body mass as an instrumental variable without controlling for individual fixed effects. Overall, he finds that only white women face penalties for high body weight that cannot be attributed to unobserved heterogeneity.

Most other papers in the literature have focused on either individual fixed effects [e.g., Han, et al (2008)], or other sources of endogeneity [e.g., Kline and Tobias (2008), or Gortmaker, et al (1993)], but not both.[1] Two papers that do account for multiple sources of bias are Averett and Korenmann (1996), who remove family-specific effects by differencing between siblings and reverse causality by using seven-year lags of BMI; and Behrman and Rosenzweig (2001) who exploit differences between female twins in combination with an instrumental variables approach. One contribution of this paper, therefore, is that it simultaneously accounts for individual fixed effects and uses instrumental variables to allow for other sources of endogeneity.

Another contribution of this paper is to consider effects of both current and past body mass on wages.[2] Previous work that considered effects of past levels of body mass used long lags (e.g., seven years) in place of current values.[3] The assumption that a history of being overweight or obese affects wages is implicit in this approach. In contrast, previous work that used fixed-effects or first-differences implicitly assumed that the wage penalty associated with obesity is the same whether the worker recently became obese, or has been obese her entire career. The preferred specifications in this paper relax the assumptions made in previous work, using the dynamic panel-data estimator developed by Holtz-Eakin, et al (1988) and Arellano and Bond (1991) to remove individual fixed effects without ignoring past

[1] Kline and Tobias (2008), who use an IV approach, argue that controlling for fixed effects is unnecessary if the data contain a rich set of individual and family characteristics. Han, et al (2008), who use fixed effects, argue that the most common instrumental variables tend to be very weak once fixed effects are controlled for.

[2] Chen (2012) is the only other paper to allow both past and current BMI to affect wages. Examining respondents in their 30s, she considers effects of both current BMI and BMI 10 years earlier.

[3] Examples include Gortmaker, et al (1993), Sargent and Blanchflower (1994), and Gregory and Ruhm (2009).

levels of body mass.[4]

Finally, using data from the NLSY97 (instead of the more commonly used NLSY79) allows this paper to focus on workers in the first several years of their full-time work life. These years are important for the purposes of this study because wage growth is higher earlier in careers, and changes in jobs and occupations are more important.[5] Effects on wages or job-finding due to employer distaste or stereotyping could have large, lasting effects. The market also knows less about workers early in their careers, implying that any signal inferred from body mass would have a larger effect for younger than for older workers.[6] For these reasons, it seems more likely that wage regressions that use differencing to remove fixed effects will capture effects of discrimination as they unfold in a sample of younger workers than in a sample of older workers. Differencing the wages of older workers might instead simply remove the accumulated effects of past discrimination.

The estimation results suggest that past levels of body mass affect current wages, and they support the inclusion of an autoregressive term in wage equations. Being morbidly obese in the previous year lowers the wages of white men by 16% relative to normal-weight peers. White women face a penalty for being overweight in the previous year of roughly 10%, and an

[4] This is the first paper to consider effects of body mass on wages in a dynamic panel data model, but similar estimation has been used to study related topics. Michaud and van Soest (2008) study the relationship between health and wealth among older couples in the Health and Retirement Study. Goldman, et al (2010) study effects of food prices on body weight in the HRS; and Ng, et al (2010) study affects of diet and other behavior on body weight using a sample of men in China.

[5] See, Murphy and Welch (1992), Topel and Ward (1992), and Neal (1999) among others.

[6] See Altonji and Pierret (2001) for a discussion of statitical discrimination in the presence of employer learning. Lange (2007) finds that the market quickly learns about the ability of workers, with expectation errors falling by half in the first three years.

additional penalty for past body mass above the cutoff for morbid obesity of up to 15%. After conditioning on body mass in the previous year, the only penalty I find for current body mass is for women above the morbid obesity cutoff.

The next section briefly discusses models of wages and body mass, building up to the preferred dynamic panel specification. Section 2 discusses the data, and Section 3 goes into greater detail about the estimation. Section 4 presents results, while also considering alternative specifications of body mass, various robustness tests, and tests for sample selection bias. Section 5 concludes.

1 Empirical Models of Body Mass and Wages

Following much of the recent literature on effects of body mass on wages, our first attempt at specifying a wage regression to measure these effects might take the form

$$w_{it} = X_{it}\beta + BMI_{it}\phi + \nu_{it}, \tag{1}$$

where w_{it} is the log of person i's wage in period t, X_{it} is a vector of observable variables and BMI_{it} is a vector that expresses body mass using a series of dummy variables or a polynomial. The error term, ν_{it}, is allowed to contain an individual fixed effect:

$$\nu_{it} = \alpha_i + \varepsilon_{it}.$$

The vector BMI_{it} is potentially correlated with both the fixed effect, α_i, and the time-specific part of the error, ε_{it}, leading to possible endogeneity.[7]

Previous work has used individual fixed effects or first differences to eliminate bias caused by α_i in equation (1), but potential correlation between BMI_{it} and ε_{it} then remains.[8] The use of instrumental variables could correct for bias caused by the correlation of BMI_{it} and ε_{it}, but all of the instruments used so far in the literature are likely to be correlated with the fixed effect.[9] Combining the two approaches could eliminate both sources of bias, but would require a valid instrument for changes in BMI_{it}. Fortunately, the panel structure of the data provide such instruments.

If a history of being overweight or obese can affect wages, which is suggested by some previous work, regressions like equation (1) should be modified to allow effects of both current and lagged body mass. Since lagged body mass might be correlated with lagged wages, which are themselves likely correlated with current wages, lagged wages should be included as well. Using a single lag of both wage and body mass results in a simple autoregressive specification of the wage equation:

$$w_{it} = \gamma w_{it-1} + X_{it}\beta + BMI_{it}\phi + BMI_{it-1}\phi_1 + \alpha_i + \varepsilon_{it}. \qquad (2)$$

[7] See Cawley (2004) for a very thorough discussion of how and why body mass might be correlated with the error term in a wage regression.

[8] Examples include Cawley (2004), Baum and Ford (2004) and Han, *et al* (2008).

[9] Examples include Berhman and Rosenzweig (2001) using lagged BMI as an instrument, and Cawley (2004), Gregory and Ruhm (2009) and others using BMI of a family member.

