Testing for Bias from Censored Regressors

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Abstract

We derive tests for the presence of bias from using censored regressors in linear regression analysis. The test follows from the principles of (Hausman) specification tests, and is applicable in situations of exogenous censoring. We apply the test in two substantive empirical applications; the estimation of the effects of financial wealth on household consumption, and the estimation of the impact of foreign denominated debt on firm investment decisions. In each application we find strong rejection of the absence of censoring bias.

1. Introduction

Estimating a linear model with a censored regressor implies bias in the coefficient estimates. This is true regardless of whether the regressor is exogenous in a standard regression model, or endogenous with estimation using instrumental variables. As we have discussed elsewhere (Rigobon and Stoker (2005a)), quite different biases can arise in these cases, although it is difficult to find any cases where using a censored regressor does not induce any bias.[1]

In this paper, we present tests of the presence of bias from using a censored regressor. We focus on the case of exogenous censoring, as defined below. This provides structure that facilitates standard approaches to specification tests, as we spell out.[2]

Censoring is a very prevalent phenomena with observed data on financial variables. To understand the practical role of our test, it is useful to consider the two main sources of censoring of regressors. The first source is censoring that arises because of inadequate measurement or reporting. For instance, household survey data may contain observations of family income up to a bound of (say) \$100,000, with higher values recorded as "\$100,000 or higher." An upper bound of this sort (top-coding) and/or a lower bound (bottom-coding) is very common for financial variables. In addition, observations may

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[1] One exception occurs with a bivariate model with an exogenous regressor, with random censoring to the regressor mean. In this case, the OLS coefficient will not be biased.

[2] There is a very small literature discussing biases from censored regressors, let alone tests for their absence. One exception is Nicoletti and Peracchi (2005), who develop tests for the adequacy of data imputation in a GMM framework.

be assigned to a censoring value in a less systematic way than indicated by bounding. For instance, zero recording of stock market wealth could indicate zero holdings, or might arise for a household that was unwilling to disclose their true holdings. Random non-reporting, or other related difficulties give rise to censoring. All these problems cause bias in estimation. Our tests will gauge how seriously such censoring has affected the empirical results.

The second source of censoring arises when one wants to control for an economic or financial concept that is not observed directly. The observed data represents bounded or otherwise censored versions of the actual concept of interest. For a concrete example, suppose that one is studying whether cash constraints affect the amount of investing done by individual firms. One doesn't observed "extent of cash constraints." Suppose that one uses the new debt issued by a firm as an imperfect proxy, since new debt represents a recent infusion of cash, and therefore would vary inversely with cash constraints. However, new debt represents a censored version of "cash constraints," since negative values are never observed. Zero values of new debt could reflect either mild cash constraints or severe cash constraints. The same can be said for proxying cash constraints by total outstanding debt. Our tests will judge the impact of this kind of censoring on the results. With this example, the question addressed by our tests is whether new debt is the appropriate variable of interest, or whether it is a censored version of the appropriate "extent of cash constraints."

Our tests are based on comparing estimates computed from the whole data sample with estimates computed from the subsample where all censored observations have been deleted. This comparison is a good starting point for investigating the impact of censoring, and is formally justified under *exogenous censoring* as defined below. If there is a significant difference in the estimates, then there is evidence that censoring has biased the empirical findings.

Section 2 begins with a more detailed motivation of our testing procedure, followed by the test statistics and related analytics. With linear regression, the tests are straightforward, but it is important to consider details carefully. We also note how the procedures extend to IV estimation and situations with heteroskedasticity. In Section 3, we use the tests in two substantial applications. First is estimating the marginal propensity to consume out of financial wealth. Second is studying the impact of foreign denominated debt in investment decisions. We find substantial impacts of censoring in each of these applications. Section 4 gives some simulation results on the power of the tests, and Section 5 contains some concluding remarks.

2. Testing for Bias from Censored Regressors

2.1. Motivation

We now motivate our testing procedure with a stylized version of an application we pursue in Section 3. Assume we are interested in estimating the impact of exchange rate devaluation on the investment behavior of firms. Now, it is clear that the effect of the exchange rate depends on the exposure that a firm has to exchange rate risk. A firm that has net liabilities indexed or denominated in foreign currency clearly will experience a negative shock after that currency is devalued, and that is true even for firms that do not export or have assets in that foreign currency. Such a depreciation implies a deterioration of the balance sheet that would limit the possibilities of investment for the firm. Alternatively, a firm that is actively exporting or has net assets in foreign currency may experience an improvement in its balance sheet after a devaluation, which may enhance investment possibilities.

However, how do we actually measure the firm level exposure to exchange rate risk? By looking at the liabilities denominated in foreign currency, or assets? What about exports, or imports of raw materials? What about contracts that have their payments indexed to the foreign currency? Finally, what about financial instruments, such as swaps, that can be affected by exchange rate movements but are out of the balance sheet? In practice, researchers take various values from reported financial statements and use them as proxies of the true exchange rate exposure. This can induce noise as with standard errors-in-variables structure, but can also induce problems from censoring. For example, suppose we take liabilities in foreign currency as a proxy for the exposure to exchange rate risk. The idea is that firms with higher liabilities have higher exposure to the exchange rate. This is true enough, but the problem is that in financial statements liabilities in foreign currency appear with a lower bound of zero when the actual exposure might be different. Indeed, all firms that have negative net liabilities in foreign currency will record a value of zero in the observed proxy. In fact, the use of values from reported financial statements is plagued by this type of mismeasurement.

The situation of bias from a censored regressor is illustrated in Figure 1. With our example, suppose that the x-axis is the debt denominated in foreign currency; while the y-axis is investment. Here, firm investment decisions respond to the true exchange rate exposure (represented by the darker points). We don't measure the true exposure values, but instead proxy them by using debt in foreign currency, so that our data is bound censored at zero (the lighter points). Clearly, the regression with the observed data is biased (the lighter line), with a greater effect estimated than the true regression (the darker line). Now, under certain conditions we can drop the censored data points, and get consistent estimates of the true regression with the remaining data points (often called the "complete cases"). We cover those conditions below, but in any case, it is clear that the use of the censored regressors causes bias.

Contrast this with the situation where the debt in foreign currency is the properly specified regressor after all, or where the observed zeros in the data are "true" zeros. This situation is depicted in 2. Now, the variable represented on the x-axis appears censored, but the investment decision actually depends on the observed variable itself (and not on a variable that it is proxying for). Now, we estimate the same effect whether the observations with zero values are included or excluded. That is, including the (apparently) censored observations will not significantly affect the estimates.[3]

Our test is based on comparing estimates from the full data sample with those obtained when the censored observations are dropped. Rejection means that censoring has induced bias, which would lead one to look much more closely at the censoring process, and to use alternative methods to solve

[3]For an example where zeros are naturally thought of as "true" zeros, consider estimating the behavior of the cash reserves in the banking sector. It is a common practice of bank regulation that a bank must hold larger cash reserves the worse its asset position is. A common practice of determining the quality of the assets is that banks use rating agencies. If rating agencies make mistakes, then the credit rating is a noisy and censored measure of the true riskiness of the assets. However, that noise is irrelevant from the perspective of the cash reserves. If the asset is investment grade, the bank does not set aside additional reserves, but if it is non-investment grade then it has to hold additional cash. That is, cash reserves are affected by the credit rating, not the underlying asset riskiness.

the problem of estimation. We now introduce the test formally, followed by its distribution and other analytical features.

2.2. Formal Set-Up and Testing Approach

We consider a situation where we are studying how a response y_i is related to a single regressor x_i and a $k-1$ vector of regressors $w_i^{'} = \left(1, w_i^{+}\right)$.[4] The regressor x_i takes on a given value ξ with probability p.[5] Define $d_i \equiv 1\left[x_i = \xi\right]$ to indicate when that occurs. We consider the simplest situation, where the observed data $\left\{\left(y_i, x_i, d_i, w_i^{'}\right) \mid i = 1, ..., n\right\}$ is an i.i.d. random sample from a distribution with finite second moments.

