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Exogenous Treatment and Endogenous Factors: Vanishing of Omitted Variable Bias on the Interaction Term*

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Abstract

Whether interested in the differential impact of a particular factor in various institutional settings or in the heterogeneous effect of policy or random experiment, the empirical researcher confronts a problem if the factor of interest is correlated with an omitted variable. This paper presents the circumstances under which it is possible to arrive at a consistent estimate of the mentioned effect. We find that if the source of heterogeneity and omitted variable are jointly independent of policy or treatment, then the OLS estimate on the interaction term between the treatment and endogenous factor turns out to be consistent.

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Significant increase in the use of random experiments in the development economics and natural experiments throughout other fields of economics is raising the question of whether it is possible to obtain a consistent estimate of the heterogeneous treatment effect if the heterogeneity is occurring along the lines of a factor which is correlated with some omitted variable(s). Likewise, empirical researchers are often interested in estimation of the differential impact of a particular factor (which maybe correlated with omitted variables) in various institutional settings. These two situations are similar if the policy variable or assignment to the treatment group is uncorrelated with either the factor of interest (source of heterogeneity) or with the omitted variable inasmuch as the goal is to estimate the coefficient on the interaction term between the policy/treatment variable and the factor of interest which is correlated with the error term.

The textbook approach to econometric modeling suggests that we ought to include all the relevant variables into a model. The justification of this approach is due to possible (partial) correlations among the explanatory variables. Indeed, every standard econometric textbook shows, if included regressors are partially correlated with an excluded additional explanatory variable, the exclusion of this additional relevant regressor will result in omitted variable bias.[1]

This straightforward theoretical result is of serious consequence for data analysts, since applied researchers are rarely able to follow the suggestion to include all the relevant explanatory variable. In reality, we cannot always include all the omitted variables for various reasons, often due to their unobservability. Unless there is an instrumental variable (IV) available, there is little hope to get consistent estimates of the model parameters then.

As an alternative to the IV approach, one can assess the magnitude or at least the direction of the bias. However, theoretical textbooks' discussions about omitted variable bias always focus on the example when the true model contains two variables (in addition to the constant term), but the estimated model omits one variable, which is correlated with the regressor of interest. This setup allows researchers to talk about the direction of the bias and speculate whether the biased OLS estimate helps in understanding the issue at hand or one should definitely be searching for a way to obtain consistent estimates. In applied works, however, the case with just two relevant factors is very uncommon and, therefore, when applied researchers discuss the direction of the bias they just hope that in case of more than two variables in the model, the direction of the bias will be the same as in the textbook example. But every textbook

[1] It is worth reminding another relevant standard textbook fact: excluding an explanatory variable that is partially uncorrelated with included regressors has no effect on unbiasedness and consistency of the OLS estimates.

consideration of the issue concludes with the warning that in the case of three or more variables in the model, it is difficult to tell what would be the direction of the bias. This applies to the estimation of the heterogeneous treatment effect since there are at least four variables in this setting (in addition to the constant term): an endogenous factor, an omitted variable correlated with this endogenous factor, an exogenous treatment[2], and an interaction term between the treatment and endogenous factor.

A natural question that comes to mind in this case is whether there are at least some situations when the exclusion of the relevant variable is of not such a severe consequence. Is there a scenario under which the unobserved covariate correlated with the included regressors does not cause much trouble (at least) for some of the model parameters that are of interest? It turns out that this situation is indeed possible and quite common in applied works. Let all the regressors but the exogenous regressor of main interest and the interaction term between this exogenous regressor and an endogenous covariate to be jointly independent of the exogenous regressor of the main interest.[3] Then, the OLS estimate of the coefficient on this interaction term is consistent. Therefore, one can use this result to inform policy makers of the differential impact of some endogenous factors in different policy settings, or about heterogeneous treatment effect when the source of heterogeneity is endogenous, provided that the endogenous factor of interest and the unobservable are jointly independent of the policy/treatment. While a special case, it is very common in applied studies and is of huge relevance for policy analysis. For example, Blank (1991) study of the AER experiment with random assignment to single-blind vs. double-blind refereeing focuses on the heterogeneous impact of the treatment by gender and rank of the university, Nizalova (2010) explores the impact of informal care policies on the wage effects on informal care and labor supply.

To the best of our knowledge, consistency of the estimate of the OLS coefficient for the interaction between a policy/treatment variable and an observed endogenous factor when the covariate and the unobservable are jointly independent of the policy/treatment has not been shown previously. Here we derive this rather important and relevant result explicitly. The rest of the paper is structured in the following way. Section 2 describes the relevant applications. Section 3 provides econometric results referring to one of the applications presented in Section 2 as an example. Using Monte Carlo simulations, Section 4 presents the extension of the model with only four regressors to the case when additional explanatory variables are included. Conclusions follow in Section 5.