Han, et al (2008) point out that these instruments are very weak once fixed effects are removed, and my preliminary estimation suggests the same.

Both BMI_{it} and BMI_{it-1} are potentially correlated with α_i, as is w_{it-1}.[10] As before, BMI_{it} might be correlated with ε_{it}. Although the assumption can be relaxed and will be tested empirically, assume for now that there is no serial correlation in ε.

Dynamic panel data models like equation (2) can be estimated using the method developed in Holtz-Eakin, *et al* (1988) and Arellano and Bond (1991) (HENR and AB in what follows) for use in short, unbalanced panels.[11,12] The first step in this estimation is to difference equation (2) to eliminate the fixed effect:

$$\Delta w_{it} = \gamma \Delta w_{it-1} + \Delta X_{it}\beta + \Delta BMI_{it}\phi + \Delta BMI_{it-1}\phi_1 + \Delta\varepsilon_{it}. \quad (3)$$

After differencing, ΔBMI_{it} and ΔBMI_{it-1} are both correlated with the error term due to the correlation of BMI_{it} with ε_{it}, and BMI_{it-1} with ε_{it-1}. Furthermore, the lagged wage difference, Δw_{it-1}, is correlated with the error because $\Delta\varepsilon_{it}$ contains ε_{it-1}.

Fortunately, the data provide us with further lags of both wages and body mass. Under the assumption of no serial correlation in ε, w_{it-2} is a valid instrument for Δw_{it-1}. It is not correlated with ε_{it} or ε_{it-1}, but is correlated with Δw_{it-1}. The difference GMM estimator of HENR and AB also includes further lags of the wage, where available, as additional instruments

[10] Further lags of BMI or w can also be included. This will be discussed further in Section 3. For now, one lag is sufficient to explain the model,

[11] See Arellano and Honoré (2001) or Bond (2002) for reviews of this literature. Additionally, in the process of documenting his STATA .ado file, XTABOND2, Roodman (2006) provides a useful introduction to these estimators.

[12] The length of the panel is relevant because a fixed effects estimator in an autoregressive model is biased in short panels.

to improve efficiency. Similarly, BMI_{it-2} should be a valid instrument for both ΔBMI_{it}, and ΔBMI_{it-1}. One more lag of BMI is needed for identification, but further lags are already included as instruments.

There are a few pitfalls we must be aware of when estimating a linear dynamic panel data model. One alluded to above is that the assumption of no serial correlation in ε might be violated in practice. Cawley (2004) mentions this as a possibility when discussing work that used a seven-year lag in body mass. Fortunately, we can test for this sort of serial correlation, and adapt our instruments in response. These tests and other common problems with differenced GMM estimation will be discussed in Section 3, which goes into more detail about the estimation in this paper.

2 Data

This paper uses data from the 1997 through 2009 waves of the National Longitudinal Survey of Youth 97 (NLSY97). The individuals in the sample were between 12 and 16 years of age in 1996. They are between 24 and 30 when interviewed in 2009. The data also contain detailed information on labor market history, demographics and other common control variables.

The NLSY97 has a few advantages over the 1979 cohort of the NLSY for the purposes of this paper. First of all, the '97 cohort was young enough at their first interview that we can observe nearly all of the sample as they begin their careers, and they were asked about height and weight in every year of the survey. In contrast, respondents in NLSY79 were as old as 22 when first interviewed, and they were not asked about weight in '79, '80, 83, '84 or '87. As a result, respondents in the NLSY79 are between 25 and 33 years old by 1990, the first year that BMI can be observed for three years in a row.[13] 1994 is then the last year that the '79 cohort was interviewed annually.

In what follows, attention is limited to white men and women out of a concern for sample size. Over twice as many respondents identify as white than as black, which is the second largest racial or ethnic group. Since the preferred estimation requires three years in a row of valid wage observations, the number of observations is reduced more for minorities and women than for white men'

The rest of this section discusses information on body mass in the data, followed by a brief description of the estimation sample. A more detailed discussion of the sample's selection is left to a data appendix.

2.1 Body Mass and Measurement Error

The data include self-reported height and weight in each year, which allows the creation of BMI for each individual and year. Self-reported BMI introduces an obvious source of measurement error that has been and continues to be a problem in this literature. Even worse, roughly 10% of person/year observations in the estimation sample come from telephone interviews, which might worsen the problem of misreporting relative to in-person interviews.[14]

[13] If fixed effects were removed using forward orthogonal deviations instead of first differences, the first year we could use would be 1986, but respondents would still be as old as 29 by then.

[14] White women in particular appear to lose weight when interviewed over the phone instead of in person. The NLSY79 data used in a number of previous studies also has a mix of in-person and telephone interviews.

In all specifications that include current or lagged BMI variables, I include corresponding dummy variables for the interview being conducted over the telephone. Additionally, estimating all regressions separately for each gender and race group provides some allowance for differences in reporting by group, as well as differences in returns to body mass.

Bound and Krueger (1991) point out that first differencing can reduce problems with measurement error that is caused by systematic misreporting. Working in the context of earnings data, they show that measurement that is positively correlated between years can cancel out in first differences. Evidence from NHANES and simple comparisons of phone versus in-person interviews in the NLSY97 point more to systematic under-reporting than to classical measurement error, and this misreporting is more pronounced for white women than for men. If most of the measurement error in BMI is due to under-reporting that is correlated from year to year, changes in reported BMI should be largely driven by changes in actual BMI.

I do not adjust self-reported BMI based on the relationships between reported and actual height and weight in NHANES data, as Cawley (2004) and others do. There is no reason to believe that the assumptions required to treat NHANES data as validation data for any NLSY cohort are valid.[15] Furthermore, most of any rescaling is likely to cancel out in first differences. As an alternative, I simply examine robustness of the estimates to shifting

[15] The critical assumption is that the distribution of the actual measure conditional on the reported is the same in both data sets. As Han, et al (2009) point out, this is unlikely when respondents in one survey expect to be measure, but respondents in the other do not. Furthermore, unless being interviewed over the phone actually does cause weight loss, a quick look at either NLSY cohort demonstrates that changing the context of the interview violates this assumption.

the cutoffs for BMI categories.