The issue of interest is whether x_i is the correctly specified regressor or whether it is a censored version of the correct regressor. That is, the null hypothesis is that data is consistent with the regression model specified as

$$y_i = \delta x_i + \gamma^{'} w_i + \varepsilon_i \quad i = 1, ..., n \tag{H_0}$$

with $E\left(\varepsilon|x, w\right) = 0$. For our basic tests, we add the assumption of homoskedasticity, namely $E\left(\varepsilon^2|x, w\right) = \sigma^2$.

The alternative hypothesis is associated with the regression model specified as

$$y_i = \delta x_i^* + \gamma^{'} w_i + \varepsilon_i \quad i = 1, ..., n \tag{H_1}$$

with $E\left(\varepsilon|x^*, w\right) = 0$. Here the observed variable x_i is a censored version of the true (but unobserved) regressor x_i^*, as in

$$x_i = \left(1 - d_i\right) x_i^* + d_i \xi. \tag{1}$$

where d_i is the censoring indicator, with $\Pr\left\{d = 1\right\} = p$. If (H_1) is the correct specification, then the OLS coefficients of y regressed on x and w will be biased as estimators of δ and γ, as discussed in Rigobon and Stoker (2005a).[6]

To facilitate our testing approach, we add the following assumption to the alternative model (H_1):

Assumption: Exogenous Censoring. In model (H_1), we have that

$$E\left(\varepsilon|x^*, w, d\right) = 0 \tag{2}$$

[4] Note that the intercept is included within w_i.

[5] For instance, if x_i is bounded below by $\xi = 0$, then p is the percentage of observed values of x_i that hit the bound 0. Our framework is not restricted to bounds – we could have various types of random censoring to the value ξ.

[6] With regard to our motivation, y is firm investment, x^* is exposure to exchange rate risk, x is liabilities in foreign currency and w are other controls. The censoring value ξ is zero, and d indicates which observations have zero liabilities in foreign currency.

Exogenous censoring implies that censoring is not systematically related to the value of the response y under study.[7]

For notation, suppose that the $\{y_i\}$ data is collected in the $n \times 1$ matrix Y and the $\left\{\left(x_i, w_i^{'}\right)\right\}$ data is collected in the $n \times k$ matrix X. The data is sorted; the n_0 observations with $d_i = 0$ are first and the n_1 observations with $d_i = 1$ are last. The matrices are partitioned accordingly as

$$Y = \begin{bmatrix} Y_0 \\ Y_1 \end{bmatrix}; \quad X = \begin{bmatrix} X_0 \\ X_1 \end{bmatrix} = \begin{bmatrix} x_0 & W_0 \\ \xi \cdot \iota & W_1 \end{bmatrix} \tag{3}$$

where x_0, W_0, W_1 are the appropriate matrices of observations on x and w, and ι is the n_1-vector of ones. We assume that $X_0^{'}X_0$ and $W_1^{'}W_1$ are nonsingular.

Denote the regression coefficients compactly as $\beta = \left(\delta, \gamma^{'}\right)^{'}$. Define two estimators of β as follows; first utilizing the full sample of data,

$$\tilde{b} = \left(X_0^{'}X_0 + X_1^{'}X_1\right)^{-1}\left(X_0^{'}Y_0 + X_1^{'}Y_1\right) \tag{4}$$

and second utilizing only the data points with with $d_i = 0$ (the non-censored observations),

$$\hat{b}_0 = \left(X_0^{'}X_0\right)^{-1} X_0^{'}Y_0 \tag{5}$$

Denote the probability limits of these estimators as

$$\text{plim } \tilde{b} = \tilde{\beta} \tag{6}$$

and

$$\text{plim } \hat{b}_0 = \hat{\beta}_0 \tag{7}$$

Our testing approach is based on differences in the two regression estimators (4) and (5). Under the null hypothesis, and the exogenous censoring assumption (2), it is clear that

$$\hat{\beta}_0 = \tilde{\beta} = \beta, \tag{8}$$

since x_i is correctly measured and exogenous. We test (8) against

$$\hat{\beta}_0 \neq \tilde{\beta} \tag{9}$$

Under these assumptions, $\hat{b}_0$ is a consistent estimator of β under both the null and alternative hypotheses. Clearly under (H_0), there is no bias induced by taking a subsample, since x_i is correctly observed and exogenous. Under the alternative (H_1), condition (2) implies that there is no bias induced

[7]This is in line with the examples we have listed above: the top coding of income and zero debt in the financial statements, etc., which do not depend on household consumption decisions or investment decisions respectively.

by truncating the sample to those points with $d_i = 0$, namely to the uncensored data (or complete cases). We have $\hat{\beta}_0 = \beta$, the true value, under both the null and alternative.[8]

Under the null (H_0), the full sample regression $\tilde{b}$ is a consistent estimator of β, with $\tilde{\beta} = \beta$. Under the alternative (H_1), $\tilde{\beta}$ will typically contain biases, as detailed in Rigobon and Stoker (2005a), with $\tilde{\beta} \neq \beta$. Therefore, testing (8) against (9) is a test of whether biases have been induced by using a censored regressor.

2.3. The Test Statistic and Equivalents

We can immediately propose a test statistic by noting that under (H_0), $\tilde{b}$ is a consistent *and* efficient estimator of β. Therefore, we appear to have all the ingredients for a classical specification test as established by Hausman (1978). Namely, under the null hypothesis $\tilde{b}$ is efficient and $\hat{b}_0$ is consistent, and under the alternative hypothesis, $\tilde{b}$ is inconsistent and $\hat{b}_0$ is consistent. This suggests forming a (Wald) test as

$$\tilde{H} = \left(\hat{b}_0 - \tilde{b}\right)' \hat{V}_b^{-1} \left(\hat{b}_0 - \tilde{b}\right) \tag{10}$$

where $\hat{V}_b$ is a consistent estimate of

$$V_b = Var\left(\hat{b}_0\right) - Var\left(\tilde{b}\right) \tag{11}$$

This is the right idea, but a problem arises because under the null, V_b is singular. We can solve this by using a generalized inverse of $\hat{V}_b$, but it is more informative to examine the source of the singularity.

For this, note that we can set the (censoring) value $\xi = 0$ without loss of generality.[9] Therefore, we have $x_i = 0$ when $d_i = 1$, or in matrix form, $X_1 = [0, W_1]$. The singularity arises because we cannot identify δ using the data with $d_i = 1$.

Taking this further, note that the full sample estimator $\tilde{b}$ can be written in pooled form as

$$\tilde{b} = \left(X_0'X_0 + X_1'X_1\right)^{-1} X_0'X_0\hat{b}_0 + \left(X_0'X_0 + X_1'X_1\right)^{-1} \begin{bmatrix} 0 & 0 \\ 0 & W_1'W_1 \end{bmatrix} \begin{bmatrix} 0 \\ \hat{c}_1 \end{bmatrix} \tag{12}$$

where

$$\hat{c}_1 = \left(W_1'W_1\right)^{-1} W_1'Y_1 \tag{13}$$

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[8] If censoring is not exogenous, then dropping points where $d_i = 1$ can induce bias from truncating the dependent variable y, as familiar from models of endogenous sample selection. That is, we could induce $\hat{\beta}_0 \neq \tilde{\beta}$ under the null hypothesis. We need to rule that out.