[2]We call treatment exogenous as we assume that the source of heterogeneity and omitted variable(s) are jointly independent of the treatment.

[3]We also discuss a weaker set of conditions later in the paper.

1 Relevant Applications

This section is devoted to the description of relevant empirical studies where the interest lies either in the estimation of the heterogeneous treatment effect when the heterogeneity lies along the lines of a factor which is correlated with omitted variables or in the estimation of the differential impact of the correlated factor under various (supposedly exogenous) policy regimes.

1.1 Heterogeneous Impact of Treatment in Experimental Studies

Earlier works which evaluated the effects of large scale random experiments and those which exploited the so-called natural experiments mostly focused on the estimation of the treatment effect only. One of the exceptions we found dates back to 1991 and describes the experimental evidence on the effects of double-blind versus single-blind reviewing on the probability of acceptance of a paper for publication in the American Economic Review (Blank 1991).

The AER experiment was held over the period 1987-1989 and resulted into a sample of 1,498 papers with completed referee reports, which were either double-blind or single-blind through a random assignment. The results suggested that the double blind procedure is stricter, which is confirmed by a significantly lower acceptance rate and more critical referee reports. However, the emphasis of the paper is not on the overall effect of the double-blind refereeing, but rather on the heterogeneous impact of the treatment, which is the focus of this paper. In particular, some earlier studies found that women have higher acceptance rates in double-blind journals (Ferber and Teiman 1980), and this was chosen as one of the important dimensions of heterogeneity. Other dimensions included the rank of the university and indicators whether the institution is U.S. nonacademic or foreign. Clearly, gender is likely to be correlated with other important factors, which were not observed in the experiment, such as age and experience in the profession. Likewise, being in a higher ranked university maybe the result of the overall higher unobserved productivity. The coefficients on interaction terms turned out to be statistically insignificant, suggesting no benefits of double-blind refereeing to either women or authors from lower-ranked universities. But can this finding be trusted? The author states that the coefficients on the interaction terms "should be robust to the inclusion of any other variables in the model, since they come from two experimental samples that are identical in all other characteristics" (Blank 1991, p. 1054). At the same time with respect to the main effects of gender and the university rank, the author claims that "it is not clear how to interpret the coefficients on these variables, because they are contaminated by excluded variables" (Blank 1991, p. 1055). These statements are indications of what we are to prove explicitly in this paper: the consistency of the estimates of the heterogeneous impact of random treatment/ exogenous policy when the heterogeneity occurs along the lines of a factor correlated with the omitted variable(s).

In recent years a considerable number of works has appeared which either directly investigate the heterogeneity of treatment effect or point to the possibility of its existence. This subsection describes several examples of such studies. However, the studies which do estimate the heterogeneous effects are more reserved than Blank (1991) with respect to the discussion of the consistency of the estimates.

Blau et al. (2010) report on the impact of a trial in which the Committee on the Status of Women in the Economics Profession (CSWEP) randomly chose the participants of the CSWEP Mentoring Program (CeMENT) which "aimed at assisting female junior faculty in preparing themselves for the tenure hurdle." The authors find that in 3-5 years after the Program participants have higher likelihood of having any top-tier publication and more publications in general, as well as more federal grants. As the rate of acceptance to the journals may depend on the rank of the university (Blank 1991), it may be interesting to investigate whether the impact of the CeMENT is different for junior female faculty from low-rank versus high rank universities.

A recent study by Glewwe, Kremer, and Moulin (2009) focuses on the evaluation of a randomized trial in rural Kenya estimating the effect of provision of free textbooks on the students' test scores. Compared to the earlier literature on the effect of the textbook provision on the test scores, the authors find no significant treatment effect. However, when taking into account the heterogeneity by the past test scores, they reach the conclusion that the best students do benefit from the textbook provision. The study has a cross-sectional set-up and therefore the authors could not control for students' ability. The previous test scores, likewise the current test scores, are clearly correlated with the unobserved ability. Therefore, the authors study the heterogeneity of the treatment effect along the lines of a factor which is correlated with the error term.

Similarly, Banerjee et al. (2007) evaluate the two randomized experiments in India where a remedial education program hired young women to teach students lagging behind in basic literacy and numeracy skills. They also consider the previous test scores as the source of the heterogeneity of impact by dividing the sample into terciles according to the past score distribution. The largest gains are experienced by children at the bottom of the test-score distribution.