2.2 The Estimation Sample

The data appendix describes the selection of the estimation sample in greater detail. For now, it is important to note that only jobs following full-time labor market entry are used in the estimation that follows. Entry is defined as the first two consecutive years in which the individual works full time and is employed for at least 75% of the year. The sample excludes respondents who were in the military, as well as observations for women who reported a pregnancy since the last interview. Limiting attention to observations that can be use as time t, $t-1$, or $t-2$ in equation (3) leaves 9,097 for white men, and 5,453 for white women.

Table 1 presents basic summary statistics for each group. (Appendix Table A1 presents a more complete set of summary statistics.) The dependent variable in all of the regressions that follow is the natural log of hourly wage. Average log wage is roughly 2.3 for men and 2.2 for women, which translates to hourly wages of roughly 10 and 8.8, respectively.

Average BMI exceeds 25, which is the threshold for being overweight, for both men and women. Less than 2% of white men are underweight (BMI ≤ 18.5), while 4.2% of white women are. Over 57% of white men and nearly 42% of white women are overweight or heavier. 22% of white men and 20% of white women are obese or heavier (BMI ≥ 30). And almost 8% of men and over 10% of women qualify as morbidly obese (BMI ≥ 35).

The average man or woman in the sample is almost 24 years old. Average years in the labor market is roughly 4.5 for white men and four for women. White men have accumulated an average of 4.2 years of actual experience, while white women have accumulated 3.8 years of experience. In 2009, the average man has been in the labor market for seven years and has accumulated 6.4 years of actual work experience. The average woman has been in the labor market for 6.2 years, accumulating 5.6 years of actual experience.

3 Estimation

Recall that the differenced equation we're interested in takes the form

$$\Delta w_{it} = \gamma \Delta w_{it-1} + \Delta X_{it}\beta + \Delta BMI_{it}\phi + \Delta BMI_{it-1}\phi_1 + \Delta\varepsilon_{it}. \tag{3}$$

As mentioned in the first section, the efficient GMM estimation developed by HENR and AB uses the second and all later lags as instruments for Δw_{it-1} and any other endogenous variables. All of these GMM instruments enter separately for each year. At $t = 3$, w_{i1} is used as an instrument for Δw_{i2}. At $t = 4$, w_{i2} and w_{i1} are available as instruments, and so on. This allows the use of further lags, where available, without limiting observations only to cases that have those lags. If one of the lags is missing, it enters as a zero. If only Δw_{it-1} is endogenous, the matrix of GMM instruments looks like

$$\begin{bmatrix} w_{i1} & 0 & 0 & ... & 0 & ... & 0 \\ 0 & w_{i1} & w_{i2} & ... & 0 & ... & 0 \\ \vdots & & & \ddots & \vdots & & \vdots \\ 0 & 0 & 0 & ... & w_{i1} & ... & w_{iT} \end{bmatrix}$$

where the first row are instruments for $t = 3$, the second for $t = 4$, etc. The matrix of exogenous variables, which could include standard instrumental variables, is then concatenated onto this matrix. If the vector BMI_{it} is also treated as endogenous, its lags form a similar matrix. Letting Z denote the matrix of all instruments and $\widehat{\epsilon}$ the vector of estimated residuals, we have the following set of moment conditions: $E\left[Z'\widehat{\epsilon}\right] = 0$.

These moment conditions are estimated using XTABOND2 in Stata 11.[16] (See Roodman (2006) for documentation of this program.) All estimates use two-step efficient GMM, which produces robust standard errors, and apply the Windmeijer (2005) finite sample correction to the variance.

3.1 Testing Assumptions

In Section 1, we assumed that the errors, ε, are not serially correlated. Fortunately, this assumption can be tested and the estimation can be modified in response. Regressions based on equation (3) will be $AR(1)$ by design because $\Delta\varepsilon_{it}$ and $\Delta\varepsilon_{it-1}$ both contain ε_{it-1}; however, if there is serial correlation in ε, equation (3) will be at least $AR(2)$ because ε_{it-1} in $\Delta\varepsilon_{it}$ will be correlated with ε_{it-2} in $\Delta\varepsilon_{it-2}$.

AB developed tests for this situation. When the results of difference GMM estimation are presented below, these test results will be presented as well. If a regression is found to be $AR(2)$, but not $AR(3)$, we simply have to take one further lag of each instrument (use w_{it-3} instead of w_{it-2}, and BMI_{it-3} instead of BMI_{it-2}). If the regression is $AR(3)$, we go back

[16] Stata contains its own programs to estimate these models, some of which were influenced by Roodman's. I opt for XTABOND2 because it provides a more complete array of tests than Stata's commands do.

another period, and so on.

The results below are also presented with two tests for the validity of instruments. First, the Hansen J test of overidentifying restrictions tests the joint validity of the moment conditions. Second, a "difference-in-Hansen" test can be conducted to test the validity of subsets of the instruments. When BMI is allowed to be endogenous, this test is used to consider the validity of wage lags and BMI lags separately. In each case, a sufficiently low p-value causes us to reject the exogeneity of instruments.

These tests can also be used to fine tune the specification of the model. For example, if the errors of a specification based on equation (3) are $AR(2)$, more than one lag of the wage might be needed to control for effects of workers' history of earnings. If a difference-in-Hansen test rejects the exogeneity of lagged levels of BMI as instruments, one lag of BMI might not be enough to control for effects of the workers' body mass histories. Adding further lags might then eliminate the part of the error that was correlated with our instruments.[17]

3.2 Potential Problems with Instruments

It's easy to see that the use of all lagged values of wage and body mass variables quickly produces a large number of instruments. As discussed by Roodman (2009) and elsewhere, having a large number of instruments can overfit the endogenous variables, biasing estimates towards OLS and weakening the Hansen tests of instruments' joint validity. He recommends that

[17] Recall that the instruments are lagged levels. Adding lags to a specification based on equation (3) adds lagged differences. Thus adding, say, a second lag of the (differenced) wage does not mean that a second lag level of the wage can not be used as an instrument.

researchers *a*) report the number of instruments used in each regression, *b*) consider the sensitivity of estimates to reductions in the number of instruments, and *c*) be more cautious than accepting p-values of the J-statistic that barely exceed traditional levels of significance.[18]

I follow all of these recommendations in the next section. All lags were used in preliminary estimation, but the results presented in the next section restrict lags to the second through fifth. Using fewer lags as instruments should produce estimates that are less biased by weak instruments.[19]

3.3 Regression Specifications

The next section contains results from simple OLS and fixed-effects estimation as well as the preferred GMM estimation. The BMI variables used in the main regressions are dummy variables for being overweight or heavier (BMI $\geq$ 25), obese (BMI $\geq$ 30), and morbidly obese (BMI $\geq$ 35). The comparison group in each case consists of individuals who are normal weight or below.[20] This specification was chosen for it's combination of simplicity and flexibility. Robustness to changes in these cutoffs are also considered. Robustness to changes in these cutoffs are also considered.

