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Abel Brodeur
Mathias Lé
Marc Sangnier
Yanos Zylberberg

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Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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Abel Brodeur
Paris School of Economics,
CEP, London School of Economics and IZA

Mathias Lé
Paris School of Economics

Marc Sangnier
Aix-Marseille University,
CNRS and EHESS

Yanos Zylberberg
CREI, Universitat Pompeu Fabra

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

Star Wars: The Empirics Strike Back[*]

Journals favor rejection of the null hypothesis. This selection upon tests may distort the behavior of researchers. Using 50,000 tests published between 2005 and 2011 in the *AER*, *JPE*, and *QJE*, we identify a residual in the distribution of tests that cannot be explained by selection. The distribution of p-values exhibits a camel shape with abundant p-values above 0.25, a valley between 0.25 and 0.10 and a bump slightly below 0.05. The missing tests (with p-values between 0.25 and 0.10) can be retrieved just after the 0.05 threshold and represent 10% to 20% of marginally rejected tests. Our interpretation is that researchers might be tempted to *inflate* the value of those almost-rejected tests by choosing a "significant" specification. We propose a method to measure *inflation* and decompose it along articles' and authors' characteristics.

JEL Classification: A11, B41, C13, C44

Keywords: hypothesis testing, distorting incentives, selection bias, research in economics

Corresponding author:

Yanos Zylberberg
CREI, Universitat Pompeu Fabra
Ramon Trias Fargas, 25-27
08005 Barcelona
Spain
E-mail: yzylberberg@crei.cat

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If the stars were mine
I'd give them all to you
I'd pluck them down right from the sky
And leave it only blue.
"If The Stars Were Mine" by Melody Gardot

1 Introduction

The introduction of norms–confidence at 95% or 90%–and the use of eye-catchers–stars–have led the academic community to accept more easily starry stories with marginally significant coefficients than starless ones with marginally insignificant coefficients.[1] As highlighted by Sterling (1959), this effect has modified the selection of papers published in journals and arguably biased publications toward tests rejecting the null hypothesis. This selection is not unreasonable. The choice of a norm was precisely made to strongly discriminate between rejected and accepted hypotheses.

As an unintended consequence, researchers may now anticipate this selection and consider that it is a stumbling block for their ideas to be considered. As such, among a set of acceptable specifications for a test, they may be tempted to keep those with the highest statistics in order to increase their chances of being published. Keeping only such specifications would lead to *inflation* in the statistics of observed tests.

Inflation and selection are different. Selection (rejection or self-censorship) consists in the non-publication of the paper given the specification displayed. Inflation is a publication-oriented choice of the displayed specification among the set of acceptable specifications. The choice of the right specification may depend on its capacity to detect an effect.[2]

In our interpretation, inflation can be distinguished from selection as the former should have different empirical implications than the latter. We assume that selection results in a probability of being published that increases with the value of test statistics presented in a paper. Inflation would not necessarily satisfy this property. Imagine that there are three types of results, green lights are clearly rejected tests, red lights clearly accepted tests and amber lights uncertain tests, i.e. close

[1] Fisher (1925) institutionalized the significance levels. R. A. Fisher supposedly decided to establish the 5% level since he was earning 5% of royalties for his publications. It is however noticeable that, in economics, the academic community has converged toward 10% as being the first hurdle to pass, maybe because of the stringency of the 5% one.

[2] Authors may be tempted to shake their data before submitting a paper, or stop exploring further specifications when finding a "significant" one. Bastardi et al. (2011) and Nosek et al. (2012) explicitly refer to this wishful thinking in data exploration.

to the 5% or 10% statistical significance thresholds but not there yet. We argue that researchers would mainly inflate when confronted with an amber test such as to paint it green, rather than in the initially red and green cases where inflation does not change the status of a test. In other words, we should expect a shortage of amber tests relatively to red and green ones. The shift in the observed distribution of statistics would be inconsistent with the previous assumption on selection. There would be (i) a valley (not enough tests around 0.15 as if they were disliked relatively to 0.30 tests) and (ii) the echoing bump (too many tests slightly under 0.05 as if they were preferred to 0.001 tests).

We find evidence for this pattern. The distribution of tests statistics published in three of the most prestigious economic journals over the period 2005-2011 exhibits a sizable under-representation of marginally insignificant statistics. In a nutshell, once tests are normalized as z-statistics, the distribution has a camel shape with (i) missing z-statistics between 1.2 and 1.65 (p-values between 0.25 and 0.10) and a local minimum around 1.5 (p-value of 0.12), (ii) a bump between 2 and 4 (p-values slightly below 0.05). We argue that this pattern cannot be explained by selection and derive a lower bound for the inflation bias under the assumption that selection should be weakly increasing in the exhibited z-statistics. We find that ten to twenty percent of tests with p-values between 0.05 and 0.0001 are misallocated. Interestingly, the interval between the valley and the bulk of p-values corresponds precisely to the highest marginal returns for the selection function.[3]

To our knowledge, this project is the first to identify a pattern of published tests that cannot be explained by selection and to propose a way to measure it.[4] To achieve this, we conducted a census of tests in the literature. Identifying tests necessitates a good understanding of the argument developed in an article and a strict process avoiding any subjective selection of tests. This collecting process generated $50,078$ tests grouped in $3,389$ tables (or results subsections) and 641 articles published in the American Economic Review, the Journal of Political Economy, and the Quarterly Journal of Economics between 2005 and 2011.

[3] It is theoretically difficult to separate the estimation of inflation from selection: one may interpret selection and inflation as the equilibrium outcome of a game played by editors/referees and authors as in the model of Henry (2009). Editors and referees prefer to publish results that are "significant". Authors are tempted to inflate (with a cost), which pushes editors toward being even more conservative and exacerbates selection and inflation. A strong empirical argument in favor of this game between editors/referees and authors would be an increasing selection even below 0.05, i.e. editors challenge the credibility of rejected tests. Our findings do not seem to support this pattern.

[4] Gadbury and Allison (2012) recently proposed a method which analyzes the distribution of tests very close to the statistical significance thresholds and compares amber-red tests with amber-green tests. Their analysis is developed in the same spirit as ours, but is local and has not been implemented.

In addition to the census of tests, we collect a broad range of information on each paper and author. This allows us to compare the distribution of published tests along various dimensions. For example, we find evidence that inflation is less present in articles where stars are not used as eye-catchers. To make a parallel with central banks, the choice not to use eye-catchers might be considered as a commitment to keep inflation low.[5] Inflation is also smaller in articles with theoretical models, or in articles using data from randomized control trials or laboratory experiments. We also present evidence that papers published by tenured and older researchers are less prone to inflation.