Banerjee et al. (2009) estimate the impact of a randomized introduction of microcredit in a new market. They find that households with an existing business at the time of the program invest more in durable goods. Moreover, households with high propensity to become business owners see a decrease in nondurable consumption, while households with low propensity to become business owners show an increase in nondurable spending. The study is again set up as a cross-section and there is a considerable room for omitting variables which determine past business ownership and current propensity to become a business owner and the consumption patterns. People who are already business owners or have a higher potential to become ones are potentially different from the rest of the population in characteristics which may as well determine the spending patterns.

1.2 Policies Addressing Concerns Related to Population Aging

The demographic trends over the past half-century have shown a tendency towards population aging both in the developed and in the majority of developing countries. The biggest concerns discussed in policy circles related to the population aging remain the burdening of the retirement systems in the countries where they exist and old-age poverty in the absence of such systems (Gavrilov and Heuveline 2003). However, there is a second as important and often overlooked concern - the ever-growing costs of elderly care, both public and private. These two concerns are discussed at different times, during different meetings, and most often by different authorities and scholars. Therefore the measures that emerge from these debates may turn out to be incompatible. On the one hand, policy measures suggested in the debate over retirement systems sustainability include removal of the disincentives for labor force participation for near elderly (CBO 2004, U.S. DHHS 1997, Apfel 2004). This would effectively raise the wage rate faced by the targeted group, as in the case of the elimination of the Social Security earnings test for those older than 65 in the United States (Friedberg 2000). On the other hand, the role of informal caregiving is emphasized as means to "keep many individuals at home who would otherwise require expensive institutional care" (U.S. DHHS 1997, p.6). Policies targeting these two objectives may turn out to conflict with each other, given that they target the same population of near elderly[4]: higher wages may decrease hours devoted to informal care for elderly parents, while policies encouraging informal care may lead to fewer working hours.

[4] McGarry (2003) cites that according to the Commonwealth Fund's (1999) report, the fraction of women providing care is highest among the 45-64 age group [near elderly]: 13 percent compared to 10 percent for women of 30-44 years old and 7 percent of women 65 years old or older.

Approximating the effect of the labor supply stimulating policies with the wage rate Nizalova (2010) estimates the wage effects on informal care supply and labor supply for the population of the near elderly in different institutional settings related to the long-term care. Using the data from 12 European countries (Study of Health, Aging, and Retirement in Europe), the author finds that in the countries with more generous formal long-term care arrangements, individuals are more responsive to wage changes both in labor supply and in informal care supply. In such a setting individuals reduce hours of care for elderly parents while increasing labor supply. At the same time, in the countries where the policies are directed towards the promotion of informal care, the wage responsiveness is smaller.

The major concern of Nizalova's (2010) is that the wage in both time allocation equations is correlated with omitted variables (such as ability, work attitude, responsibility, which are usually not observed by researchers) leading to a bias in the estimated wage effects. However, the question of interest is not the wage effect per se, but the differences in the wage effects in different institutional settings, i.e., the coefficient on the interaction term between wages and variables describing policies.

2 Econometric Result in the Context of the AER Experiment

For concreteness, let us talk about the example of the AER experiment on the effect of double-blind versus single-blind reviewing we have described in the previous section. To start with, we consider only one dimension of heterogeneity - the rank of the university which the author is affiliated with. A simplified relation between the acceptance rates[5] and assignment to the blind review group can be described as:

$$y = \beta_1 + \beta_2 r \cdot d + \beta_3 r + \beta_4 d + \beta_5 c + \varepsilon, \tag{1}$$

where r is a continuous[6] variable specifying rank of the university, d is an indicator of the double-blind treatment, c is the unobserved individual-specific effect, and u is the idiosyncratic error. Our main question of interest is whether the effect of the double-blind reviewing affects the acceptance rates differently depending on the rank of the university. In other words, the parameter of interest in Equation (1) is β_2. However, we realize that unobserved personality traits, c, that also enter the equation for the acceptance rate are correlated with the university rank. While the correlation between the time-invariant part of the

[5]Blank (1991) uses a linear probability model for the estimates of the effect of the double-blind procedure. Thus, for the sake of simplicity we use the OLS setting as well.