All of the difference GMM estimates presented below are from specifications that include only one lag of wage and one lag of the BMI dummy variables. The main effect of adding further lags of either BMI or the wage was reducing observations by 20 percent for men and 25 percent for women.

[18] Roodman (2009) suggests that a p-value as high as 0.25 might be cause for concern.

[19] Preliminary estimates (not shown) found that more recent lags are strong instruments for differences, but further lags quickly become weak instruments.

[20] Those who are underweight were not separated out because there are very few of them. The results presented below appear robust to this decision.

None of the tests for $AR(2)$ errors suggest that more lags of the wage are needed, and the tests of overidentification fail to suggest a problem with the instruments. Further lags of BMI are never statistically significant and do not qualitatively change the results for the first lag of BMI. Given that the average number of years since labor market entry is intentionally low (less than 4.5), it should not be surprising that a simple lag structure is adequate to capture labor market history in this sample.

All specifications control for the local labor market's unemployment rate and percent obese in the state,[21] as well as dummy variables for the unemployment rate being missing, region, living in an urban area, and being interviewed over the phone. When lagged values of BMI variables are included, the corresponding lag of the phone dummy is also included. Education enters as dummy variables for completing high school, some college, or college and beyond. To control for time variables as thoroughly as possible, I include dummy variables indicating calendar year and the number of years since labor market entry. None of the estimation in this paper is weighted.

I also control for actual experience in the labor market and it's square in the dynamic panel estimates. To better control for commitment to the labor market, I added interactions of experience with years in the labor market. This was done to increase the likelihood that the matrix of lagged wage variables used as GMM instruments would be exogenous.[22] Since ac-

[21] Local unemployment and state identifiers are provided by the NLSY97 Geocode files. Percent obese in the state is tracked by the CDC.

[22] Long lags of the wage variable might themselves reflect the accumulation of experience or committment to the labor market. Using actual experience instead of potential, and then adding these interactions improved difference-in-Hansen tests for the exogeneity of the lagged wage instruments.

tual experience might be endogenous, potential experience, its square and its interactions were used as traditional instruments when estimating the dynamic panel models. In the OLS and fixed-effects regressions, where exogeneity of lagged wage variables are not an issue, I simply use potential experience instead of actual.

Finally, it's not clear whether controls for part time work, marital status, the number of children, and occupation should be included in the regressions or not due to their potential endogeneity. For example, occupation has the potential to influence both wages and body mass; however, selection into and out of occupations might be one of the avenues through which body mass affects wages, which suggests that occupation should not be included.[23] Most specifications will not include these controls, but I will discuss robustness of the main results to their inclusion.

4 Results

This section presents results for each race and gender group. For the sake of comparison to previous work, simple OLS and FE results will be presented first. Then the difference GMM specifications will be discussed.

4.1 OLS and Fixed Effects

Given the results of previous work, neither the OLS nor the FE results for white men, presented in Table 2A, are surprising. The OLS coefficients

[23] Lakdawalla and Philipson (2007), for example, provide evidence that sedentary occupations contribute to obesity for men, but obesity drives selection into sedentary occupations for women.

are consistent with a wage premium of roughly 5% for being overweight, and a penalty for morbid obesity of roughly 9%. When individual fixed effects are removed in the third and fourth columns, the coefficients on both being overweight and being morbidly obese fall, and the only statistically significant effect is a 5% premium for being obese. Adding lagged values of the BMI categories appears to matter only for the OLS estimates, which suggest that the 9% penalty associated with morbid obesity is due to lagged morbid obesity.

The results for white women presented in Table 2B also reveal statistically significant effects on log wage in the OLS regression that disappear once individual fixed effects are controlled for. The OLS results suggest that the wages of overweight women are 6.4% lower than their normal-weight peers, but that appears to be due to lagged overweight status. Oddly, the coefficient on being obese suggests a 5% premium, and is statistically significant at the 10% level. Surprisingly, none of the coefficients in the FE specifications for white women are statistically significant, with the coefficient on being overweight falling to 0.0078 (0.0310).

4.2 Dynamic Panel Results

As mentioned above, the results in Tables 2A and 2B are misspecified if workers' history in the labor market matters. This subsection presents results from GMM estimation of dynamic panel data models that allow both past wages and past body mass to affect current wages. The preferred specifications also allow body mass to be endogenous.

The first two columns of Table 3 present results for men, and the next two present results for women. The first and third columns present results from estimation that assumes BMI categories are exogenous. The second and fourth allow body mass to be endogenous.

Overall, the results for white men and especially white women support the inclusion of an autoregressive term in the wage equations. In specifications that treat BMI categories as endogenous, lagged morbid obesity appears to lower the wages of white men by roughly 18% relative to normal weight men. I also find evidence consistent with white women facing a wage penalty for being overweight in the previous period, but that evidence is not robust to the inclusion of occupation controls.

The results suggest that lagged morbid obesity lowers the wages of white men by 16.4% relative to normal-weight men. The analogous coefficient is only -0.053 (0.065) when BMI is assumed to be exogenous. On the other hand, the only statistically significant coefficient on a BMI category for women suggests a 9% premium for being obese, but only when BMI categories are assumed to be exogenous. The differences between specifications for both men and women are consistent with BMI being endogenous even after fixed effects are removed, although difference-in-Hansen tests (not shown) do not explicitly reject the exogeneity of BMI vectors.

The results in Table 3 also support the inclusion of an autoregressive term in the wage equations. The coefficients on the lag of log wage are always positive and statistically significant. When BMI is treated as endogenous, the coefficient is 0.218 (0.058) for women and 0.079 (0.042) for men. Similar regressions that exclude the autoregressive term (not shown) are at least AR(2) and the second lags of BMI variables can no longer be used as instruments due to the added serial correlation.