The literature on tests in economics was flourishing in the eighties and already shown the importance of selection and the possible influence of inflation. On inflation, Leamer and Leonard (1983) and Leamer (1985) point out the fact that inferences drawn from coefficients estimated in linear regressions are very sensitive to the underlying econometric model. They suggest to display the range of inferences generated by a set of models. Leamer (1983) rules out the myth inherited from the physical sciences that econometric inferences are independent of priors: it is possible to exhibit both a positive and a negative effect of capital punishment on crime depending on priors on the acceptable specification. Lovell (1983) and Denton (1985) study the implications of individual and collective data mining. In psychological science, the issue has also been recognized as a relatively common problem (see Simmons and Simonsohn 2011 and Bastardi et al. 2011).

On selection, the literature has referred to the file drawer problem: statistics with low values are censored by journals. We consider this as being part of selection among other mechanisms such as self-censoring of insignificant results by authors. A large number of recent publications quantify the extent to which selection distorts published results (see Ashenfelter and Greenstone 2004 or Begg and Mazumdar 1994). Ashenfelter et al. (1999) propose a meta-analysis of the Mincer equation showing a selection bias in favor of significant and positive returns to education. A generalized method to identify reporting bias has been developed by Hedges (1992) and extended by Doucouliagos and Stanley (2011). Card and Krueger (1995) and Doucouliagos et al. (2011) are two other examples of meta-analysis dealing with publication bias. The selection issue has also received a great deal of attention in the medical literature (Berlin et al. 1989, Ioannidis 2005, Ridley et al. 2007), in psychological science (Simmons and Simonsohn 2011, Fanelli 2010a) or in political science (Gerber et al. 2010).

[5] However, such a causal interpretation might be challenged: researchers may give up on stars precisely when their use is less relevant, either because coefficients are very significant and the test of nullity is not a particular concern or because coefficients are not significant.

Section 2 details the methodology to construct the dataset and provides some information on tests' meta-data. Section 3 documents the distribution of tests. Section 4 proposes a method to measure inflation. Finally, we discuss the main results and present the sub-samples' analysis in section 5.

2 Data

In this section, we describe the reporting process of tests published in the American Economic Review, the Journal of Political Economy, and the Quarterly Journal of Economics between 2005 and 2011. We then provide some descriptive statistics.

2.1 Reporting process

The ideal measure of interest of this article is the reported value of formal tests of central hypotheses. In practice, the large majority of those formal tests are two-sided tests for regressions' coefficients and are implicitly discussed in the body of the article (i.e. "coefficients are significant").[6] To simplify the exposition we explain the process as if we only had two-sided tests for regressions' coefficients but the description applies to our treatment of other tests.

Not all coefficients reported in tables should be considered as tests of central hypotheses. Accordingly, we trust the authors and report tests that are discussed in the body of the article except if they are explicitly described as controls. The choice of this process helps to get rid of cognitive bias at the expense of parsimony. With this mechanical way of collecting tests we also report statistical tests that the authors may expect to fail, but we do not report explicit placebo tests. When the status of a test was unclear when reading the paper, we prefer to add a non-relevant test than censor a relevant one.

As we are only interested in tests of central hypotheses of articles, we also exclude descriptive statistics or groups comparisons.[7] A specific rule concerns two-stage procedures. We do not report first-stages, except if the first-stage is described by authors as a major contribution of the article. We do include tests in extensions or robustness tests and report numbers exactly as they are presented in articles, i.e. we never round them up or down.

We report some additional information on each test, i.e. the issue of the journal,

[6] 85% of collected test are presented using a regressions' coefficient and their associated standard errors.

[7] A notable exception to this rule was made for experimental papers where results are sometimes presented as mean comparisons across groups.

the starting page of the article and the position of the test in the article, its type (one-sided, two-sided, correlation test, etc.) and the status of the test in the article (main, non-main). We prefer to be conservative and only attribute the status of "non-main" statistics if evidence are clearly presented as "complementary", "additional" or "robustness checks". Finally, the number of authors, JEL codes when available, the presence of a theoretical model, the type of data (laboratory experiment, randomized control trials or other), the use of eye-catchers (stars or other formatting tricks such as bold printing), the number of research assistants and researchers the authors wish to thank, the rate of tenure among authors and data and code availability on the website of the journal are also recorded. We do not report the sample size and the number of variables (regressors) as this information is not always provided by authors. Exhaustive reporting rules we used are presented in the online appendix.

We also collected information from curricula vitae of all the authors who published in the three journals over the period of interest. We gathered information about academic affiliation at the time of the publication, the position at the main institution (assistant professor, associate professor, etc.), whether the author is or was an editor (or a member of an editorial board) of an economic journal, and the year and the institution where the PhD was earned.

2.2 Descriptive statistics

The reporting process described above provides $50,078$ tests. Journals do not contribute equally: most of the tests come from the American Economic Review, closely followed by the Quarterly Journal of Economics. The Journal of Political Economy provides a little less than a fifth of the sample. Out of the $50,078$ tests extracted from the three journals, around $30,000$ are rejected at the 10% significance level, $27,000$ at 5%, and $21,000$ at 1%.

Table 1 gives the decomposition of tests along several dimensions. The average number of tests per article equals 78. It is surprisingly high but it is mainly driven by some articles with a very large number of tests reported. The median article reports 58 tests and 5 tables. These figures are reasonable as tests are usually diluted in many different empirical specifications. Imagine a paper with two variables of interest (e.g. democracy and institutions), six different specifications per table and five tables. We would report 60 coefficients, a bit more than our median article.

More than half of the articles use eye-catchers defined as the presence of stars or bold printing in a table, excluding the explicit display of p-values. These starry tests represent more than sixty percent of the total number of tests(the average number of tests is higher in articles using eye-catchers). With the conservative way of reporting main results, more than seventy percent of tables from which tests are extracted are considered as main. More than a third of the articles in our sample explicitly rely on a theoretical framework but when they do so, the number of tests provided is not particularly smaller than when they do not. Only a fifth of articles are single-authored.[8]

Tests using data from laboratory experiments or randomized control trials constitute a small part of the overall sample. To be more precise, the AER publishes relatively more experimental articles while the QJE seems to favor randomized controlled trials. The overall contribution of both types is equivalent (with twice as many laboratory experiments than randomized experiments but more tests in the latter than in the former).

3 The distribution of tests

In this section, we describe the raw distribution of tests and propose methods to alleviate the over-representation of round values and the potential overweight attributed to articles with many tests. We then derive the distribution of tests and comment on it.

The collecting process groups three types of measures : p-values, tests statistics when directly reported by authors, and coefficients and standard errors for the vast majority of tests. In order to get a homogeneous sample, we transform p-values into the equivalent z-statistics (a p-value of 0.05 becomes 1.96). For tests reported using coefficients and standard errors, we simply construct the ratio of the two.[9] Recall that the distribution of a t-statistic depends on the degrees of freedom, while that of a z-statistic is standard normal. As we are unable to reconstruct the degrees of freedom for all tests, we will treat these ratios as if they were following an asymptotically standard normal distribution under the null hypothesis. Consequently, when the sample size is small, the level of rejection we use is not adequate. For instance, some tests for which we associate a z-statistic of $z = 1.97$ might not be rejected at the 5% significance threshold.