[9] If ξ is nonzero, note that we can rewrite (H_0) and (H_1) equivalently in terms of $x_i^+ = x_i - \xi$, with the intercept shifted by $\delta\xi$.

is the estimator of the $k-1$ vector γ The difference $\hat{b}_0 - \tilde{b}$ is then

$$\begin{aligned} \hat{b}_0 - \tilde{b} &= \left(X_0^{'}X_0 + X_1^{'}X_1\right)^{-1} \begin{bmatrix} 0 & 0 \\ 0 & W_1^{'}W_1 \end{bmatrix} \left\{ \hat{b}_0 - \begin{bmatrix} 0 \\ \hat{c}_1 \end{bmatrix} \right\} \\ &= A\left\{\hat{c}_0 - \hat{c}_1\right\} \end{aligned} \tag{14}$$

where $\hat{c}_0$ is the $k-1$ subvector of $\hat{b}_0$ that estimates γ, with

$$A = \left(X_0^{'}X_0 + X_1^{'}X_1\right)^{-1} \begin{bmatrix} 0 \\ W_1^{'}W_1 \end{bmatrix} \tag{15}$$

The expression (14) is interesting for several reasons. First, the k vector difference $\hat{b}_0 - \tilde{b}$ is determined by the $k-1$ vector $\hat{c}_0 - \hat{c}_1$; again, this is the source of the singularity of the covariance of $\hat{b}_0 - \tilde{b}$. Second, since $W_1^{'}W_1$ is nonsingular, the matrix A is of full rank $k-1$, so that inference on the basis of $\hat{b}_0 - \tilde{b}$ is equivalent to inference on the basis of $\hat{c}_0 - \hat{c}_1$. Therefore, the Hausman procedure leads to the test statistic

$$\hat{H} = \left(\hat{c}_0 - \hat{c}_1\right)^{'} \hat{V}_c^{-1} \left(\hat{c}_0 - \hat{c}_1\right) \tag{16}$$

where $\hat{V}_c$ is a consistent estimator of $Var\left(\hat{c}_0 - \hat{c}_1\right)$, namely

$$\hat{V}_c = s^2 \left[\left(W_0^{'} \left(I - x_0 \left(x_0^{'}x_0 \right)^{-1} x_0^{'} \right) W_0 \right)^{-1} + \left(W_1^{'}W_1 \right)^{-1} \right] \tag{17}$$

where s^2 is a consistent estimator of σ^2.

But further, this development reveals our test of censored regressor bias to be a classical Chow test on the stability of the estimates of γ across the 0 sample and the 1 sample. This applies whether the (censoring) value $\xi = 0$ or not. We can assert standard equivalence results (c.f. Chow (1960) and Fisher (1970)) to get a residual based statistic that is a trivial matter to compute. Define the following residual vectors:

$$\tilde{u} = Y - X\tilde{b}, \qquad u_0 = Y_0 - X_0\hat{b}_0, \qquad u_1 = Y_1 - W_1\hat{\gamma}_1 \tag{18}$$

Then if

$$\hat{F} \equiv \left[\frac{\left(\tilde{u}'\tilde{u} - \left(u_0^{'}u_0 + u_1^{'}u_1 \right) \right)}{(k-1)} \right] \Bigg/ \left[\frac{u_0^{'}u_0 + u_1^{'}u_1}{n - (2k+1)} \right] \tag{19}$$

we have

$$\hat{H} = (k-1) \cdot \hat{F} \tag{20}$$

provided (17) uses the estimate $s^2 \equiv \left(u_0^{'}u_0 + u_1^{'}u_1 \right) \Big/ \left(n - (2k+1) \right).$

It is clear how our test statistic is distributed under the null hypothesis. Under homoskedasticity $\left(E\left(\varepsilon^2|x,w\right) = \sigma^2\right)$, we have that $\hat{H}$ is asymptotically $\chi^2(k-1)$. If we add normality $\left(\varepsilon_i \sim \mathcal{N}\left(0,\sigma^2\right)\right)$, then $\hat{F}$ is distributed as $F\left(k-1, n-(2k+1)\right)$. Tests are carried out as in the most standard of regression testing situations. The simplicity of the test implies that it can be implemented with any standard software

2.4. Several Censored Regressors

The clarity of the testing structure permits an immediate extension to situations where several regressors are censored. Consider first the case where there are two censored regressors x_1 and x_2. To be precise, the null hypothesis is that data is consistent with the regression model specified as

$$y_i = \delta_1 x_{1i} + \delta_2 x_{2i} + \gamma^{'} w_i + \varepsilon_i \quad i = 1, ..., n \tag{H_0}$$

with $E(\varepsilon|x_1, x_2, w) = 0$ and $E(\varepsilon^2|x_1, x_2, w) = \sigma^2$ and $w_i^{'} = (1, w_i^{+})$ is now a $k-2$ vector. The alternative hypothesis is associated with the regression model specified as

$$y_i = \delta_1 x_{1i}^{*} + \delta_2 x_{2i}^{*} + \gamma^{'} w_i + \varepsilon_i \quad i = 1, ..., n \tag{H_1}$$

with $E(\varepsilon|x_1^*, x_2^*, w) = 0$, where the observed variables x_{1i}, x_{2i} are censored versions of the true (but unobserved) regressors x_1^*, x_2^*, as in

$$x_{1i} = (1 - d_{1i})\, x_{1i}^{*} + d_{1i}\xi_1 \tag{21}$$

$$x_{2i} = (1 - d_{2i})\, x_{2i}^{*} + d_{2i}\xi_2 \quad . \tag{22}$$

d_{1i}, d_{2i} are the censoring indicators as before, and ξ_1, ξ_2 are the censoring values.

Testing for bias from censored regressors again is based on a comparison of coefficients from subsamples with censored data to those from the full data sample. But with two regressors, there are now four subsamples to consider; sample 00 of complete cases with $d_{1i} = 0, d_{2i} = 0$ sample 10 with $d_{1i} = 1, d_{2i} = 0$, (or x_1 censored but x_2 not), sample 01 with $d_{1i} = 0, d_{2i} = 1$, (or x_2 censored but x_1 not) and sample 11 with $d_{1i} = 1, d_{2i} = 1$ (both censored).

Absence of censoring bias implies that the identified coefficients in each subsample are all the same. Using the above logic, there are k identified coefficients in subsample 00, $k-1$ in subsample 10, $k-1$ in subsample 01 and $k-2$ in subsample 11. That is, in the unrestricted specification, the total number of coefficients estimated is

$$k^* = k + (k-1) + (k-1) + (k-2) = 4k - 4 \tag{23}$$

and the number of coefficient restrictions associate with the absence of censoring bias is

$$k^{**} = k^* - k = (k-1) + (k-1) + (k-2) = 3k - 4 \tag{24}$$

Denote the sum-of-square residuals from the full sample as $\tilde{u}'\tilde{u}$ as above, and from subsample 00 as

SSR_{00}, from 10 as SSR_{01}, etc. , and form

$$\hat{F} \equiv \left[\frac{(\tilde{u}'\tilde{u} - (SSR_{00} + SSR_{01} + SSR_{10} + SSR_{11})}{k^{**}}\right] \Bigg/ \left[\frac{SSR_{00} + SSR_{01} + SSR_{10} + SSR_{11}}{n - k^{*}}\right] \tag{25}$$

This F statistic provides the test, with $\hat{H} = k^{**}\hat{F}$ distributed as asymptotically $\chi^2(k^{**})$ under the null hypothesis. With normality ($\varepsilon_i \sim \mathcal{N}(0, \sigma^2)$), then $\hat{F}$ is distributed as $F(k^{**}, n - k^{*})$. This logic extends to any number of censored regressors, with care taken to base the degrees of freedom on the number of identified regressors.

It is clear that zero bias from censored regressors means that the coefficient estimates should not differ significantly across any of the subsamples. In particular, we can do a quick test based on comparing estimates with complete cases, or where no variable is at its bound, with the full data set. We call this a "Partial CR Test" below, and it is really an effortless computation in typical empirical applications.[10] The better approach is exemplified by (25), which is to identify all the relevant subsamples (that is, data segments defined by subsets of variables at their bounds, etc.), and test coefficient stability over all segments. We refer to this as a "Full CR Test" below. We illustrate both of these tests in the empirical applications of Section 3 below.