4.2.1 Alternative BMI Specifications

It is worth noting that simple linear specifications of BMI only produced a statistically significant effect of BMI on wages in OLS regressions.[24] This is not entirely inconsistent with the non-parametric estimation of Kline and Tobias (2008) and Gregory and Ruhm (2011), both of which suggest a non-linear relationship between BMI and wages. Gregory and Ruhm (2011) also argue that their evidence suggests the penalty associated with BMI is likely due to exceeding levels that are considered physically attractive. If that is the case, using dummy variables for exceeding various BMI cutoffs should provide a simple, tractable way to allow for this nonlinearity; however, there is no reason to believe that thresholds capturing what is (or is not) physically attractive should correspond to categories defined by the WHO.

As a first step, Table 4 presents results that are similar to those in Table 3 that treated BMI as endogenous, but the BMI categories are shifted by 0.5 points in either direction. (E.g., they are either lowered half a point to 24.5, 29.5 and 34.5, or raised to 25.5, 30.5 and 35.5.) Men face a penalty for past morbid obesity regardless of which direction the cutoffs shift, but the penalty is slightly larger with the higher cutoff (35.5) than the lower cutoff (34.5). Men also appear to face a penalty for lagged obesity when cutoffs are lowered by half a point. When cutoffs are lowered by half a point, the coefficient on lagged overweight status jumps from -0.071 (0.055) in Table 3

[24] I also considered quadratic specifications. As with the linear specification, no statistically significant effect was found in quadratic BMI specifications once individual fixed effects were removed.

to -0.128 (0.049), which is statistically significant at a 1% level.

I next considered using a single dummy variable for having a BMI that exceeds various levels. The results for men (not shown) support those in Tables 3 and 4, suggesting a penalty for lagged body mass that begins somewhere around the threshold for morbid obesity. Even when I included a dummy variable for being overweight so that the comparison group would be normal weight men, I found that the penalty for men began as BMI approached 35.

Table 5 present results for women using various BMI thresholds, again allowing BMI to be endogenous. Using a single dummy variable, evidence of a penalty for high body mass in the previous year begins above the cutoff for morbid obesity: the coefficient for a lagged BMI over 35.5 is -0.23 (0.11). We see a penalty for current body mass above a BMI of 36. The coefficient on a dummy variable for a current BMI above 37 (column 4) is -0.256 (0.128).

Table 5 also presents results from estimates that pair a dummy variable for a BMI above 24.5 with indicators for higher BMIs. This shifts the comparison group to women with normal (or lighter) BMIs and allows us to revisit the negative effect of lagged overweight status found in Table 4. Once again, there is evidence of a penalty as BMI exceeds morbid obesity: The effect of a current BMI above 37 is -0.228 (0.106), and the effect of a lagged BMI over 37 is -0.147 (0.066). Consistent with the results of Table 4, the results in columns 6 and 7 suggest women with a lagged BMI exceeding 24.5 face a penalty of nearly 10% relative to lighter women.

4.3 Robustness to Additional Control Variables

As mentioned previously, there are a number of variables that might be correlated with both wages and BMI that have been excluded so far due to potential endogeneity. Tables 6A and 6B consider the robustness of previous results to adding controls for working part time (less than 35 hours per week), marital status, number of children and occupation. Table 6A considers the preferred specification from Table 3 for white men, while Table 6B considers the specification from column 7 or Table 5 for white women.

For each set of added variables, Tables 6A and B also present p-values of difference-in-Hansen tests for exogeneity of the added instruments. The first column of each table adds dummy variables indicating both current and lagged part time work. The next column adds a dummy variable for being married, and indicators for having one, two and three or more children. The third column adds indicators for the following occupation groups: service, which is the omitted group; managerial, professional and technical; clerical; sales; and all other blue collar occupations. All of the added variables are treated as exogenous, except for occupation. The second lagged levels are used as instruments for changes in the occupation dummy variables.[25]

The results of Table 6A suggest that the penalty white men face for past morbid obesity is robust to the addition of these control variables. Adding controls for working part time, which only 7 percent of the male sample does, leaves the coefficient on lagged morbid obesity roughly unchanged. If

[25] Preliminary estimation rejected the exogeneity of current occupation for both men and women. Only one lag is used as instruments for the occupation variables out of concern for the size of the instrument matrix.

adding controls for marriage and children, or for occupation, has any effect, they increase the penalty. When all of these additional controls are included at the same time, the coefficient on lagged morbid obesity suggests a 21% penalty relative to normal-weight peers. In each case, difference-in-Hansen tests fail to reject the exogeneity of the added instruments.

The results in Table 6B also suggest that estimates for white women presented in column 7 of Table 5 are robust to the inclusion of these controls. Part time work is more common for women than for men in the sample, and one might expect marriage or children to have larger effects for women; however, controlling for these variables has no effect on the results. Furthermore, overidentification tests provide no reason to doubt the exogeneity of either part time status and the additional family control variables.[26]

Adding dummy variables for occupation group appears to affect the coefficient on current BMI exceeding 37 in Table 6B, but the difference-in-Hansen tests suggest that even the second lagged levels of these occupation variables are questionable instruments. Finding evidence that occupation is endogenous for women but (possibly) not for men is consistent with the results of Lakdawalla and Philipson (2007). They argue that on-the-job exercise due to occupation has a causal effect on the weight of men, but that body mass affects selection into occupation for women. Controlling for occupation might make sense when examining the wages of men, but occupation is more likely to be an avenue through which body mass affects the wages

[26] Recall that these difference-in-Hansen tests are looking for exogenteity after individual fixed effects are removed. If, for example, the unobservables correlated with the tendency to marry are constant over the short timeframe examined in the paper, they would cancel out in first differences.

of women.

4.4 Selection Bias

There are two reasons to worry about selection into the estimation sample biasing the results in this paper: The difference GMM estimator requires three consecutive years of labor market participation, and I've limited attention to workers who have worked full time for at least 75 percent of two consecutive years. On the other hand, the difference GMM estimator will only be affected by selection bias if selection into the sample is correlated with time-varying unobservables. Selection based on unobservables that are constant over time is removed by differencing.