The transformation into z-statistic allows us to observe more easily the fat tail of tests (with small p-values). Figure 1(a) presents the raw distribution. Remark that a very large number of p-values end up below the 0.05 significance threshold (more

[8] See Card and DellaVigna (2012, 2013) for recent studies about top journals in economics.

[9] These transformations allow us to obtain direct or reconstructed statistics for all but three types of tests collected: (i) tests reported as a zero p-value, (ii) tests reported as a p-value lower than a threshold (e.g. $p < 0.001$), and (iii) tests reported with a zero standard error. These three cases represent 727 tests, i.e. 1.45% of the total sample.

than 50% of tests are rejected at this significance level).

Two potential issues may be raised with the way authors *report* the value of their tests and the way we *reconstruct* the underlying statistics. First, a small proportion of coefficients and standard errors are reported with a pretty poor precision (0.020 and 0.010 for example). Reconstructed z-statistics are thus over-abundant for fractions of integers ($\frac{1}{1}, \frac{2}{1}, \frac{3}{1}, \frac{1}{3}, \frac{1}{2}, \ldots$). Second, some authors report a lot of versions of the same test. In some articles, more than 100 values are reported against 4 or 5 in others. Which weights are we suppose to give to the former and the latter in the final distribution? This issue might be of particular concern as authors might choose the number of tests they report depending on how close or far they are from the thresholds.[10]

To alleviate the first issue, we randomly redraw a value in the interval of potential z-statistics given the reported values and their precision. In the example given above, the interval would be $[\frac{0.0195}{0.0105}, \frac{0.0205}{0.0095}] \approx [1.86, 2.16]$. We draw a z-statistic from a uniform distribution over the interval and replace the previous one. This reallocation should not have any impact on the analysis other than smoothing potential discontinuities in histograms.[11]

To alleviate the second issue, we construct two different sets of weights, accounting for the number of tests per article and per table in each article. For the first set of weights, we associate to each test the inverse of the number of tests presented in the same article such that each article contributes the same to the distribution. For the second set of weights, we associate the inverse of the number of tests presented in the same table (or result sub-section) multiplied by the inverse of the number of tables in the article such that each article contributes the same to the distribution and tables of a same article have equal weights.

Figure 1(b) presents the de-rounded distribution.[12] The shape is striking. The distribution presents a camel pattern with a local minimum around $z = 1.5$ (p-value of 0.12) and a local maximum around 2 (p-value under 0.05). The presence of a local maximum around 2 is not very surprising, the existence of a valley before more so. Intuitively, selection could explain an increasing pattern for the distribution of z-statistics at the beginning of the interval $[0, \infty)$. On the other hand, it is likely

[10] For example, one might conjecture that authors report more tests when the significance of those is shaky. Conversely, one may also choose to display a small number of satisfying tests as others tests would fail.

[11] For statistics close to significance levels, we could have taken advantage of the information embedded in the presence of a star. However, this approach could only have been implemented for a very reduced number of observations and only in cases where stars are used.

[12] In what follows, we use the word "de-rounded" to refer to statistics to which we applied the method described above.

that there is a natural decreasing pattern of the distribution over the whole interval. Both effects put together could explain the presence of a unique local maximum, a local minimum before, less so. Our empirical strategy will consist in formalizing this argument: only a shift of statistics can generate such a pattern and the inflation bias seems to explain this shift.[13]

Figures 1(c) and (d) present the weighted distributions of de-rounded statistics. The camel shape is more pronounced than for the unweighted distributions. A simple explanation is that weighted distributions underweight articles and tables for which a lot of tests are reported. For these articles and tables, our conservative way to report tests might have included tests of non-central hypotheses.

The pattern shown in figures 1(b), (c) and (d) is very consistent. It is not driven by a particular journal or a particular year. The last three sub-figures of figure 9 show the distributions for each of the three journals. The shape is also similar for each specific year.

In figures 5 to 9, we plot the distribution of tests over sub-samples along some characteristics of tests or articles. We analyze further the variations of inflation across sub-samples in section 5, but we can already notice that the shape of the distribution varies along several dimensions. For example, the camel shape is less pronounced in articles without eye-catchers and articles with a theoretical contribution (see figure 5). Similarly, inflation seems lower in papers written by senior researchers whether seniority is captured by years since PhD, tenure or editorial responsibilities (see figure 6). Inflation seems larger in articles that thank research assistants (see figure 7). In contrast, it does not vary along data and codes availability on journals' website (see figure 8). Finally, inflation appears to be less intense in articles using randomized control trials or laboratory experiments data (see figure 9). All in all, the pattern that we document is not invariant along authors' and articles' characteristics.

[13] In the online appendix, we also test for discontinuities. We find evidence that the total distribution of tests presents a small discontinuity around the 0.10 significance threshold, but not much around the 0.05 or the 0.01 thresholds. This modest effect might be explained by the dilution of hypothesis tested in journal articles. In the absence of a single test, empirical economists provide many converging arguments under the form of different specifications for a single effect. Besides, an empirical article is often dedicated to the identification of more than one mechanism. As such, the real statistic related to an article is a distribution or a set of arguments and this dilution smoothes potential discrepancies around thresholds. The online appendix also presents an analysis using the Benford's law to look for manipulation in reported coefficients and standard errors.

4 A method to measure inflation

In this section, we present a method to obtain an estimate of inflation. The basic question this method attempts to answer is as follows: how much of the misallocation of test statistics can be attributed to inflation? The idea is that the observed distribution of published z-statistics may be thought as generated by (i) an input distribution, (ii) a selection over results, and (iii) a noise, which will partly capture inflation. We first describe a very simple model of selection in academic publishing. Then, we discuss the identification strategy and the different *counterfactual* distributions to which we can compare the *observed* one. In our framework, under the restricting assumption that selection favors high over low statistics, the ratio of the observed density over the input density would be increasing in the exhibited statistic. The empirical strategy will consist in capturing any violation of this prediction and relating it to the inflation bias. We also discuss stories that may challenge this interpretation. Finally, we apply this method to the distribution of tests presented in the previous section.

4.1 The selection process

We consider a very simple theoretical framework of selection into journals. We abstract from authors and directly consider the universe of working papers.[14] Each economic working paper has a unique hypothesis which is tested with a unique specification. Denote z the absolute value of the statistic associated to this test and φ the density of its distribution over the universe of working papers, the *input*.

A unique journal gives a value $f(z,\varepsilon)$ to each working paper where ε is a noise entering into the selection process.[15] Working papers are accepted for publication as long as they pass a certain threshold F, i.e. $f(z,\varepsilon) \geq F$. Suppose without loss of generality that f is strictly increasing in ε, such that a high ε corresponds to articles with higher likelihood to be published, for a same z. Denote G_z the distribution of ε conditional on the value of z.