We now consider two simple variations, namely to situations of heteroskedasticity and situations where the censored regressor is endogenous.

2.4.1. Heteroskedasticity

Since our test statistic is based on standard regression coefficients, adjustment for unknown heteroskedasticity is easy, say with $E(\varepsilon^2 | x, w) = g(x, w)$. In particular, we use (16) with

$$\hat{V}_c = \left[\hat{V}^*(\hat{c}_0) + \hat{V}^*(\hat{c}_1)\right], \tag{26}$$

where $\hat{V}^*(\hat{c}_0)$, $\hat{V}^*(\hat{c}_1)$ are heteroskedasticity-consistent variance estimators. Under standard conditions, c.f. White (1980), we will have $\hat{H}$ distributed asymptotically $\chi^2(k-1)$ under the null, as before.

2.4.2. Instrumental Variables

The situation where there is an endogenous censored regressor is substantially different than the case of ordinary regression. The biases induced by censoring are of a different character (c.f. Rigobon and Stoker (2005a)), and the amount of information available from the censored data is different when one

[10] Generally, when there are two or more bounded/censored regressors, all the parameters will be identified in both the complete case data segment and the non-complete case data segment, so the Partial CR test is literally a Chow test of coefficient stability across those two groups.

has an instrument than when one does not. Nevertheless, we can propose a test for bias of the same style as that discussed above. We now carry that out, but do not delve further into this case, leaving that for future research (for instance, we make no claims that there are not better tests than the one given below).

Consider the same situation as above (models (H_0) and (H_1)) but where x and possibly w are endogenous. We have a $j \geq k$ vector z of instruments, which can contain components of w, and the observed data $\left\{ \left(y_i, x_i, d_i, w_i^{'}, z_i^{'} \right) \mid i = 1, ..., n \right\}$ is an i.i.d. random sample from a distribution with finite second moments. We assume $E\left(\varepsilon | z\right) = 0$ and as before, we add the (relaxable) assumption of homoskedasticity, namely $E\left(\varepsilon^2 | z\right) = \sigma^2$. We assume that there is nonsingular covariance between (x, w) and a k-subvector of z, conditional on $d = 0$. In line with (3), denote the matrix of observations on z as

$$Z = \left[\begin{array}{c} Z_0 \\ Z_1 \end{array} \right] \tag{27}$$

where Z_0 refers to observations with $d_i = 0$ and Z_1 to those with $d_i = 1$.

Our testing approach is to compare estimators computed with the full sample to those computed on the (potentially uncensored) subset. We require an assumption of exogenous censoring as before: with instruments, the assumption is

Assumption: Exogenous Censoring, with Instruments. In models (H_0) and (H_1), we have that

$$E\left(\varepsilon | z, d\right) = 0. \tag{28}$$

This assumption is stronger than the usual assumption required by instrumental variables estimation, and as before, it rules out dependent variable truncation bias.

Our approach to testing is simple. Denote the TSLS estimator of β using the full sample as

$$\tilde{b}^{IV} = \left(X^{'} Z \left(Z^{'} Z \right)^{-1} Z^{'} X \right)^{-1} \left(X^{'} Z \left(Z^{'} Z \right)^{-1} Z^{'} Y \right) \tag{29}$$

and denote the TSLS estimator of β using only the data points with with $d_i = 0$ as

$$\hat{b}_0^{IV} = \left(X_0^{'} Z_0 \left(Z_0^{'} Z_0 \right)^{-1} Z_0^{'} X_0 \right)^{-1} \left(X_0^{'} Z_0 \left(Z_0^{'} Z_0 \right)^{-1} Z_0^{'} Y_0 \right) \tag{30}$$

The presence of censored regressor bias is indicated by a large value of the difference $\hat{b}_0^{IV} - \tilde{b}^{IV}$. We can detect that with the test statistic

$$\tilde{H}^{IV} = \left(\hat{b}_0^{IV} - \tilde{b}^{IV} \right)^{'} \left(\hat{V}_b^{IV} \right)^{-1} \left(\hat{b}_0^{IV} - \tilde{b}^{IV} \right) \tag{31}$$

Under the null (H_0), $\tilde{H}^{IV}$ is distributed at $\chi^2(k)$. The Appendix contains details on the asymptotic normality of $\left(\hat{b}_0^{IV} - \tilde{b}^{IV}\right)$ as well as the variance estimator $\hat{V}_b^{IV}$.

There does not appear to be the same kind of singularity issue with $\tilde{H}^{IV}$ as with $\tilde{H}$ of (10), because of the alteration of the instruments between (29) and (30).[11] However, as we mentioned, we have not carried out a full analysis of the best ways to detect censored regressor bias in the case with instruments, and so there might be procedures that are better than using $\tilde{H}^{IV}$ of (31).

3. Applications

In this section we apply our test in two applications. The first is to the estimation of the marginal propensity to consume out of financial wealth, which is a standard question of household behavior relevant to macroeconomic policy. The second is the estimation of the impact of foreign denominated debt in firms' investment decisions, which is a typical problem in corporate finance. In these applications, we are mostly concerned with highlighting the situations where censoring is a problem, rather than focusing on estimating the coefficients of a particular specification. The objective is to show how prevalent the censoring problem is, and how it can be detected. Furthermore, we would like to be able to say something about the direction of such bias in the two applications to help improve future estimation.

3.1. Marginal Propensity to Consume out of Financial Wealth

An important question in macroeconomics and monetary policy is the measurement of the marginal propensity to consume out of financial wealth. For example, determining how much the central bank has to increase interest rates after a boom in the stock market depends exclusively on how much aggregate demand will expand when financial wealth increases. Rigobon and Sack (2003) estimated how much the U.S. Federal Reserve (Fed) reacts to a stock market boom and argued that it was in line with a wealth effect of 4 percent, the estimate typically used by the Fed. However, there is little agreement about the actual size of this effect. Some estimates of this marginal propensity are close to zero[12], while others have obtained estimates as large as 10 percent.[13] So, the issue is whether the reaction of the Fed is appropriate or not, or whether their assumption of 4 percent wealth effect is accurate. There are other instances in which knowing the marginal propensity to consume out of financial wealth is important for policy design: property taxes, or taxes on corporate profits, among many others.

The main problem in the estimation of marginal propensity to consume out of financial wealth is that financial information is very incomplete. Individuals that hold large amounts of wealth in financial markets usually have large incomes. But their income is usually mismeasured − top coded − implying

[11] Namely, projection on Z versus projection on Z_0.
[12] See Poterba (2000) and Ludvigson and Steindel. (1999).
[13] See Parker (1999) and all the references therein.

that estimating the effect using only wealth will bias the coefficients upward due to the censoring of the income process. Furthermore, the information on wealth is itself censored − especially in the lower end of the distribution. Typically, there are very few observations that have accurate (uncensored) income and wealth values.

We now study the impact of censored regressors in an application to household consumption and wealth. We follow Parker (1999) closely.[14] The data includes consumption, current income and a computed permanent component of consumption that depends on the cohort in which the household belongs, characteristics of the household (such as retirement status, family size, etc.), and financial information. By construction, in these data the income variables are *not* censored − the observations with top-coded income variables of the original survey have been dropped. Therefore we concentrate on the impact of the censoring of wealth. We observe three financial wealth variables: total wealth, housing wealth, and stock market wealth.