I use a simple test developed by Semykina and Wooldridge (2010) to shed light on this issue. First, I use a probit to estimate the probability of being in the sample in year t with valid observations for t, $t-1$ and $t-2$; and use the resulting estimates to calculate an inverse Mills ratio.[27] I then add the inverse Mills ratio and its interactions with time dummies to the wage regression. The null hypothesis of no selection is then rejected if the coefficients on the Mill ratio and its interactions are jointly significant.[28]

The null hypothesis of no selection cannot be rejected for either men or women. The p-value on the test of joint statistical significance is 0.374 for white men and 0.146 for white women. Furthermore, none of the individual

[27] AFQT, which would be differenced out of the wage regressions, is used as an instrument in the probit estimates.

[28] The test proposed by Semykina and Wooldridge (2010) does not require adjusting standard errors for the first-stage probit estimation; however, it is important to note that coefficients from estimated wage regression should not be presented as selection-corrected results.

coefficients on the inverse Mills ratio or its interactions with year are statistically significant. These tests, therefore, provide no reason to suspect the results of this paper are biased by time-varying sample selection.

5 Discussion

This paper is the first to consider effects of body mass on wages in a dynamic panel data model. This framework allows us to consider the effects of both current and past body mass, while also controlling for past wages. Furthermore, it improves upon previous attempts to account for unobserved heterogeneity by removing individual-specific components of the error term, while also exploiting a large set of instrumental variables.

The results suggest that past levels of body mass affect the wages of young workers more often than current body mass. White men appear to be penalized for a history of morbid obesity. White women face a penalty for a history of being overweight, with an additional penalty for past morbid obesity. The only evidence of an effect of current body mass is a penalty faced by morbidly obese women.

These results have important implications for the literature on body mass and earnings. First of all, the specifications of wage regressions used in this literature should be relaxed to allow for dynamic effects. First-differenced or fixed-effects estimation should not assume that past changes in body mass have no effect on more recent changes in wages. Furthermore, autoregressive wage equations should be considered because past earnings might affect both current earnings and body mass.

The importance of past body mass also supports the focus of other authors on indirect effects of body mass on wages. Han, et al (2009) consider indirect effects of teen body mass on wages through effects of body mass on education and occupation. The results of this paper, which control for education and (in robustness tests) occupation, suggest that body mass might affect wages through more subtle means as well. Negative effects on job search or promotion rates, for example, might explain the effects of past body mass on current wages.

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A Data Appendix

(This section, other than Tables A1 and A2, has not been updated since the previous draft.)

This appendix describes the selection of the estimation sample, and presents more detailed summary statistics.

The sample was first restricted to white respondents in the NLSY97. This drops 3,752 respondents, leaving 2,702 white men and 2,530 white women. 277 respondents who reported being in the military, most of them male, were then dropped. This left 29,688 person/year observations for 2,474 men, and 29,772 observations for 2,481 women.

Restricting observations to those in which the respondent has entered the labor market reduces the number of observations to 10,503 for 1,684 white men, and 8,242 for 1,445 women. Only the primary (current or most recent) job is used from each interview. Observations in which a woman reported being pregnant in the current year (since the last interview) or previous year were also dropped, reducing observations to 6,439 for 1,409 women. An additional 57 observations in which a respondent reported a military occupation despite not being otherwise identified as in the military were dropped from the sample. Finally, 5 observations for men and 1 for a woman were dropped because the absolute value of the change in log wages was greater than 6.5.[29]

The preferred dynamic panel specifications require three consecutive observations in a row with non-missing values of wage and BMI. Restricting observations to those that could be from one of three such consecutive

[29] These observations were obvious outliers in the distribution of log wage changes. One of the wage observations in each case was below $0.2. Otherwise, observations that might appear to be outliers in the distribution of wages were not dropped from the sample.

years leaves 7,523 observations for 1,324 men and 3,932 observations for 835 women.

The appendix tables A1 and A2 present summary statistics for men and women, respectively, that are not presented in Table 1. The mean wage for white men is pulled up by outliers; however, estimates do not appear sensitive to dropping these high wage observations. Median hourly wages (not shown) are $9.55 for men and $8.85 for women.

As expected, the sample is largely urban. The differences in urbanicity between men and women, as well as most of the differences in education, appear to be due to how men and women enter the labor market. Looking at the entire sample (not shown), instead of focusing on those who are committed enough to the labor market to be in the estimation sample, reveals no difference by gender in urbanicity and much smaller differences in education.

Table 1. Summary Statistics

	Mean	Std. Dev.	Min	Max
White Men				
Wage	19.815	354.946	0.039	23,883.93
Log Wage	2.330	0.622	-3.252	10.081
BMI	26.723	5.514	12.838	63.313
Underweight	0.018	0.135	0	1
Overweight	0.573	0.495	0	1
Obese	0.222	0.416	0	1
Morbidly Obese	0.078	0.268	0	1
Age	23.700	2.698	16	30
Phone Interview	0.111	0.314	0	1
Yrs since LM Entry	4.469	2.657	1	14
Yrs in 2009	7.014	2.563	3	14
Actual Experience	4.169	2.458	0.75	13.058
Exp in 2009	6.400	2.413	1.846	13.058
White Women				
Wage	11.032	21.448	0.046	774.08
Log Wage	2.193	0.568	-3.069	6.652
BMI	25.790	6.654	10.962	72.620
Underweight	0.042	0.201	0	1
Overweight	0.416	0.493	0	1
Obese	0.201	0.401	0	1
Morbidly Obese	0.103	0.304	0	1
Age	23.964	2.550	16	30
Phone Interview	0.107	0.310	0	1
Yrs since LM Entry	4.052	2.494	1	13
Yrs in 2009	6.186	2.509	3	13
Actual Experience	3.792	2.295	0.75	12.769
Exp in 2009	5.624	2.333	1.558	12.769

The sample for this table includes all observations that use as t , t- 1, or t -2 in the main estimation. This results in 9,097 observations for white men, and 5,453 observations for white women. In 2009, the sample includes 1194 men and 716 women.