The density of tests in journals (the output) can be written as:

$$\psi(z) = \frac{\int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} dG_z(\varepsilon) d\varepsilon\right] \varphi(z)}{\int_0^\infty \int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} dG_z(\varepsilon) d\varepsilon\right] \varphi(z) dz}.$$

[14] Note that the selection of potential economic issues into a working paper is not modeled here. You can think alternatively that this is the universe of potential ideas and selection would then include the process from the "choice" of idea to publication.

[15] We label here ε as a noise but it also captures inclinations of journals for certain articles, the importance of the question, the originality of the methodology, or the quality of the paper.

The observed density of tests for a given z depends on the quantity of articles with ε sufficiently high to pass the threshold and on the input. In the black box which generates the output from the input, two effects intervene. First, as the value of z changes, the minimum noise ε required to pass the threshold changes: it is easier to get in, this is the selection effect. Second, the distribution G_z of this ε might change conditionally on z. The quality of articles may differ along z: this will be in the residual. We argue that this residual captures–among other potential mechanisms–local shifts in the distribution. An inflation bias corresponds to such a shift. In this framework, this would translate into productions of low ε just below the threshold against very high ε above.

4.2 Identification strategy

Our empirical strategy consists in estimating how well selection might explain the observed pattern and we interpret the residual as capturing inflation. This strategy is conservative as it may attribute to selection some patterns due to inflation.

Let us assume that we know the distribution φ. The ratio of the output density to the input density can be written as:

$$\psi(z)/\varphi(z) = \frac{\int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} dG_z(\varepsilon) d\varepsilon\right]}{\int_0^\infty \int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} dG_z(\varepsilon) d\varepsilon\right] \varphi(z) dz}.$$

In this framework, once normalized by the input, the output is a function of the selection function f and the conditional distribution of noise G_z. We will isolate selection $z \mapsto \mathbb{1}_{f(z,\varepsilon)}$ from noise $G_z(\varepsilon)$ thanks to the following assumption.

Assumption 1 (Journals like stars)**.** *The function f is (weakly) increasing in z.*

For a same noisy component ε, journals prefer higher z. Everything else equal, a 10% test is not strictly preferred to a 9% one.

This assumption that journals, referees and authors prefer tests rejecting the null may not be viable for high z-statistics. Such results could indicate an empirical misspecification to referees. This effect, if present, should only appear for very large statistics. Another concern is that journals may also appreciate clear acceptance of the null hypothesis, in which case the selection function would be initially decreasing. Journals and authors may privilege p-values very close to 1 and very close to 0, which would fit the camel pattern with two bumps.[16] We discuss the potential mechanisms challenging this assumption at the end of this section.

[16] There is no way to formally reject this interpretation. However, we think that this effect is marginal as the number of articles for which the central hypothesis is accepted is very small in our sample.

The identification strategy relies on two results. First, if we shut down any other channel than selection (the noise is independent of z), we should see an increasing pattern in the selection process, i.e. the proportion of articles selected $\psi(z)/\varphi(z)$ should be (weakly) increasing in z. We cannot explain stagnation or slowdowns in this ratio with selection or self-censoring alone. Second and this is the purpose of the lemma below, the reciprocal is also true: any increasing pattern for the ratio output/input can be explained by selection alone, i.e. with a distribution of noise invariant in z. Given any selection process f verifying assumption 1, any increasing function of z (in a reasonable interval) for the ratio of densities can be generated by f and a certain distribution of noise invariant in z. Intuitively, there is no way to identify an inflation effect with an increasing ratio of densities, as an invariant distribution of noise can always be considered to fit the pattern.

Lemma 1 (Duality). *Given a selection function f, any increasing function $g : [0, T_{lim}] \mapsto [0,1]$ can be represented by a cumulative distribution of quality $\varepsilon \sim \tilde{G}$, where $\tilde{G}$ is invariant in z:*

$$\forall t, \quad g(z) = \int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} d\tilde{G}(\varepsilon) d\varepsilon \right]$$

$\tilde{G}$ is uniquely defined on the subsample $\{\varepsilon, \exists z \in [0,\infty), f(z,\varepsilon) = F\}$, i.e. on the values of noise for which some articles may be rejected (with insignificant tests) and some others accepted (with significant tests).

Proof. In the appendix. □

Following this lemma, the empirical strategy will consist in the estimation of the best-fitting increasing function $\tilde{f}$ for the ratio $\psi(z)/\varphi(z)$. We will find the weakly increasing $\tilde{f}$ that minimizes the weighted distance with the ratio $\psi(z)/\varphi(z)$:

$$\sum_i \left[\psi(z_i)/\varphi(z_i) - \tilde{f}(z_i) \right]^2 \varphi(z_i),$$

where i is a test's identifier.

In order to estimate our effects, we have focused on the ratio $\psi(z)/\varphi(z)$. The following corollary transforms the estimated ratio in a cumulative distribution of z-statistics and relates the residual of the previous estimation to the number of statistics unexplained by selection.

Corollary 1 (Residual). *Following the previous lemma, there exists a cumulative distribution $\tilde{G}$ which represents $\tilde{f}$ uniquely defined on $\{\varepsilon, \exists z \in [0, T_{lim}], f(z,\varepsilon) = F\}$,* *such that:*

$$\forall t, \quad \tilde{f}(z) = \frac{\int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} d\tilde{G}(\varepsilon) d\varepsilon\right]}{\int_0^\infty \int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} dG_z(\varepsilon) d\varepsilon\right] \varphi(z) dz}.$$

The residual of the previous estimation can be written as the difference between $\tilde{G}$ and the true G_z:

$$u(z) = \frac{\tilde{G}(h(z)) - G_z(h(z))}{\int_0^\infty \int_0^\infty \left[\mathbb{1}_{f(z,\varepsilon)\geq F} dG_z(\varepsilon) d\varepsilon\right] \varphi(z) dz},$$

where h is defined as $f(z,\varepsilon) \geq F \Leftrightarrow \varepsilon \geq h(z)$. Define $\tilde{\psi}(z) = (1 - \tilde{G}(h(z)))\varphi(z)$ the density of z-statistics associated to $\tilde{G}$, then the cumulated residual is simply

$$\int_0^z u(\tau)\varphi(\tau) d\tau = \int_0^z \psi(\tau) d\tau - \int_0^z \tilde{\psi}(\tau) d\tau.$$

Proof. In the appendix. □

This corollary allows us to map the cumulated residual of the estimation with a quantity that can be interpreted. Indeed, given z, $\int_0^z \psi(\tau)d\tau - \int_0^z \tilde{\psi}(\tau)d\tau$ is the number of z-statistics between $[0, z]$ that cannot be explained by a selection function verifying assumption 1.

4.3 Input

A difficulty arises in practice. The previous strategy can be implemented for any given input distribution. But what do we want to consider as the exogenous input and what do we want to include in the selection process? In the process that occurs before publication, there are several choices that may change the distribution of tests: the choice of the research question, the dataset, the decision to submit and the acceptance of referees. We think that all these processes are very likely to satisfy assumption 1 (at least for z-statistics not extremely high) and that the input can be taken as the distribution before all these choices. All the choices (research question, dataset, decision to create a working paper, submission and acceptance) will thus be included in the endogenous process.