[6]Although the university rank is represented by a set of indicators in Blank (1991), we use one variable in this particular application, r, which can be thought of as being at least roughly continuous.

unobserved effect and observed covariate can be controlled for in a panel setting using the fixed effects transformation, we cannot use this solution having only a cross section of data and/or when the unobserved effect is not constant over time. In the experimental settings it is difficult to have panel data. Moreover, the most obvious unobserved characteristic in the considered experiment is the productivity of the author, which can clearly be (at least partially) time dependent. Standard econometric wisdom might lead us to a conclusion that given only cross-sectional data the estimates of all the parameters from Equation (1) will be biased and inconsistent due to a non-zero correlation between the rank of the university and unobserved personality traits. Let us see if this is indeed the case.

We can use the popular econometric textbook by Green (2003) to get the following general result. Suppose the correct specification of the regression model is $\mathbf{y} = \mathbf{i}\beta_1 + \mathbf{X}_2\beta_2 + \mathbf{X}_3\beta_3 + \varepsilon$, where $\mathbf{i}$ is a vector of ones. Premultiplying this equation by matrix $\mathbf{M}_1 = \mathbf{I} - \mathbf{i}(\mathbf{i}'\mathbf{i})^{-1}\mathbf{i}$, where $\mathbf{I}$ is an $n \times n$ identity matrix, yields a demeaned version of the original model:

$$\mathbf{M}_1\mathbf{y} = \widetilde{\mathbf{X}}_2\beta_2 + \widetilde{\mathbf{X}}_3\beta_3 + \mathbf{M}_1\varepsilon, \tag{2}$$

where $\widetilde{\mathbf{X}}_2$ and $\widetilde{\mathbf{X}}_3$ are mean-differenced $\mathbf{X}_2$ and $\mathbf{X}_3$.[7] Further, suppose we do not observe $\mathbf{X}_3$ and estimate $\mathbf{M}_1\mathbf{y} = \widetilde{\mathbf{X}}_2\beta_2 + \varepsilon^*$, where $\varepsilon^* = \widetilde{\mathbf{X}}_3\beta_3 + \mathbf{M}_1\varepsilon$ and $\mathbf{M}_1\varepsilon$ is a vector of mean-differenced errors. Then, under the usual assumptions, we modify the omitted variable formula from Green (2003) to report the probability limit of $\widehat{\beta_2}$:

$$\text{plim}(\widehat{\beta_2}) = \beta_2 + \mathbf{Q} \cdot \beta_3, \tag{3}$$

where $\mathbf{Q} = \text{plim}(\widetilde{\mathbf{X}}_2'\widetilde{\mathbf{X}}_2)^{-1}\widetilde{\mathbf{X}}_2'\widetilde{\mathbf{X}}_3$ is the probability limit of the matrix of regression coefficients from the auxiliary regressions of the excluded mean-differenced variables, $\widetilde{\mathbf{X}}_3$, on the included mean-differenced variables, $\widetilde{\mathbf{X}}_2$.

We want to apply general theoretical result (3) to model (1), which is of great interest for policy analysts. For simplicity of notation, we rewrite model (1) as:

$$y = \beta_1 + \beta_2 x_2 + \beta_3 x_3 + \beta_4 x_4 + \beta_5 x_5 + \varepsilon, \tag{4}$$

where $x_2 = x_3 \cdot x_4$. By analogy with model (1), x_5 is unobserved and instead of (4) we estimate

$$y = \beta_1 + \beta_2 x_2 + \beta_3 x_3 + \beta_4 x_4 + \varepsilon^*, \tag{5}$$

[7]Note that we are not able to estimate the intercept β_0 from the demeaned model.

where $\varepsilon^* = \varepsilon + \beta_5 x_5$. We can relate models (2) and (4) so that $\widetilde{\mathbf{X}}_2 = (\widetilde{x}_2, \widetilde{x}_3, \widetilde{x}_4)$ and $\widetilde{\mathbf{X}}_3 = \widetilde{x}_5$, where $\widetilde{x}_j$ is a mean-differenced x_j, $j = 2, 3, 4, 5$. To obtain the parameter of our interest – β_2 – we can rewrite Equation (3) in the context of (5) to be

$$\text{plim}(\widehat{\beta}_2) = \beta_2 + \beta_5 Q_2, \tag{6}$$

where scalar Q_2 is the first row of a 3×1 column $\mathbf{Q}$:

$$\mathbf{Q} = \text{plim}(\sigma_5 (\Lambda R \Lambda)^{-1} \Lambda \omega) = \text{plim}(\sigma_5 \Lambda^{-1} R^{-1} \omega), \tag{7}$$