Table 2A. Results from Basic OLS and FE Specifications
White Men

	OLS w/out lags	OLS with lags	FE w/out lags	FE with lags
Overweight	0.0514***	0.0334	0.0024	0.0017
	(0.0157)	(0.0225)	(0.0248)	(0.0247)
L.Overweight		0.0231		-0.0020
		(0.0226)		(0.0207)
Obese	0.0309	0.0107	0.0511**	0.0445*
	(0.0212)	(0.0263)	(0.0249)	(0.0254)
L.Obese		0.0330		0.0319
		(0.0284)		(0.0284)
Morbidly Obese	-0.0892***	-0.0381	0.0102	0.0179
	(0.0284)	(0.0457)	(0.0405)	(0.0381)
L.Morbidly Obese		-0.0874*		-0.0490
		(0.0460)		(0.0401)

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Estimates use 7,486 observations for 1,480 men. Observations are limited to those that could be period *t* or *t*-1 in Difference GMM specifications.

Table 2B. Results from Basic OLS and FE Specifications
White Women

	OLS w/out lags	OLS with lags	FE w/out lags	FE with lags
Overweight	-0.0643***	-0.0221	0.0078	0.0083
	(0.0205)	(0.0252)	(0.0310)	(0.0307)
L.Overweight		-0.0687***		0.0098
		(0.0258)		(0.0250)
Obese	0.0106	0.0130	0.0267	0.0253
	(0.0258)	(0.0308)	(0.0301)	(0.0288)
L.Obese		0.0275		-0.0112
		(0.0317)		(0.0299)
Morbidly Obese	0.0172	0.0136	0.0204	0.0163
	(0.0292)	(0.0331)	(0.0365)	(0.0354)
L.Morbidly Obese		-0.0083		0.0406
		(0.0336)		(0.0395)

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Estimates use 4,297 observations for 1.073 women.

Table 3. Effects of Past and Current BMI on Log Wages
Results from Dynamic Panel Data Models

	White Men		White Women	
	BMI exogenous	BMI endogenous	BMI exogenous	BMI endogenous
L.lnwage	0.1254**	0.0793*	0.3064***	0.2176***
	(0.0531)	(0.0420)	(0.0689)	(0.0580)
Overweight	0.0331	-0.1017	-0.0410	0.0349
	(0.0319)	(0.1192)	(0.0366)	(0.0932)
L.Overweight	-0.0005	0.0444	0.0060	-0.0709
	(0.0339)	(0.0490)	(0.0313)	(0.0551)
Obese	0.0447	0.0194	0.0887*	0.0525
	(0.0339)	(0.1080)	(0.0462)	(0.0877)
L.Obese	0.0383	0.0077	-0.0220	-0.0449
	(0.0348)	(0.0576)	(0.0567)	(0.0666)
Morbidly Obese	0.0396	0.0411	-0.0578	-0.0085
	(0.0444)	(0.0955)	(0.0608)	(0.0580)
L.Morbidly Obese	-0.0532	-0.1644**	-0.0426	-0.0625
	(0.0651)	(0.0675)	(0.0387)	(0.0688)
AR(1): z-statistic	-4.309	-5.044	-4.914	-4.815
p value	0.0000164	0.0000004	0.0000009	0.0000019
AR(2): z-statistic	-0.297	-0.452	-0.383	-0.446
p value	0.766	0.651	0.702	0.656
Number of Instruments	93	199	91	189
Hansen test (p value)	0.728	0.789	0.910	0.825
Diff-in-Hansen Tests for Exogeneity of Subsets of GMM Instruments (p value)				
ln(wage) lags	….	0.521	….	0.755
BMI cat. Lags	….	0.769	….	0.725

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Estimates use 5,901 observations for 1,474 men; and 3,154 observations for 1,060 women.

Table 4. Effects of Past and Current BMI on Log Wages
Robustness to Alternative BMI Cutoffs

	White Men		White Women	
VARIABLES	Cutoff - 0.5	Cutoff + 0.5	Cutoff - 0.5	Cutoff + 0.5
L.lnwage	0.0864**	0.0916**	0.2262***	0.2366***
	(0.0421)	(0.0391)	(0.0611)	(0.0587)
Overweight	-0.2169	-0.0604	-0.0251	-0.0820
	(0.1432)	(0.1513)	(0.1031)	(0.0806)
L.Overweight	0.0550	0.0103	-0.1279***	0.0055
	(0.0488)	(0.0485)	(0.0488)	(0.0535)
Obese	0.1934	0.1304	0.0532	0.0355
	(0.1282)	(0.1272)	(0.0693)	(0.0945)
L.Obese	-0.0952*	-0.0404	-0.0240	0.0172
	(0.0560)	(0.0616)	(0.0639)	(0.0646)
Morbidly Obese	-0.1172	-0.0319	0.0552	-0.0455
	(0.1100)	(0.1275)	(0.0841)	(0.0801)
L.Morbidly Obese	-0.1234**	-0.1725*	-0.0831	-0.0910
	(0.0588)	(0.0885)	(0.0632)	(0.0623)

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Effects of Past and Current BMI on Log Wages of White Women Using Fewer BMI Cutoffs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.lnwage	0.2658***	0.2873***	0.2757***	0.2683***	0.2515***	0.2402***	0.2383***
	(0.0676)	(0.0679)	(0.0669)	(0.0673)	(0.0613)	(0.0603)	(0.0588)
BMI>=24.5	-0.1018	….	….	….	-0.1277	-0.1120	-0.0849
	(0.1163)				(0.0936)	(0.1036)	(0.1016)
L.(BMI>=24.5)	-0.0709	….	….	….	-0.0854	-0.0982*	-0.0967**
	(0.0633)				(0.0559)	(0.0517)	(0.0486)
BMI>=35.5	….	-0.2573	….	….	-0.1578	….	….
		(0.2245)			(0.1101)		
L.(BMI>=35.5)	….	-0.2295**	….	….	-0.1326*	….	….
		(0.1064)			(0.0789)		
BMI>=36	….	….	-0.3851*	….	….	-0.2677*	….
			(0.2000)			(0.1449)	
L.(BMI>=36)	….	….	-0.1582**	….	….	-0.1014	….
			(0.0774)			(0.0638)	
BMI>=37	….	….	….	-0.2563**	….	….	-0.2277**
				(0.1279)			(0.1062)
L.(BMI>=37)	….	….	….	-0.1865**	….	….	-0.1470**
				(0.0736)			(0.0656)
AR(1): z-statistic	-4.687	-4.899	-4.888	-4.690	-4.836	-4.969	-4.776
p value	0.000003	0.000001	0.000001	0.000003	0.000001	0.000001	0.000002
AR(2): z-statistic	-0.519	-1.072	-0.956	-1.018	-0.767	-0.605	-0.865
p value	0.604	0.284	0.339	0.309	0.443	0.545	0.387
Hansen p value	0.868	0.962	0.981	0.962	0.927	0.962	0.972