Our analysis is based on estimating wealth effects using a log-form regression equation similar to that estimated by Parker (1999).

$$\ln C_{it} = \alpha + \beta_1 \ln W_{it} + \beta_2 \ln H_{it} + \beta_3 \ln S_{it} + \phi_1 \ln PINC_{it} + \phi_2 \ln INC_{it} + \phi_3' Controls_{it} + \varepsilon_{it} \quad (32)$$

where $C_{i,t}$ is consumption of household i at time t and W_{it}, H_{it}, S_{it} are the measures of total wealth, housing wealth and stock market wealth. There are two income variables; $PINC_{it}$ is a constructed permanent component of income and INC_{it} is the current income, which are uncensored regressors in our data. $Controls_{it}$ are variables accounting for retirement status, family size, cohorts, time, etc. For a detailed description of the data and the definition of the variables, see Parker (1999).

Table 1 shows the configuration of the sample in terms of bounded or censored wealth values.[15] There is a moderate fraction, 27,4%, of total wealth observations at the bound, and this increases to 46% percent for housing wealth and to 76.3% for stock market wealth, so the bounding/censoring phenomena is substantial. There are three 0 cells that require mention. First, the "only W at bound" cell is zero by definition, since since total wealth W is greater than or equal to the sum $H+S$ of housing and stock market wealth, so it is impossible for W to be at the bound without either H or S at the bound. The "only W and H" and "only W and S" cells are not logically constrained to be empty, but are empty in our data. In summary, there are five nonempty data segments to be take account of for the Full CR test. Finally, the "Identified Parameters" column list the number of parameters in the full model equation (32) identified in each nonempty segment.[16]

In Table 2, we present estimates of the wealth elasticities computed using the full sample and the sample of complete cases (no wealth values at the bounds). The "Full Model" estimates are from (32),

[14] We thank Jonathan Parker for his tremendous help and support in providing us not only with the data but with valuable suggestions.

[15] In fact, the censoring is at slighly higher values for internal consistency and to faciliate taking logarithms. In particular, total wealth is censored when it is less than \$5,000, housing wealth when it is less than \$4,000 and stock market wealth when it is less than \$1,000. We also eliminated a few observations where total wealth was recorded as less than the sum of housing and stock market wealth.

[16] We list the sum of all the identified parameters in the segments as the identified parameters in the full sample. The restricted model estimated with the full sample has 23 parameters.

Bounded/Censored	Observations	Percentage	Identified Parameters
Complete Cases	2,192	19.9%	23
W only	0	0.0%	
H only	429	3.9%	22
S only	3,764	34.1%	22
W and H	0	0.0%	
W and S	0	0.0%	
H and S	1,626	14.7%	21
W, H and S	3,026	27.4%	20
Full Sample	11,037	100%	108

Table 1: Bounding/Censoring of Total Wealth, Housing Wealth and Stock Market Wealth

	No Controls		Full Model	
	Full Sample	Complete Cases	Full Sample	Complete Cases
$\ln W$	.1623	.0820	.0436	.0262
	(.0055)	(.0142)	(.0058)	(.0147)
$\ln H$	.0167	.0610	.0135	.0543
	(.0058)	(.0129)	(.0041)	(.0105)
$\ln S$	.0231	.0056	.0044	.0033
	(.0036)	(.0068)	(.0025)	(.0058)
R^2	.29	.11	.63	.50
Observations	11,037	2,192	11,037	2,192

Table 2: Estimated Wealth Effects on Consumption

and the “No Controls” estimates are from regressing log consumption on a constant and the log wealth values only, for comparison. The differences between the estimates from the complete cases and from the full sample are striking. The overall wealth effect is much smaller for the complete cases. The (additional) housing wealth effect is much larger, whereas the (additional) stock market wealth effect is only marginally smaller in the complete cases, with the full model. That is, we see a completely different picture of the wealth elasticities from the complete cases. If the model (32) is correctly specified, we are seeing substantial bias from the censoring of wealth values.

We give the results of our tests for zero bias in Table 3. Recall that the Partial CR tests are based on comparing the complete cases with the full sample estimates; namely comparing the regressions reported in Table 2. The Full CR tests are based on comparing estimates of the identified parameters from each of the five data segments. Clearly, there is overwhelming rejection of zero bias from censored wealth values, using either partial or full tests and with both specifications.

To summarize, we have found strong evidence that the wealth values at the bound are not “true” values, but are consistent with censoring. Ignoring the bounding of the regressors changes the results substantively. Of course, part of this difference could be due to other misspecification problems such as important omitted variables. If the censoring itself is endogenous, then other strategies for estimation would be required.

	No Controls	Full Model
Full CR Test		
$\hat{F}$	46.36	4.28
d.f.	9, 11024	85, 10929
p-value (F)	1.2E-82	2.2E-35
p-value (χ^2)	2.9E-84	3.7E-36
Partial CR Test		
$\hat{F}$	18.26	4.03
d.f.	4, 11029	23, 10991
p-value (F)	5.8E-15	2.7E-10
p-value (χ^2)	5.2E-18	2.4E-10

Table 3: Tests of Zero CR Bias in Consumption

3.2. Impact of Foreign Denominated Debt in Investment Decisions

The macroeconomic literature has highlighted the importance that balance sheet effects have on investment decisions. In the international context, the presence of debt denominated in foreign currency might exacerbate the impact of devaluations on investment through this channel. In fact, in emerging markets, the recessionary aspect of recent devaluations has being blamed on this mechanism. As we discussed in Section 2.1, on theoretical grounds, the issue is not directly concerned with debt in foreign currency, per se, but rather on the exposure of the firm to exchange rate risk. Clearly, unhedged positions will have an effect on the firm's balance sheet, and ultimately on credit and investment decisions. One question is how unhedged positions should be measured? Assets or liabilities in foreign currency are only a small part of unhedged positions. Exports and imports of raw material are part of the exposure, as are financial instruments and contracts indexed or denominated in foreign currency. In practice, measuring the exposure to exchange rate risk is extremely difficult. For the most part, researchers have used debt denominated in foreign currency to proxy for the exposure.

In this section, we use data from Cowan, Hansen, and Herrera (2006).[17] This includes data on the investment decisions of Chilean firms, as well as information on their balance sheets. The data is a panel of firms, with information on assets, debts, and other contracts in foreign currency. For a detailed description of the data see the original paper.

Our empirical analysis is based on the following model:

$$I_{it} = \alpha_i + \delta_t + \beta_1 D_{it} + \beta_2 E_t D_{it} + \phi_1 A_{it} + \phi_2 E_t A_{it} + \varepsilon_{it} \tag{33}$$

where i, t refer to firm and year, I is the investment ratio (investment over assets), D is the total debt in dollars as a percentage of the total assets, A is total assets in dollars as a percentage of total assets, and E is the real exchange rate. We include firm fixed effects and time fixed effects, where the direct/overall impact of exchange rate variations is captured in the time effects. The impact of foreign exchange exposure (here dollars) is captured in the coefficients β_2, ϕ_2 of the interaction terms. For

[17]We thank Cowan, Hansen, and Herrara for supplying us with the non-proprietary part of their data. They also provided us with their programs and help. We thank them for their support.

Bounded/Censored	Observations	Percentage	Firm Effects	Time Effects	Identified Parameters
Complete Cases	466	35.5%	89	8	101
D only	51	3.9%	28	8	38
A only	319	24.3%	82	8	92
D and A	476	36.3%	93	8	101
Full Sample	1,312	100%	173	8	332

Table 4: Bounding/Censoring of Debt and Assets

comparison, we also estimate (33) with just the debt terms.

Debt in dollars and assets in dollars are bounded below by zero, and as we have argued before, may represent censored versions of the true unhedged positions held by firms.[18] There is a substantial fraction of the observed values at the bounds, as shown in Table 4. For instance, debt in dollars D is 0 for 40.2 % of the observations, and assets in dollars A is zero for 60.6% of the observations. Nonzero values for both of D and A are given in 35.5% of the observations. In sum, there are four nonempty segments of data for the Full CR test.