where $\Lambda = \begin{pmatrix} \sigma_2 & 0 & 0 \\ 0 & \sigma_3 & 0 \\ 0 & 0 & \sigma_4 \end{pmatrix}$, $R = \begin{pmatrix} 1 & r_{23} & r_{24} \\ r_{23} & 1 & r_{34} \\ r_{24} & r_{34} & 1 \end{pmatrix}$ and $\omega = \begin{pmatrix} r_{25} \\ r_{35} \\ r_{45} \end{pmatrix}$. Here, σ_j is the sample standard deviation of x_j, $j = 2, 3, 4, 5$, and r_{kl} is the sample correlation between x_k and x_l, $k = 2, 3, 4$ and $l = 3, 4, 5$. Then, straightforward matrix algebra reveals that

$$Q_2 = \text{plim} \frac{\sigma_5}{\sigma_2} \cdot \frac{r_{25}(1 - r_{34}^2) + r_{35}(r_{24} r_{34} - r_{23}) + r_{45}(r_{23} r_{34} - r_{24})}{1 - r_{23}^2 - r_{24}^2 - r_{34}^2 + 2 r_{23} r_{24} r_{34}}. \tag{8}$$

In the context of model (5) Q_2 is the plim of the regression coefficient on x_2 in the "auxiliary"regression of the excluded variable, x_5, on the included variables, x_2, x_3 and x_4. Thus, we clearly see that the effect of omitting x_5 depends on the magnitude of the excluded coefficient, β_5, all possible correlations between the included and excluded variables, and the standard deviations of x_2 and x_5. Equation (8) is derived under the usual assumptions, which do not impose any restrictions on the relationships between any variables. However, as we discuss in the previous section, we can restrict the assumption of non-zero correlations among some of the variables when studying the heterogeneous treatment effect, e.g. the effect of double-blind reviewing on the acceptance rates for different groups of researchers. More specifically, we are willing to assume that the university rank and productivity of the author are jointly independent of the assignment process to the double-blind reviewing procedure. Under these simplifying assumptions we obtain

$$Q_2 = \text{plim} \frac{\sigma_5}{\sigma_2} \cdot \frac{r_{25} - r_{23} r_{35}}{1 - r_{23}^2 - r_{24}^2}. \tag{9}$$

Further, note that independence of x_4 and (x_3, x_5) implies that (1) x_4 is independent of x_3, and (2) x_4 is independent of x_5 conditional on x_3, i.e., $x_5 | x_3$. The first condition guarantees $r_{23} = \sigma_3 \cdot \frac{\text{E}(x_4)}{\text{S.D.}(x_3 \cdot x_4)}$, where $\text{E}(x_4)$ is the expected value of x_4 and $\text{S.D.}(x_3 \cdot x_4)$ is the standard deviation of $x_3 \cdot x_4$. The second condition (in combination with the outcome of the first condition) insures $r_{25} = r_{23}r_{35}$. Thus, under the assumption of independence between x_4 and (x_3, x_5), $\frac{r_{25} - r_{23}r_{35}}{1 - r_{23}^2 - r_{24}^2} = 0$ and Equation (6) simplifies to

$$\text{plim}(\widehat{\beta}_2) = \beta_2. \tag{10}$$

Equation (10) implies that the coefficient estimate on the interaction term from model (5) is consistent under independence of x_4 and (x_3, x_5).

It is worth pointing out that independence of x_4 and (x_3, x_5) is stronger than necessary to guarantee this result.[8] To get Equation (10), it would be sufficient to have either $f(x_3|x_5, x_4) = f(x_3|x_5)$ or $f(x_5|x_3, x_4) = f(x_5|x_3)$ in combination with x_4 being independent of either x_5 or x_3, respectively. The conditional independence of x_4 from either x_3 or x_5 given x_5 or x_3, respectively, is weaker than the full independence. In other words, the conditional independence is implied by the full independence but not vice versa.[9]

Let us revisit the study by Blank (1991). The main question of interest there is estimating the differences in the effect of the double-blind reviewing procedure for different groups of researchers. The author is after the coefficient estimate of the interaction term between the rank of the university and variable identifying the sample randomly assigned to the double-blind reviewing. While there are valid reasons to be concerned that the university rank is correlated with the unobservables (say, productivity of the author), the treatment effect under the study is independent of the university rank as well as productivity of the authors once rank is accounted for. These two independences guarantee that the OLS estimates of the interaction terms between university rank and treatment dummies are consistent as we show above.

3 Inclusion of Additional Explanatory Variables

In practice, we virtually never encounter a situation when only three explanatory variables (in addition to the constant term) are included into a regression equation. In this section we extend our theoretical findings from the previous section to the case when other explanatory variables are added to Equation (1). Generally, we know a standard textbook fact that term **Q** in the (modified) omitted variable bias formula (3)

[8] It is this strong form of the condition guaranteeing unbiasedness and consistency of the OLS estimates that seems to be implied in Blank (1991) when suggesting that the coefficients on the interaction terms are "robust to the inclusion of any other variables in the model, since they come from two experimental samples that are identical in all other characteristics" (Blank 1991, p. 1054).