Table 6A. Effects of Past and Current BMI on Log Wages of White Men with Additional Control Variables

	Part Time	Family	Occupation	All
L.lnwage	0.0767*	0.0754*	0.0786*	0.0772*
	(0.0423)	(0.0420)	(0.0421)	(0.0417)
Overweight	-0.1154	-0.1297	-0.0858	-0.0966
	(0.1176)	(0.1205)	(0.1143)	(0.1133)
L.Overweight	0.0407	0.0439	0.0422	0.0470
	(0.0490)	(0.0492)	(0.0464)	(0.0470)
Obese	-0.0236	-0.0160	0.0447	0.0414
	(0.1127)	(0.1163)	(0.1240)	(0.1208)
L.Obese	0.0126	0.0109	-0.0156	-0.0170
	(0.0576)	(0.0576)	(0.0593)	(0.0580)
Morbidly Obese	-0.0188	-0.0264	0.0208	0.0135
	(0.1207)	(0.1219)	(0.1239)	(0.1234)
L.Morbidly Obese	-0.1693**	-0.1751**	-0.2135***	-0.2113***
	(0.0692)	(0.0683)	(0.0775)	(0.0774)
Difference-in-Hansen p-value for Added Instruments				
Part Time Work	0.441	…	…	0.900
Married & # Children	…	0.556	…	0.992
Occupation	…	…	0.495	0.392

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

When controlling for part time, current and lagged dummy variables are used. Only current dummy variables for being married, and having 1, 2, or 3 or more children are used. Occupation controls are dummy variables for currently being in one of five categories: service; managerial, professional or technical; clerical; sales; and all other blue collar. All added variables are treated as exogenous, except for occupation. The 2nd lagged levels are used as instruments for occupation. All other variables and number of observations are as in Table 3.

Table 6B. Effects of Past and Current BMI on Log Wages of White Women with Additional Control Variables

	Part Time	Family	Occupation	All
L.lnwage	0.2382***	0.2290***	0.2278***	0.2229***
	(0.0588)	(0.0578)	(0.0539)	(0.0513)
BMI >= 24.5	-0.0836	-0.1068	-0.0664	-0.0946
	(0.0989)	(0.1021)	(0.1007)	(0.1000)
L.(BMI >= 24.5)	-0.0954*	-0.0999**	-0.1091**	-0.1172**
	(0.0496)	(0.0498)	(0.0544)	(0.0548)
BMI >= 37	-0.2341**	-0.2095**	-0.1854**	-0.1547
	(0.1098)	(0.1064)	(0.0918)	(0.1335)
L.(BMI >= 37)	-0.1450**	-0.1405**	-0.1423**	-0.1290**
	(0.0652)	(0.0606)	(0.0613)	(0.0581)
Difference-in-Hansen p-value for Added Instruments				
Part Time Work	0.779	…	…	0.670
Married & # Children	…	0.635	…	0.797
Occupation	…	…	0.141	0.084

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

When controlling for part time, current and lagged dummy variables are used. Only current dummy variables for being married, and having 1, 2, or 3 or more children are used. Occupation controls are dummy variables for currently being in one of five categories: service; managerial, professional or technical; clerical; sales; and all other blue collar. All added variables are treated as exogenous, except for occupation. The 2nd lagged levels are used as instruments for occupation. All other variables and number of observations are as in Table 3.

Table A1. Additional Summary Statistics for White Men

	Mean	Std. Dev.	Min	Max
ln(Wage)	2.3296	0.6218	-3.2518	10.0810
ln(Wage) Difference	0.0516	0.5868	-6.2019	6.1420
Decrease in BMI cat.	0.0726	0.2594	0.0000	1.0000
Increase in BMI cat.	0.1313	0.3377	0.0000	1.0000
South	0.3177	0.4656	0	1
Urban	0.7001	0.4582	0	1
Part Time	0.0717	0.2580	0	1
Married	0.2308	0.4214	0	1
Any Children	0.2727	0.4454	0	1
HS	0.3597	0.4799	0	1
Some College	0.2403	0.4273	0	1
College	0.1710	0.3766	0	1
Local Unempl. Rate	6.3188	2.7277	1	27.8
Occupations				
Service	0.1621	0.3685	0	1
Mgmt, Tech., & Prof.	0.1697	0.3754	0	1
Sales	0.1092	0.3119	0	1
Clerical, Admin.	0.0890	0.2847	0	1
Misc. Blue Collar	0.4701	0.4991	0	1

Note: Local unemployment rate (x 100) and occuptions summarized where non-missing.

Table A2. Additional Summary Statistics for White Women

	Mean	Std. Dev.	Min	Max
ln(Wage)	2.1932	0.5681	-3.0695	6.6517
ln(Wage) Difference	0.0448	0.4910	-6.0604	4.4993
Decrease in BMI cat.	0.0598	0.2371	0.0000	1.0000
Increase in BMI cat.	0.1095	0.3123	0.0000	1.0000
South	0.3283	0.4696	0	1
Urban	0.7532	0.4312	0	1
Part Time	0.1152	0.3193	0	1
Married	0.2534	0.4350	0	1
Any Children	0.2021	0.4016	0	1
HS	0.2786	0.4483	0	1
Some College	0.2815	0.4498	0	1
College	0.3211	0.4669	0	1
Local Unempl. Rate	6.2415	2.6670	1.4	19.2
Occupations				
Service	0.2582	0.4377	0	1
Mgmt, Tech., & Prof.	0.2927	0.4550	0	1
Sales	0.1326	0.3391	0	1
Clerical, Admin.	0.2481	0.4319	0	1
Misc. Blue Collar	0.0685	0.2526	0	1

Note: Local unemployment rate (x 100) and occuptions summarized where non-missing.