A further complication is introduced by bounding in a panel data context. Namely, the subset of firms represented in the different segments varies, although each segment has observations from each year. Again, "Identified Parameters" gives the number of parameters identified in each segment, including the firm effects and time effects.

The results from estimating (33) over the full sample and over the complete cases are presented in Table 5. The results display the expected signs, but have different values for the complete cases that appear to be significant. This is true for the "dollar debt only" equation as well as the full equation with dollar debt and dollar assets. Perhaps most interesting is that when the full model is estimated with the complete cases, the effects appear to vanish. Each coefficient displayed is estimated very imprecisely, with the largest t-statistic being 1.2, for the $E \cdot A$ term That is, the significance of the results from estimating with the full sample could be induced by the bias from censored regressors.

The tests of zero bias from censored regressors are presented in Table 6. For the specification with debt only, there are only two data segments (D nonzero and D zero). Therefore, the Full CR test is the same as the Partial CR test, and we have a rejection (as predicted by the difference in the D coefficient above). For the full model (33), the Partial CR test fails to reject, which may reflect how imprecise the estimates with the complete cases are (i.e., the full model estimates cannot be ruled out relative to the complete case estimates). However, the Full CR test, based on estimates from each of the four data segments, does reject the hypothesis of zero bias. The evidence seems clear that zero dollar debt and zero dollar asset values are not consistent with the same model as nonzero values. Moreover, the exposure estimates when the values are nonzero are very imprecise.[19]

[18] We focus on the censoring issue, but there can be various other problems. For instance, one may argue that firms that have debt in dollars also have asset in dollars to balance, or that their exports are a sizeable proportion of their debt. This could make dollar debt a poor proxy for unhedged dollar position, leading to error-in-variables problems, etc., which

	Debt included		Debt and Assets included	
	Full Sample	Complete Cases	Full Sample	Complete Cases
D	.135	.182	.127	.027
	(.060)	(.066)	(.061)	(.063)
$E \cdot D$	-1.416	-1.988	-1.800	-.961
	(.566)	(.653)	(.607)	(.641)
A			.0591	.0166
			(.053)	(.047)
$E \cdot A$			.808	.467
			(.460)	(.402)
R^2	.29	.38	.30	.42
Observations	1,312	789	1,312	466

Table 5: Estimated Debt and Asset Effects on Investment

	Debt included	Debt and Assets included
Full CR Test		
$\hat{F}$	1.88	1.66
d.f.	75, 1054	147, 980
p-value (F)	1.5E-5	6.5E-6
p-value (χ^2)	6.2E-6	7.6E-7
Partial CR Test		
$\hat{F}$	(same as	.92
d.f.	Full)	68, 1059
p-value (F)		.67
p-value (χ^2)		.67

Table 6: Tests of Zero CR Bias in Investment

4. Notes on the Power and Size of the Test

We complete the exposition by presenting a Monte Carlo exercise that illustrates the power and the size of the CR tests. The objective is to create two samples: one in which apparently censored values are true values (our null hypothesis) and one in which the true regressor is mismeasured by censoring. The procedure is the following

1. Assume that x is a random variable with mean zero. We produce N observations.
2. We assume a value for β, and assumed a level of censoring: 5%, 10%, ... , 90%
3. We construct a censored version of x, where censoring is to a lower bound. We set the bound level to achieve the desired censoring level of 5%, 10%, etc..
4. We construct two versions of y. One is based on the true value of x (namely $y = x\beta + \varepsilon$), so that the alternative hypothesis holds (and censoring bias results). The other is based on the censored version of x, or where the null hypothesis holds.
5. We study the CR Test applied with these two types of samples.

Figures 4 and 5 show the results of the simulations when $N = 10000$. Figure 4(a) is the CR Test computed assuming the alternative hypothesis is correct. On the horizontal axis we depict the different degrees of truncation from 5 percent to 95 percent. The different schedules reflect the alterantive values of β. Figure 4(b) is the respective p-value assuming the alternative hypothesis is the true. Several patterns are worth highlighting. First, the test values increase with the degree of censoring. The rejections (measured by the p-values) occur when enough censoring exists in the data. In other words, if there is only 5 to 10 percent censoring it is hard to find a rejection. However, when there is 20 percent censoring the p-values are smaller than 5 percent.

Figures 4(c) and 4(d) show the CR Tests and p-value (from the F distribution) when the null hypothesis is true. This is the case in which the zeros do not represent censored observations, but true zeros. As can be seen, the CR Test statistics are all small (all smaller than 1.2), and the p-values are all larger than 20 percent (not even shown in the figure).

In conclusion, the test has a very good size, in that we do not find a rejection when the true model was correct. However, we found that for small censoring the power of the test is relatively low.

Finally, in Figure 5 we show the estimated coefficients to highlight expansion bias and the differences between the coefficients estimated in the alternative sub-samples. In each figure we plot the difference

cause bias in OLS coefficients.

[19] Our specification differs from the one estimated in Cowan, Hansen, and Herrera (2006). First, they have information on off-balance sheet dollar denominated contracts such as futures and options. We do not have such data because it is proprietary. Second, they control for exports and imports – we did the same and the results are identical. Third, and possibly the most important difference, is that we have firm fixed effects and they do not. Our preliminary work rejected a random effects specification which drove us to include the firm fixed effects in the analysis.

between the estimated coefficient and the true one (which goes from -0.9 to 0.9. Figure 5(a) is the estimate of β in the full sample, in the simulation where the alternative hypothesis is true. Figure 5(b) is the estimate in the complete cases sub-sample. Figure 5(c) depicts the estimates in the full sample when the null hypothesis is correct, and Figure 5(d) are the estimates in the complete case sub-sample. As can be easily seen, the bias occurs only when the estimates are performed in the full sample, and when the alternative hypothesis is true. Otherwise, the estimates are all very close to the true value of β, i.e. the differences are all close to zero.

5. Concluding Remarks

The use of bounded or censored regressors is common in applied work. In most circumstances, censoring of regressors leads to important biases. In this paper, we develop straightforward tests of whether such biases are evident, in terms of being statistically significant. The tests is described simple enough that they can be implemented with standard softwares.

The intuition of the test is standard from specification testing. When there is no bias, we get consistent and efficient estimates of the parameters using the full data set, but inconsistent estimates when there are biases. With exogenous censoring, we get consistent estimates of the parameters by dropping all of the censored observations in all circumstances. So, we can test on the basis of the difference between full sample estimates and estimates from uncensored observations only. When there are many censored regressors, there are many different data segments with some variables censored and others not, and we can test on the basis of coefficient stability across all those segments. The only subtle part involves keeping track of the number of identified parameters in each segment, which determines the appropriate degrees of freedom. We pointed out how the test applies in the heteroskedastic case, and with instruments. The intuition remains the same.