[9] An exception is the case of joint normality of variables. If (y, z, w) is normally distributed then y is independent of (z, w) implies y is independent of z and y is independent of $w|z$, *and vice versa*.

involves multiple regression coefficients, which have the signs of partial, not simple, correlations among the excluded and included variables. Thus, including additional explanatory variables that are partially uncorrelated with excluded relevant regressors has no effect on consistency of all the OLS estimates.

We employ simulations to generalize our discussion in the previous section to models with more explanatory variables, and to illustrate how OLS estimates will behave under different assumptions. To do so, we continue exploiting the example of the AER experiment. We use Monte Carlo simulations to draw the data and check the properties of the OLS estimators. The number of replications is 1000, and the results of the experiment are presented for cross-sectional sample sizes of 100 and 1000. We use Equation (1) augmented by additional explanatory variables to describe a simplified relation between the acceptance rate and double-blind treatment. Specifically, we generate the relation between the dependent and explanatory variables to be:

$$y_i = 1 + 2r_i \cdot d_i + 3r_i + 4d_i + 5f_i + 6s_i + 7n_i + 8c_i + u_i. \tag{11}$$

Here, r_i and u_i are generated as independent $\text{Normal}\ (0, 1)$. These variables represent university rank and the idiosyncratic error, respectively. The unobserved individual-specific effect, c_i, is generated as $c_i \equiv \lambda r_i + e_i^c$, where $e_i^c \sim \text{Normal}\ (0, 1)$. The explanatory variable d_i is generated as a binary variable and it is meant to represent some exogenous treatment, which is independent of the university rank.

Additional explanatory variables can include author's gender, rank of school granting the doctorate, and gender of the referee. We consider three possibilities for additional explanatory variables: (1) a variable independent of the unobservable omitted variable (for example gender of the referee), (2) a variable correlated with the treatment but uncorrelated with the unobservable omitted variable[10], (3) a variable for which the simple correlation with the unobservable omitted regressor is different from zero (gender of the author). To check these possibilities, we generate r_i as a binary indicator of whether the referee is female and set $s_i = \gamma d_i - 1 + e_i^s$, where $e_i^s \sim$ Discrete $\text{Uniform}\ (0, 3)$. In simulations we choose $\gamma = 0.5$. The way we generate both s_i and r_i reflects situations when the omitted variable is independent of additional included regressors. Finally, we consider two data generating processes (DGPs) for n_i – the rank of the school granting doctorate to the author[11] (Davis and Patterson 2001): (A) $n_i = \alpha r_i + e_i^n$, and (B) $n_i = \alpha c_i + e_i^n$, where $e_i^n \sim \text{Normal}\ (0, 1)$ for both cases. In simulations we set $\alpha = 0.5$. These two

[10] It is difficult to think of an example of such variable in the AER experiment setting, but in general the possibility of having such variables is quite high in the policy evaluations.

[11] In case of multiple authors, this can be measured by the highest rank of the schools granting doctorate among all co-authors.

DGPs for n_i result in non-zero simple correlation between the omitted variable, c_i, and additional included regressor, n_i. However, the partial correlation between n_i and c_i, i.e., correlation net of the effect of the other included regressors (in particular, r_i) is zero for DGP (A), while it is clearly not for DGP (B).

Next we proceed to estimation. First, we behave as if we are able to observe the individual-specific effect, c_i and estimate a model when all eight explanatory variables are included into the estimating equation. Second, while the population model is still captured by Equation (11), we estimate a regression with only seven explanatory variables included to reflect situations when researchers are not able to observe the individual-specific effect:

$$y_i = \beta_1 + \beta_2 r_i \cdot d_i + \beta_3 r_i + \beta_4 p_i + \beta_5 f_i + \beta_6 s_i + \beta_7 n_i + u_i^*, \tag{12}$$

where $u_i^* = u_i + \beta_8 c_i$.

Tables 1 and 2 present simulation results for $N = 100$ and $N = 1000$ when $\lambda = 0.5$ and $\lambda = 0$, respectively. Odd columns report results for the estimating equation with seven regressors, while even columns – for the estimating equation with eight regressors. Rows (1) through (6) contain means of OLS slope estimates and their corresponding standard errors from 1000 replications. Rows (7) through (11) contain the root mean squared error (RMSE), standard deviation (SD), lower quartile (LQ), median, and upper quartile (UQ) for $\widehat{\beta}_2$ – our main coefficient of interest – from 1000 replications. Also, the first four columns report the results when n_i is generated according to DGP (A), while the last four columns – according to DGP (B).