The idea here is to consider a large range of distributions for the input. The classes of distribution should ideally include unimodal (with the mode being 0) distribution as the distributions of some of our subsamples are unimodal in 0, and ratio distributions as the vast majority of our tests are ratio tests. They should also capture as much as possible of the fat tail of the observed distribution (distributions should allow for the large number of rejected tests and very high z-statistics). Let us consider three candidate classes.

Class 1 (Gaussian). *The Gaussian/Student distribution class arises as the distribution class under the null hypothesis of t-tests. Under the hypothesis that tests are t-tests for independent random processes following normal distributions centered in* 0*, the underlying distribution is a standard normal distribution (if all tests are done with infinite degrees of freedom), or a mix of Student distributions (in the case with finite degrees of freedom).*

This class of distributions naturally arises under the assumption that the underlying hypotheses are always true. For instance, tests of correlations between variables that are randomly chosen from a pool of uncorrelated processes would follow such distributions. From the descriptive statistics, we know that selection should be quite drastic when we consider a normal distribution for the exogenous input. The output displays more than 50% of rejected tests against 5% for the normal distribution. A normal distribution would rule out the existence of statistics around 10. In order to account for the fat tail observed in the data, we extend the class of exogenous inputs to Cauchy distributions. Remark that a ratio of two normal distributions follows a Cauchy law. In that respect, the class of Cauchy distributions satisfies all the ad hoc criteria that we wish to impose on the input.

Class 2 (Cauchy). *The Cauchy distributions are fat-tail ratio distributions which extend the Gaussian/Student distributions: (i) the standard Cauchy distribution coincides with the Student distribution with* 1 *degree of freedom, (ii) this distribution class is, in addition, a strictly stable distribution.*

Cauchy distributions account for the fact that researchers identify mechanisms among a set of correlated processes, for which the null hypothesis might be false. As such, Cauchy distribution allows us to extend the input to fat-tail distributions.

The last approach consists in an empirical counterfactual distribution of statistics obtained by performing random tests on large and classic datasets.

Class 3 (Empirical). *We randomly draw* 4 *variables from the World Development Indicators (WDI) and run* $2,000,000$ *regressions between these variables and stock the z-statistic behind the first variable.*[17] *Other datasets/sample can be considered and the shapes are very close.*

[17] To be consistent with the literature, we just ran two million regressions (see Sala-i Martin 1997 and Hendry and Krolzig 2004).

How do these different classes of distributions compare to the observed distribution of published tests?

Figures 2(a) and (b) show how poorly the normal distribution fits the observed one. The assumption that input comes from uncorrelated processes can only be reconciled with the observed output through a drastic selection (which would generate the observed fat tail from a Gaussian tail). The fit is slightly better for the standard Cauchy distribution, e.g. the Student distribution of degree 1. The proportion of rejected tests is then much higher with 44% of rejected tests at the 0.05 significance level and 35% at 0.01. Cauchy distributions centered in 0 and the empirical counterfactuals of statistics from the World Development Indicators have fairly similar shapes. Figures 2(c) and (d) show that the Cauchy distributions as well as the WDI empirical input may help to capture the fat tail of the observed distribution. Figures 2(e) and (f) focus on the tail: Cauchy distributions with parameters between 0.5 and 2 as well as the empirical placebo fit very well the tail of the observed distribution. More than the levels of the densities, it is their evolution which gives support to the use of these distributions as input: if we suppose that there is no selection nor inflation once passed a certain threshold ($p<0.000001$ for these levels), we should indeed observe a constant ratio output/input.

In what follows, we will first consider as exogenous inputs (i) the WDI empirical input which will be the higher bound in terms of fat-tail (it is close to a Cauchy of parameter 1.5); (ii) the Student distribution; (iii) and a rather thin-tail distribution, i.e. the Cauchy distribution of parameter 0.5. These distributions cover a large spectrum of shapes and results are not sensitive to changes in the choice of inputs. As such, we will finally restrict the analysis to the empirical WDI input for the sake of brevity.

4.4 Discussion

The quantity that we isolate is a cumulated residual (the difference between the observed and the predicted cumulative function of z-statistics) that cannot be explained by selection. In our interpretation, it will capture a *local* shift of z-statistics that reflects the inflation bias. In addition, this quantity is a lower bound of inflation as any *globally* increasing pattern (in z) in the inflation mechanism would be captured as part of the selection effect.

Several observations may challenge our interpretation. First, the noise ε actually includes the quality of a paper and quality may be decreasing in z. The amount of efforts put in a paper might end up being higher with very low p-values or p-values around 0.15. Authors might for instance erroneously estimate selection by journals and produce low effort in certain ranges of z-statistics. Second, the selection function may not be increasing as a well-estimated zero might be interesting and there are no formal tests to exclude this interpretation. We do not present strong evidence against this mechanism. However, two observations make us confident that this preference for well-estimated zero does not drive the whole camel shape. The first argument is based on anecdotal evidence: very few papers of the sample present a well-estimated zero as their central result. Second, this preference for well-estimated zero should not depend on whether eye-catchers are used or whether a theoretical model is attached to the empirical analysis and we find disparities along those characteristics. Similarly, this preference should not depend on authors' characteristics but inflation seems to vary along these features.

In addition, imagine that authors could predict exactly where tests will end up and decide to invest in the working paper accordingly. This *ex ante* selection is captured by the selection term as long as it displays an increasing pattern, i.e. projects with expected higher z-statistics are always more likely to be undertaken. We may think of a very simple setting where it is unlikely to be the case: when designing experiments (or randomized control trials), researchers compute power tests such as to derive the minimum number of participants for which an effect can be statistically captured. The reason is that experiments are expensive and costs need to be minimized under the condition that a test may settle whether the hypothesis can or cannot be rejected. We should expect a thinner tail for those experimental settings and this is exactly what we observe. For this reason, we will not apply our methodology to these samples. In the other cases, the limited capacity of authors to predict where the z-statistics may end up as well as the modest incentives to limit oneself to small samples make it more unlikely.

5 Results

In this section, we first apply our estimation strategy to the full sample and propose non-parametric and parametric analyses. Then, we divide tests into sub-samples and we provide the results separately for each sub-samples.

5.1 Non-parametric application

We group observed z-statistics by bandwidth of 0.01 and limit our study to the interval $[0, 10]$. Accordingly, the analysis is made on $1,000$ bins. As the empirical input appears in the denominator of the ratio $\psi(z)/\varphi(z)$, we smooth it with an

Epanechnikov kernel function and a bandwidth of 0.1 in order to dilute noise (for high z, the probability to see an empty bin is not negligible).

Figures 3(a) and (b) give both the best increasing fit for the ratio of observed density to the empirical WDI input and the associated partial sum of residuals, i.e. the lower bound for the inflation bias.[18] Results are computed with the Pool-Adjacent-Violators Algorithm.