We have not addressed the question of what to do when censored regressor bias is found. Consistent estimates are obtained from the uncensored observations, but they often represent a small subset of the full data. The next step in our research is to resolve various estimation problems when censoring exists, such as how best to include censored observations in the analysis.[20]

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A. Appendix: Formulae for Test with Instrumental Variables

The asymptotic distribution of the test statistic $\tilde{H}^{IV}$ of (31) follows directly from the asymptotic normality of $\sqrt{n}\left(\hat{b}_0^{IV} - \tilde{b}^{IV}\right)$. In accordance with (3) and (27), let the vector of disturbances $E = \{\varepsilon_i\}$ be partitioned as

$$E = \begin{bmatrix} \varepsilon_0 \\ \varepsilon_1 \end{bmatrix}$$

with ε_0 the vector of values with $d_i = 0$ and ε_1 the vector of values with $d_i = 1$. Then we can write

$$\hat{b}_0^{IV} - \tilde{b}^{IV} = A\varepsilon_0 + B\varepsilon_1 \tag{34}$$

where

$$A = D_0\left(X_0'Z_0\left(Z_0'Z_0\right)^{-1}Z_0'\right) - D\left(X'Z\left(Z'Z\right)^{-1}Z_0'\right)$$

$$B = -D\left(X'Z\left(Z'Z\right)^{-1}Z_1'\right)$$

$$D_0 = \left(X_0'Z_0\left(Z_0'Z_0\right)^{-1}Z_0'X_0\right)^{-1}$$

$$D = \left(X'Z\left(Z'Z\right)^{-1}Z'X\right)^{-1}$$

Using standard arguments applied to (34), we have that under the null hypothesis, $\sqrt{n}\left(\hat{b}_0^{IV} - \tilde{b}^{IV}\right)$ has a limiting normal distribution with mean 0 and variance V_b^{IV}, where

$$V_b^{IV} = \sigma^2 \text{plim}\ \left\{n \cdot \left(AA' + BB'\right)\right\}$$

The covariance structure is expressed as follows: let

$$M_{XZ} = \text{plim}\ n^{-1}X'Z, \quad M_{ZZ} = \text{plim}\ n^{-1}Z'Z$$

$$M_{XZ,0} = \text{plim}\ n^{-1}X_0'Z_0, \quad M_{ZZ,0} = \text{plim}\ n^{-1}Z_0'Z_0, \quad M_{ZZ,1} = \text{plim}\ n^{-1}Z_1'Z_1$$

then we can write

$$\begin{aligned} V_b^{IV} \quad = \quad & \sigma^2\Big[\Delta_0\left(M_{XZ,0}M_{ZZ,0}^{-1}M_{XZ,0}'\right)\Delta_0 + \Delta\left(M_{XZ}M_{ZZ}^{-1}M_{ZZ,0}M_{ZZ}^{-1}M_{XZ}'\right)\Delta \\ & -\Delta_0\left(M_{XZ,0}M_{ZZ}^{-1}M_{XZ}'\right)\Delta - \Delta\left(M_{XZ}M_{ZZ}^{-1}M_{XZ,0}'\right)\Delta_0 \\ & +\Delta\left(M_{XZ}M_{ZZ}^{-1}M_{ZZ,1}M_{ZZ}^{-1}M_{XZ}'\right)\Delta\Big] \end{aligned}$$

where

$$\Delta = \left(M_{XZ}M_{ZZ}^{-1}M_{XZ}^{'}\right)^{-1}, \quad \Delta_0 = \left(M_{XZ,0}M_{ZZ,0}^{-1}M_{XZ,0}^{'}\right)^{-1}.$$

Therefore, under the null $H^{IV} = n\left(\hat{b}_0^{IV} - \tilde{b}^{IV}\right)^{'}\left(V_b^{IV}\right)^{-1}\left(\hat{b}_0^{IV} - \tilde{b}^{IV}\right)$ is asymptotically $\chi^2(k)$. The same statement is true for $\tilde{H}^{IV} = \left(\hat{b}_0^{IV} - \tilde{b}^{IV}\right)^{'}\left(\hat{V}_b^{IV}\right)^{-1}\left(\hat{b}_0^{IV} - \tilde{b}^{IV}\right)$ of (31), where the estimated covariance matrix is

$$\hat{V}_b^{IV} = s^2\left(AA^{'} + BB^{'}\right),$$

with s^2 a consistent estimator of σ^2, such as $s^2 = (1/n)\left(Y - X\tilde{b}^{IV}\right)^{'}\left(Y - X\tilde{b}^{IV}\right)$.

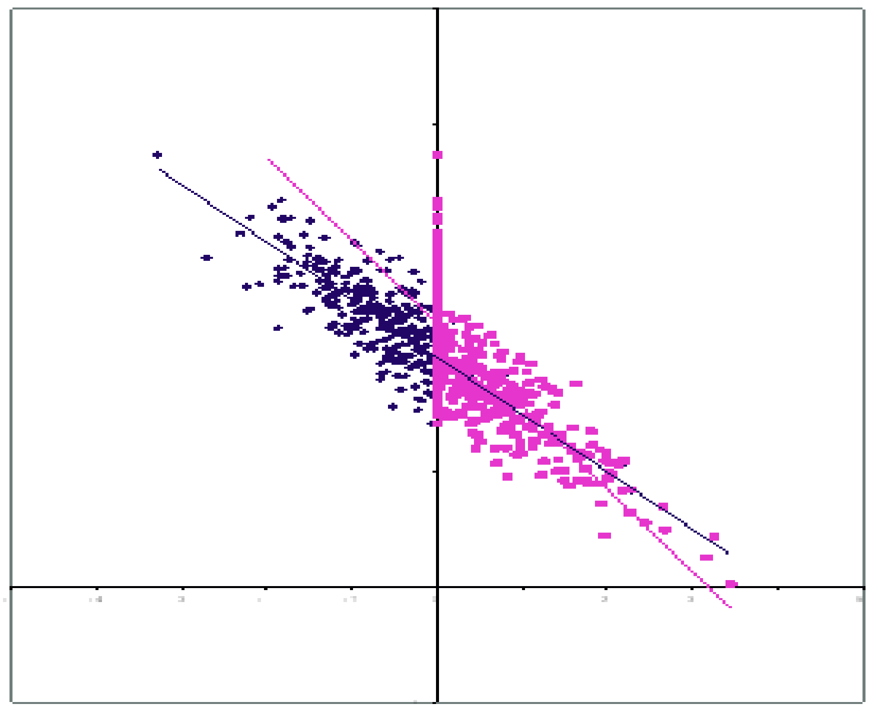


Figure 1: Bias when using censored data versus only the complete cases.

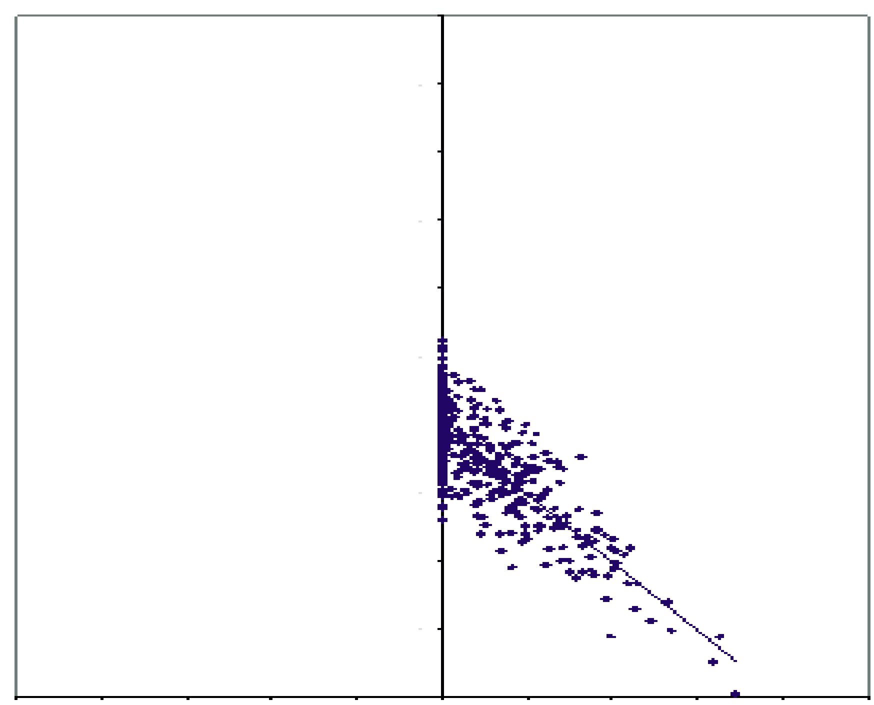


Figure 2: True data is not truncated. The observed zeros of the independent variable are true zeros.