When $\lambda = 0$, $\mathrm{Corr}(w_i, c_i) = 0$ and OLS estimation delivers consistent estimates of all model parameters for both regressions considered, as long as the unobserved heterogeneity in Equation (12) is partially uncorrelated with all of the additional included regressors. Indeed, from Table 2 we see that the OLS estimates of β_2, β_4 and β_7 are consistent for both models considered for both values of λ selected when DGP (A) is used to generate n_i. As expected, even when $\lambda = 0$, $\widehat{\beta}_7$ is inconsistent when we use DGP (B) to generate n_i, since the partial correlation between the unobserved heterogeneity and n_i is not zero in that case. Contrary, the OLS estimates of β_3 from the model with seven regressors are consistent only when $\lambda = 0$ regardless of the sample size and DGP used to generate n_i. The fact that s_i is correlated with d_i has no effect on any of the OLS estimates for all possible scenarios, since both of these variables are independent of the unobserved heterogeneity. Similarly, $\widehat{\beta}_5$ is always consistent.

Clearly, when eight regressors are included all estimates are unbiased and consistent. More importantly, when only seven regressors are used, the OLS estimates of β_2 and β_4 are still consistent, while the consistency of $\widehat{\beta}_3$ and $\widehat{\beta}_7$ depends on the (partial) correlations between r_i and c_i and between n_i and c_i, respectively. The simulation findings are unambiguous: when the partial correlation between the unobserved heterogeneity and some included regressor is different from zero, the OLS slope estimate of that included regressor is the only estimate which is inconsistent, and its bias does not disappear as $N \longrightarrow \infty$.

Not surprisingly, the OLS estimates of all parameters when eight relevant regressors are used have smaller standard errors than the OLS estimates when only seven regressors are available due to a smaller error variability in the former case. The RMSEs for $\widehat{\beta}_2$ in all cases are (almost) identical to the corresponding SDs. Once again, this is hardly surprising, as $\widehat{\beta}_2$ is consistent in all cases considered.

4 Conclusions

Increasing interest in the heterogeneity of the impact in policy evaluation and random experiment settings leads to a question of whether the estimates are consistent when the source of heterogeneity is correlated with some omitted variable(s). This paper presents the conditions under which it is possible to arrive at a consistent OLS estimate of the mentioned effect. We explicitly show that if the source of heterogeneity and omitted variable are jointly independent of the policy/treatment, then the OLS estimate on the interaction term between the treatment and endogenous factor turns out to be consistent.

We discuss the relevant applications and provide simulation evidence for the OLS estimator of the interaction term between the exogenous treatment and endogenous factor. It turns out that even in the case when more than four regressors (constant term, treatment effect, regressor correlated with the omitted variable, and an interaction term) of different nature are included, the estimates of the main treatment effect and the coefficient on the interaction term can still be unbiased and consistent. To be precise, the simulation findings suggest that when the partial correlation between the omitted variable and some included regressor is different from zero, the OLS slope estimate of that regressor is the only estimate which is biased and inconsistent.

This paper provides an important formal proof of the validity of the estimates of the heterogeneous impact when the source of heterogeneity is correlated with the omitted variable(s).

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Table 1: OLS Estimation Results for $(\beta_2, \beta_3, \beta_4, \beta_5, \beta_6, \beta_7)' = (2, 3, 4, 5, 6, 7)'$ and $\lambda = 0.5$.