Two interpretations emerge from this estimation. First, the best increasing fit displays high marginal returns to the value of statistics $\partial\tilde{f}(z)/\partial z$ only for $z \in [1.5, 2]$, and a plateau thereafter. Selection is intense precisely where it is supposed to be discriminatory, i.e. before the thresholds. Second, the misallocation of z-statistics captured by the cumulated residuals starts to increase slightly before $z = 2$ up to 4 (the bulk between p-values of 0.05 and 0.0001 cannot be explained by an increasing selection process alone). At the maximum, the misallocation reaches 0.025, which means that 2.5% of the total number of t-statistics are misallocated between 0 and 4. As there is no residual between 0 and 2, we compare this 2.5% to the total proportion of z-statistics between 2 and 4, i.e. 30% of the total population. The conditional probability of being misallocated for a z-statistic between 2 and 4 is thus around 8%. As shown by figures 3(c), (d), (e), (f), results do not change when the input distribution is approximated by a Student distribution of degree 1 and a Cauchy distribution of parameter 0.5. The results are very similar both in terms of shape and magnitude.

A concern in this estimation strategy is that the misallocation could reflect different levels of quality between articles with z-statistics between 2 and 4 compared to the rest. We cannot rule out this possibility. However, two observations gives support to our interpretation: the start of the misallocation is right after (i) the first significance thresholds, and (ii) the zone where the marginal returns of the selection function are the highest.[19]

As already suggested by the shapes of weighted distributions (figures 1(c) and (d)), the results are much stronger when the distribution of observed z-statistics is corrected such that each article or each table contributes the same to the overall distribution. Sub-figures 10(a)-(d) presented in the online appendix plot the best increasing non-parametric fits against the empirical WDI input and associated cumulated residuals when distributions are weighted by article or table. The shape

[18] Note that there are less and less z-statistics per bins of width 0.01. On the right-hand part of the figure, we can see lines that look like raindrops on a windshield. Those lines are bins for which there is the same number of observed z-statistics. As this observed number of z-statistics is divided by a decreasing and continuous function, this gives these increasing patterns.

[19] This result is not surprising as it comes from the mere observation that the observed ratio of densities reaches a maximum between 2 and 4.

of misallocation is similar but the magnitude is approximately twice as large as in the case without weights: the conditional probability of being misallocated for a z-statistic between 2 and 4 is now between 15% and 20%. In a way, the weights may compensate for our very conservative reporting process.

Even though the results are globally inconsistent with the presence of only selection, the distribution of misallocated z-statistics is a bit surprising (and not completely consistent with inflation): the surplus observed between 2 and 4 is here compensated by a deficit after 4. Inflation would predict such a deficit before 2 (between 1.2 and 1.7, which corresponds to the valley between the two bumps). This result comes from the conservative hypothesis that the pattern observed in the ratio of densities should be attributed to the selection function as long as it is increasing. Accordingly, the stagnation of the ratio observed before 1.7 is captured by the selection function. Nonetheless, as the missing tests still fall in the bulk between 2 and 4, they allow us to identify a violation of the presence of selection alone: the bump is too big to be reconciled with the tail of the distribution. In the next sub-section, we get rid of this inconsistency by imposing more restricting assumptions on the selection process.

5.2 Parametric application

A concern about the previous analysis is that it attributes the surplus of misallocated tests between 2 and 4 to missing tests after this bulk. The mere observation of the distribution of tests does not give the same impression. Apart from the bulk between 2 and 4, the other anomaly is the valley around $z = 1.5$. This valley is considered as a stagnation of the selection function in the previous non-parametric case. We consider here a less conservative test by estimating the selection function under the assumption that it should belong to a set of parametric functions.

Assume now that the selection process can be approximated by an exponential polynomial function, i.e. consider a selection function of the following form:

$$f(z) = c + \exp(a_0 + a_1 z + a_2 z^2).$$

The pattern of this function allows us to account for the concave pattern of the observed ratio of densities.[20]

Figure 4 presents the best parametric fits and the partial sums of residuals. As in the non-parametric case, the figure presents results using the empirical WDI input, the Student input, and the Cauchy(0.05) input. Contrary to the non-parametric

[20] The analysis can be made with simple polynomial functions but it slightly worsens the fit.

case, the misallocation of t-statistics starts after $z = 1$ (p-values around 0.30) and is decreasing up to $z = 1.65$ (p-values equals to 0.10 and first significance threshold). These missing statistics are then completely retrieved between 1.65 and $3 - 4$, and no misallocation is left for the tail of the distribution. Remark that the magnitude of misallocation is very similar to the non-parametric case. Sub-figures 11(a)-(d) presented in the online appendix plot the best increasing parametric fits against the empirical WDI input and associated cumulated residuals when distributions are weighted by article or table.

5.3 Subsample analysis

Information we collected about articles and authors allow us to split the full sample of tests into sub-samples along various dimensions and to compare our measure of inflation across sub-samples. It seems reasonable to expect inflation to vary along characteristics of the paper, e.g. the importance of the empirical contribution, or characteristics of the authors, e.g. the expected returns from a publication in a prestigious journal.[21]

In this sub-section, we split the full sample of published z-statistics along various dimensions and present associated distributions. We perform a different estimation of the best-fitting selection function on each subsample using the method presented above. For space consideration, we restrict ourselves to the analysis of unweighted distributions. Figures of corresponding cumulated residuals from parametric and non-parametric estimations using the empirical WDI input are presented in the online appendix and summarized in table 2.

In sub-samples presented in figure 5, we split the full sample depending on the presentation of the results and the content of the paper. Sub-figures (a) and (b) distinguish between tests presented using eye-catchers or not. The analysis on the eye-catchers sample shows that misallocated z-statistics between 0 and 4 account for more than 3% of the total number of tests against 1% for the no eye-catchers sample. The conditional probability of being misallocated for a z-statistic between 2 and 4 is around 12% in the eye-catchers sample against 4% in the no eye-catchers one. Not using stars may act as a commitment for researchers to not be influenced by the distance of their tests from the 10% or 5% significance thresholds. Sub-figures (c) and (d) split the sample depending on whether the test is presented as a main

[21] However, this analysis cannot be considered as causal. From the blank page to the published research article, researchers choose the topic, collect data, decide on co-authorship, where to submit the paper, etc. All these choices are made either simultaneously or sequentially. None of them can be considered as exogenous since they are related to the expected quality of the outcome and to its expected likelihood to be accepted for publication.

test or not (tests or results explicitly presented as “complementary”, “additional” or “robustness checks”). The camel shape is more pronounced for results not presented as a main result. The emphasis put on the empirical analysis may also depend on the presence of a theoretical contribution. In articles having a theoretical content, the main contribution of the paper is divided between theory and empirics. This intuition may explain the results shown in sub-figures (e) and (f): there seems to be almost no inflation in articles with a theoretical model.