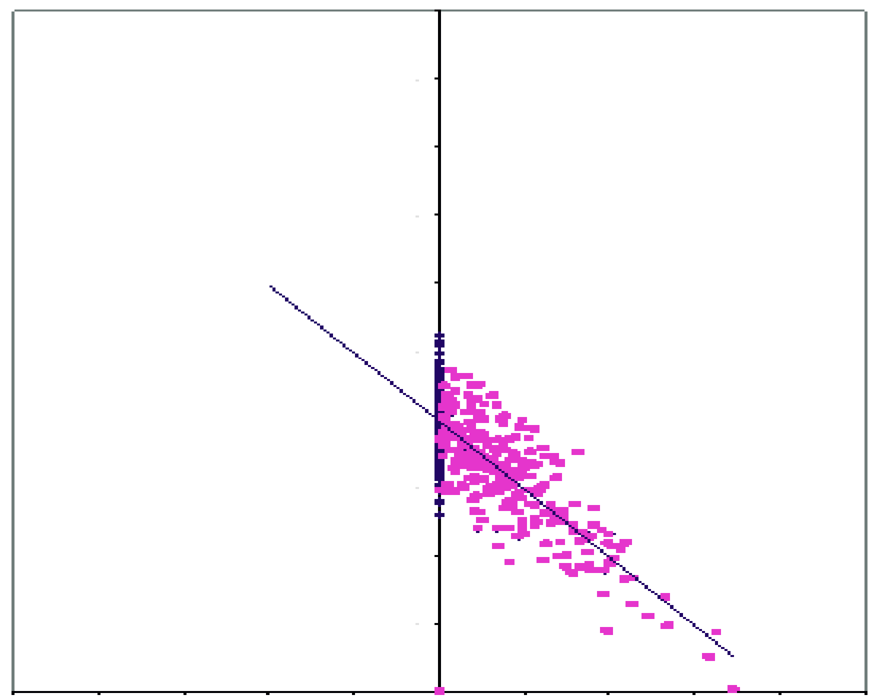
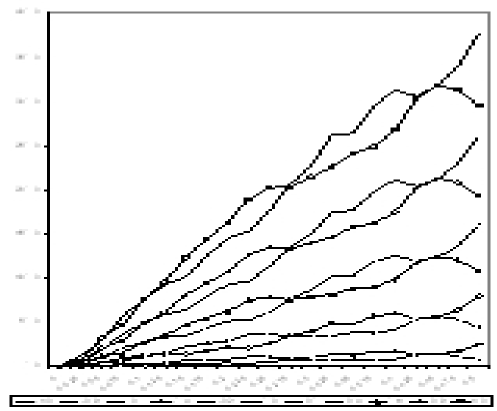
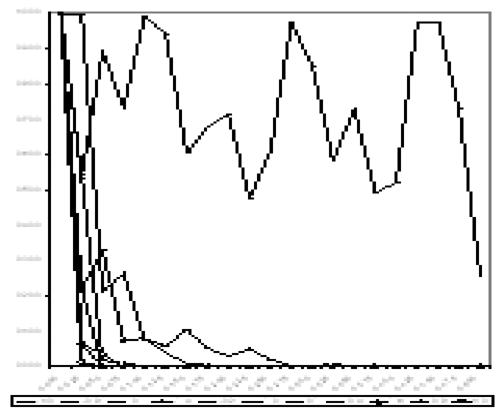


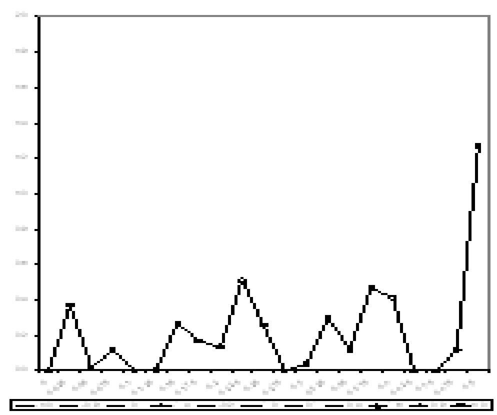
Figure 3: Estimates using all data and only the complete cases.



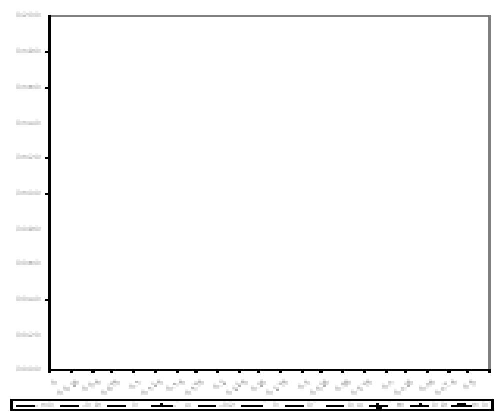
(a) F-test under H1



(b) P-value under H1

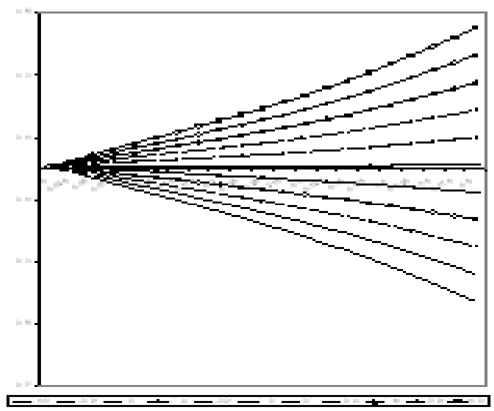


(c) F-test under H0

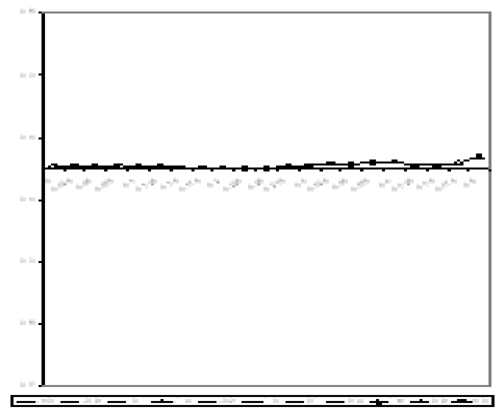


(d) P-value under H0

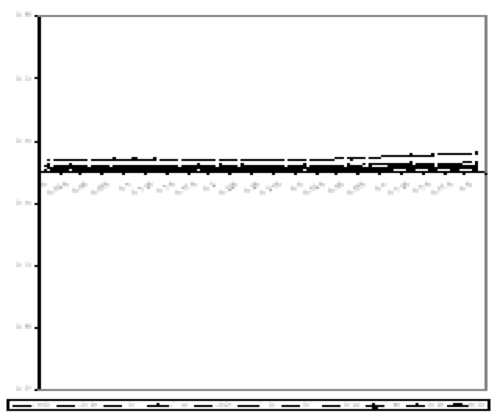
Figure 4: F-test and P-values for different values of β and different degrees of censoring.



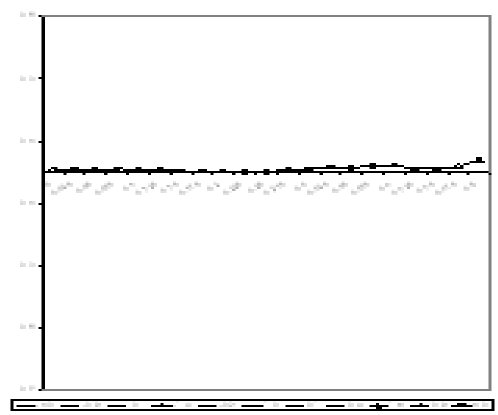
(a) Estimate of β, full sample, under H1



(b) Estimate of β, CC sub-sample, under H1



(c) Estimate of β, full sample, under H0



(d) stimate of β, CC sub-sample, under H0

Figure 5: Estimates of β for the full sample and the complete cases sub-samples.