# of Regressors:		7	8	7	8	7	8	7	8
		$N = 100$		$N = 1000$		$N = 100$		$N = 1000$	
		(A): $n_i = 0.5r_i + e_i^n$				(B): $n_i = 0.5c_i + e_i^n$			
(1)	$\widehat{\beta}_2$	1.959	1.978	2.010	2.003	1.962	1.978	2.013	2.003
	SE($\widehat{\beta}_2$)	(1.677)	(0.210)	(0.512)	(0.064)	(1.502)	(0.210)	(0.459)	(0.064)
(2)	$\widehat{\beta}_3$	7.007	3.004	6.994	3.000	6.228	3.007	6.192	2.999
	SE($\widehat{\beta}_3$)	(1.253)	(0.165)	(0.384)	(0.050)	(1.071)	(0.157)	(0.329)	(0.048)
(3)	$\widehat{\beta}_4$	4.051	4.006	3.980	3.996	4.058	4.006	3.990	3.996
	SE($\widehat{\beta}_4$)	(1.688)	(0.211)	(0.524)	(0.065)	(1.512)	(0.211)	(0.470)	(0.065)
(4)	$\widehat{\beta}_5$	4.952	5.005	5.033	5.001	4.967	5.005	5.032	5.001
	SE($\widehat{\beta}_5$)	(1.652)	(0.206)	(0.511)	(0.063)	(1.479)	(0.206)	(0.458)	(0.063)
(5)	$\widehat{\beta}_6$	6.000	6.001	5.998	5.998	6.003	6.001	5.993	5.998
	SE($\widehat{\beta}_6$)	(0.739)	(0.092)	(0.229)	(0.028)	(0.661)	(0.092)	(0.205)	(0.028)
(6)	$\widehat{\beta}_7$	7.028	7.007	6.993	6.999	10.193	7.007	10.190	6.999
	SE($\widehat{\beta}_7$)	(0.833)	(0.104)	(0.256)	(0.032)	(0.666)	(0.104)	(0.205)	(0.032)
(7)	RMSE($\widehat{\beta}_2$)	1.700	0.210	0.505	0.064	1.552	0.210	0.450	0.064
(8)	SD($\widehat{\beta}_2$)	1.701	0.209	0.506	0.064	1.552	0.209	0.450	0.064
(9)	LQ($\widehat{\beta}_2$)	0.795	1.836	1.674	1.961	0.967	1.836	1.693	1.961
(10)	Median($\widehat{\beta}_2$)	1.958	1.972	2.008	2.002	1.982	1.972	2.000	2.002
(11)	UQ($\widehat{\beta}_2$)	3.197	2.108	2.356	2.044	3.032	2.108	2.319	2.044

Table 2: OLS Estimation Results for $(\beta_2, \beta_3, \beta_4, \beta_5, \beta_6, \beta_7)' = (2, 3, 4, 5, 6, 7)'$ and $\lambda = 0$.

# of Regressors:		7	8	7	8	7	8	7	8
		$N = 100$		$N = 1000$		$N = 100$		$N = 1000$	
		(A): $n_i = 0.5r_i + e_i^n$				(B): $n_i = 0.5c_i + e_i^n$			
(1)	$\widehat{\beta}_2$	1.959	1.978	2.010	2.003	1.962	1.978	2.013	2.003
	SE($\widehat{\beta}_2$)	(1.677)	(0.210)	(0.512)	(0.064)	(1.502)	(0.210)	(0.459)	(0.064)
(2)	$\widehat{\beta}_3$	3.007	3.007	2.994	3.000	3.026	3.010	2.990	2.999
	SE($\widehat{\beta}_3$)	(1.253)	(0.156)	(0.384)	(0.048)	(1.058)	(0.148)	(0.325)	(0.045)
(3)	$\widehat{\beta}_4$	4.051	4.006	3.980	3.996	4.058	4.006	3.990	3.996
	SE($\widehat{\beta}_4$)	(1.688)	(0.211)	(0.524)	(0.065)	(1.512)	(0.211)	(0.470)	(0.065)
(4)	$\widehat{\beta}_5$	4.952	5.005	5.033	5.001	4.967	5.005	5.032	5.001
	SE($\widehat{\beta}_5$)	(1.652)	(0.206)	(0.511)	(0.063)	(1.479)	(0.206)	(0.458)	(0.063)
(5)	$\widehat{\beta}_6$	6.000	6.001	5.998	5.998	6.003	6.001	5.993	5.998
	SE($\widehat{\beta}_6$)	(0.739)	(0.092)	(0.229)	(0.028)	(0.661)	(0.092)	(0.205)	(0.028)
(6)	$\widehat{\beta}_7$	7.028	7.007	6.993	6.999	10.193	7.007	10.190	6.999
	SE($\widehat{\beta}_7$)	(0.833)	(0.104)	(0.256)	(0.032)	(0.666)	(0.104)	(0.205)	(0.032)
(6)	RMSE($\widehat{\beta}_2$)	1.700	0.210	0.505	0.064	1.552	0.210	0.450	0.064
(7)	SD($\widehat{\beta}_2$)	1.701	0.209	0.506	0.064	1.552	0.209	0.450	0.064
(8)	LQ($\widehat{\beta}_2$)	0.795	1.836	1.674	1.961	0.967	1.836	1.693	1.961
(9)	Median($\widehat{\beta}_2$)	1.958	1.972	2.008	2.002	1.982	1.972	2.000	2.002
(10)	UQ($\widehat{\beta}_2$)	3.197	2.108	2.356	2.044	3.032	2.108	2.319	2.044