One might consider that articles and ideas from researchers with higher academic rankings are more likely to be valued by editors and referees. Accordingly, inflation may vary with authors’ status : well-established researchers facing less intense selection should have less incentives to inflate. A first proxy that we use to capture authors’ status is experience. Sub-figures (a) and (b) of figure 6 present the distributions of tests in articles having an average PhD-age of authors below and above the median PhD-age of the sample. We find that inflation is more pronounced among relatively younger authors. A second indicator reflecting authors’ status is whether they are involved in the academic editorial process. Sub-figures (c) and (d) split the sample in two groups: the former is made of articles published by authors who were not editors or members of editorial boards before publication, while the latter is made of articles published by at least one editor or member of an editorial board. Inflation appears to be slightly more intense among the first group. Another proxy of authors’ status which is strongly related to incentives to publish in top journals is whether authors are tenured or not. We compute the rate of tenure among authors of each paper and split the sample along this dimension in sub-figures (e) and (f).[22] The first distribution is the one of tests from articles published by at least one tenured author three years before publication. The second distribution of tests comes from articles published by authors who were all non-tenured three years before publication. We find that the presence of at least one tenured researcher among authors is associated with a strong decline in inflation. All in all, this finding seems in line with the idea that inflation varies along expected returns to publication in prestigious journals.

Sub-figures (a) and (b) of figure 7 split the sample of published tests between single-authored and co-authored papers: inflation seems to be larger in single-authored papers. We also collected the number of individuals the authors thank

[22] Getting information about effective tenure status of authors may be difficult as positions’ denomination varies across countries and institutions. Here, we only consider full professors as tenured researchers. Besides, the length of the publication process makes it hard to know the precise status of authors at the time of submission. Here, we arbitrarily consider positions of authors three years before publication.

in the published version of the paper. Sub-figures (c) and (d) split the sample between articles that use research assistants and those that do not. Sub-figures (e) and (f) split the sample between articles with a number of thanks (excluding research assistants) below or above the median. Inflation tends to be a bit smaller when no research assistants are thanked and in articles with a relatively low number of thanks.

In figure 8 we investigate whether data and programs disclosure for replication alters inflation. Whether data and codes are available on the website of the journal for replication purposes has attracted a great deal of attention lately (see Dewald et al. (1986) and McCullough et al. 2008). For instance, the AER implemented a mandatory data and code archive few years ago. On the other hand, the JPE archive access is available solely to JPE subscribers. We verify for each article whether data and codes are available on the website of the journal. The analysis of the different sub-samples does not show conclusive evidence that data or programs availability mitigates inflation.

To conclude this sub-sample analysis, we investigate the distribution of tests depending on the source of data. There is an increasing use of randomized control trials in economics and many researchers advocate that it is a very useful way to accumulate knowledge without relying on questionable theory or statistical methods. In figure 9, sub-figure (a) presents the distribution of tests relying on data obtained from randomized control trials. Sub-figure (b) plots the distribution of tests that rely on data from laboratory experiments, and sub-figure (c) all other type of data. The randomized control trials data distribution exhibits a very smooth pattern: there is neither a valley between 0.25 and 0.10, nor a significant bump around 0.05. There is small evidence of inflation for the sub-sample of laboratory experiments. However, z-statistics seem to disappear after the 0.05 threshold in both cases. As argued before, randomized control trials and laboratory experiments are designed such as to minimize the costs while being able to detect an effect. Very large z-statistics are thus less likely to appear which violates our hypothesis that selection is increasing. Hence, we cannot evaluate the inflation bias for these two sub-samples with our methodology.

Finally, we do not find clear evidence that inflation differs across journals as illustrated by sub-figures (d), (e), and (f) of figure 9.

Overall, we find that the intensity of inflation varies along different dimensions of papers' and authors' characteristics. Interestingly, these variations seem consistent with the returns of displaying a "significant" empirical analysis.

6 Conclusion

He who is fixed to a star does not change his mind. (Da Vinci)

In this paper, we have identified a misallocation in the distribution of the test statistics in some of the most respected academic journals in economics. Our analysis suggests that the pattern of this misallocation is consistent with what we dubbed an inflation bias : researchers might be tempted to inflate the value of those almost-rejected tests by choosing a "significant" specification. We have also quantified this inflation bias : among the tests that are marginally significant, 10% to 20% are misreported. These figures are likely to be lower bounds of the true misallocation as we use very conservative collecting and estimating processes. Results presented in this paper may have potentially different implications for the academic community than the already known publication bias. Even though it is unclear whether these biases should be larger or smaller in other journals and disciplines,[23] it raises questions about the importance given to values of tests *per se* and the consequences for a discipline to ignore negative results.

A limit of the present work is that it does not say much about the mechanisms behind inflation. Nor does it say much about the role of expectations of authors/referees/editors in the magnitude of selection and inflation. Understanding the effects of norms requires not only the identification of the biases, but also an understanding of how the academic community adapts its behavior to those norms (Mahoney 1977). For instance, Fanelli (2009) discusses explicit professional misconduct, but it would be important to identify the milder departures from a getting-it-right approach.

We identified some papers' and authors' characteristics that seem to be related to the inflation bias. For instance, the use of eye-catchers is very significantly correlated with inflation. Some factors such as being in a tenure-track job are also correlated with inflation. The inflation bias seems also to be related to the type of empirical analysis (e.g. randomized control trials) and the existence of a theoretical contribution. Finally, data and code availability do not seem to be associated with substantially less inflation.

Suggestions have already been made in order to reduce selection and inflation biases (see Weiss and Wagner (2011) for a review). First, some journals (the Journal of Negative Results in BioMedecine or the Journal of Errology) have been launched

[23] Auspurg and Hinz (2011), Gerber et al. (2010) and Masicampo and Lalande (2012) collect distributions of tests in journals of sociology, political science and psychology but inflation cannot be untangled from selection. See Fanelli (2010b) for a related discussion about the hierarchy of sciences.

with the ambition of giving a place where authors may publish non-significant findings. Second, attempts to reduce data mining have been proposed in medicine or psychological science. There is a pressure for researchers to submit their methodology/empirical specifications before running the experiment (especially because the experiment cannot be reproduced). Some research grants ask researchers to submit their strategy/specifications (sample size of the treatment group for instance) before starting a study. It seems however that researchers pass through this hurdle by (i) investigating an issue, (ii) applying ex-post for a grant for this project, (iii) funding the next project with the funds given for the previous one. Nosek et al. (2012) propose a comprehensive study of what has been considered and how successful it was in tilting the balance towards "getting it right" rather than "getting it published".

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Table 1: Descriptive statistics.

		Number of . . .	
Sample	Articles	Tables	Tests
Full	641	3,389	50,078
AER	327	1,561	21,934
	[51]	[46]	[44]
JPE	110	625	9,311
	[17]	[18]	[19]
QJE	204	1,203	18,833
	[32]	[35]	[38]
Using eye-catchers	350	2,043	32,269
	[55]	[60]	[64]
Main		2,487	35,288
		[73]	[70]
With model	229	979	15,727
	[36]	[29]	[31]
Single-authored	134	695	10,586
	[21]	[21]	[21]
At least one editor	400	2,145	31,649
	[62]	[63]	[63]
At least one tenured author	312	1,659	25,159
	[49]	[49]	[50]
With research assistants	361	2,009	30,578
	[56]	[59]	[61]
Data available	327	1,603	22,143
	[51]	[47]	[44]
Codes available	331	1,662	23,585
	[52]	[49]	[47]
Laboratory experiments data	86	344	3,503
	[13]	[10]	[7]
Randomized control trials data	37	249	4,032
	[6]	[7]	[8]
Other data	522	2,798	42,543
	[81]	[83]	[85]

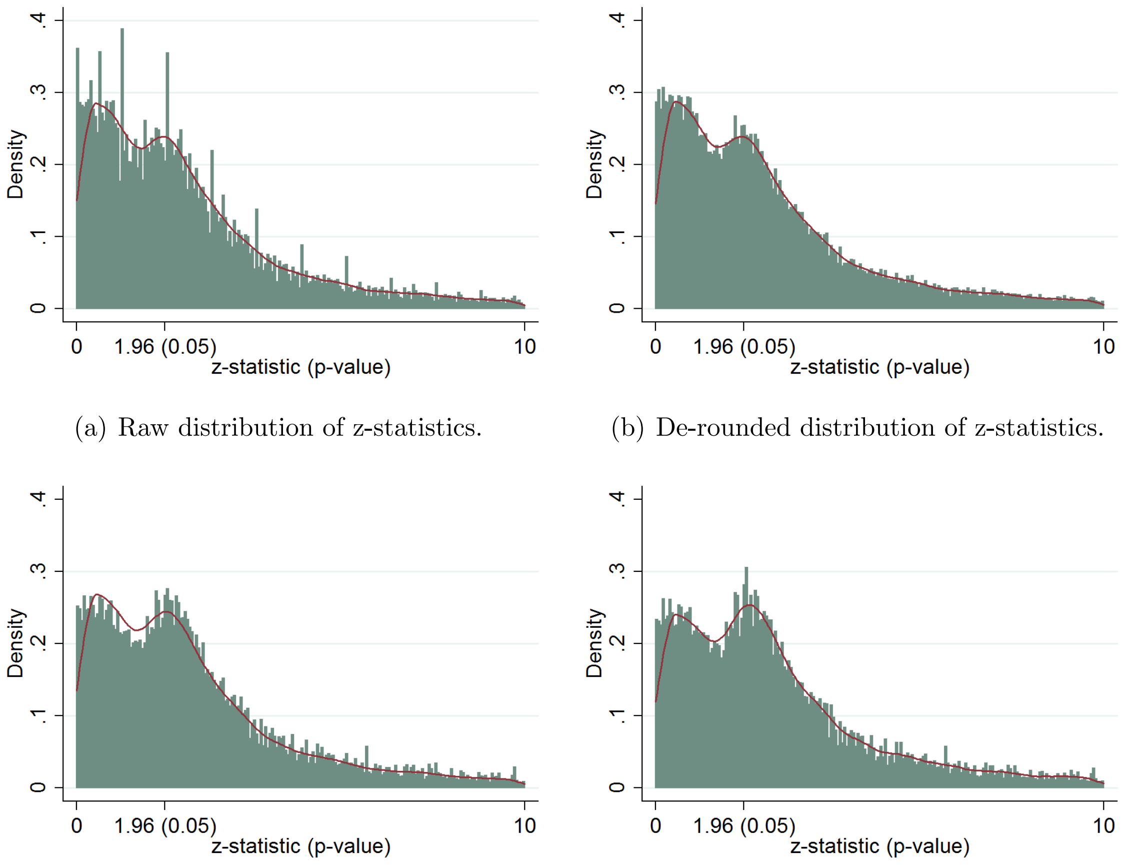
Sources: AER, JPE, and QJE (2005-2011). This table reports the number of tests, tables, and articles for each category. "*Tables*" are tables or results' groups presented in the text. Proportions relatively to the total population are indicated between brackets. "*Using eyes-catchers*" corresponds to articles or tables using stars or bold printing to highlight statistical significance. "*Main*" corresponds to results non explicitly presented as robustness checks, additional or complementary by the authors. "At least one editor" corresponds to articles with at least one member of an editorial board prior to the publication year among the authors. "*At least one tenured author*" correspond to articles with at least one full professor three years before the publication year among the authors. "*Data available*" corresponds to articles for which data can be directly downloaded from the journal's website. "*Codes available*" correspond to articles for which codes can be directly downloaded from the journal's website. The sum of articles or tables by type of data slightly exceeds the total number of articles or tables as results using different data sets may be presented in the same article or table.

Table 2: Summary of parametric and non-parametric estimations using the empirical WDI input for various samples.

Sample	Non-parametric estimation		Parametric estimation	
	z-statistic at the maximum of cumulated residuals	Maximum cumulated residuals	z-statistic at the maximum of cumulated residuals	Maximum cumulated residuals
Full	3.86	0.026	3.85	0.028
Full, weighted by article	3.82	0.030	3.81	0.035
Full, weighted by table	3.97	0.031	3.96	0.036
Eye-catchers	3.85	0.035	3.84	0.033
No eye-catchers	3.40	0.014	3.39	0.021
Main results	3.96	0.022	3.95	0.026
Non-main results	3.85	0.041	3.85	0.036
With model	3.84	0.008	3.83	0.017
Without model	3.86	0.038	3.85	0.033
Low average PhD-age	3.85	0.047	3.94	0.041
High average PhD-age	3.86	0.010	3.85	0.016
No editor	3.96	0.031	3.95	0.029
At least one editor	3.86	0.023	3.85	0.027
No tenured	3.95	0.039	4.03	0.038
At least one tenured author	3.45	0.015	3.64	0.019
Single-authored	3.84	0.040	3.83	0.038
Co-authored	3.86	0.023	3.85	0.025
With research assistants	3.86	0.032	3.98	0.033
Without research assistants	3.82	0.017	3.81	0.020
Low number of thanks	3.86	0.018	3.85	0.022
High number of thanks	3.85	0.034	3.84	0.032
Data available	3.86	0.029	3.85	0.028
Data not available	3.99	0.025	3.98	0.028
Codes available	3.27	0.022	3.84	0.023
Codes not available	3.99	0.031	3.98	0.033
Data and codes available	3.27	0.028	3.85	0.027
Data or codes not available	3.99	0.026	3.98	0.029

Sources: AER, JPE, and QJE (2005-2011) and authors' calculation. See the text for the definitions of weights. "*Low average PhD-age*" corresponds to articles written by authors whose average age since PhD is below the median of the articles' population. "*Low number of thanks*" corresponds to articles where the number of individuals thanked in the title's footnote is below the median of the articles' population. See notes of table 1 for the definitions of other categories.

Figure 1: Distributions of z-statistics.



(a) Raw distribution of z-statistics.

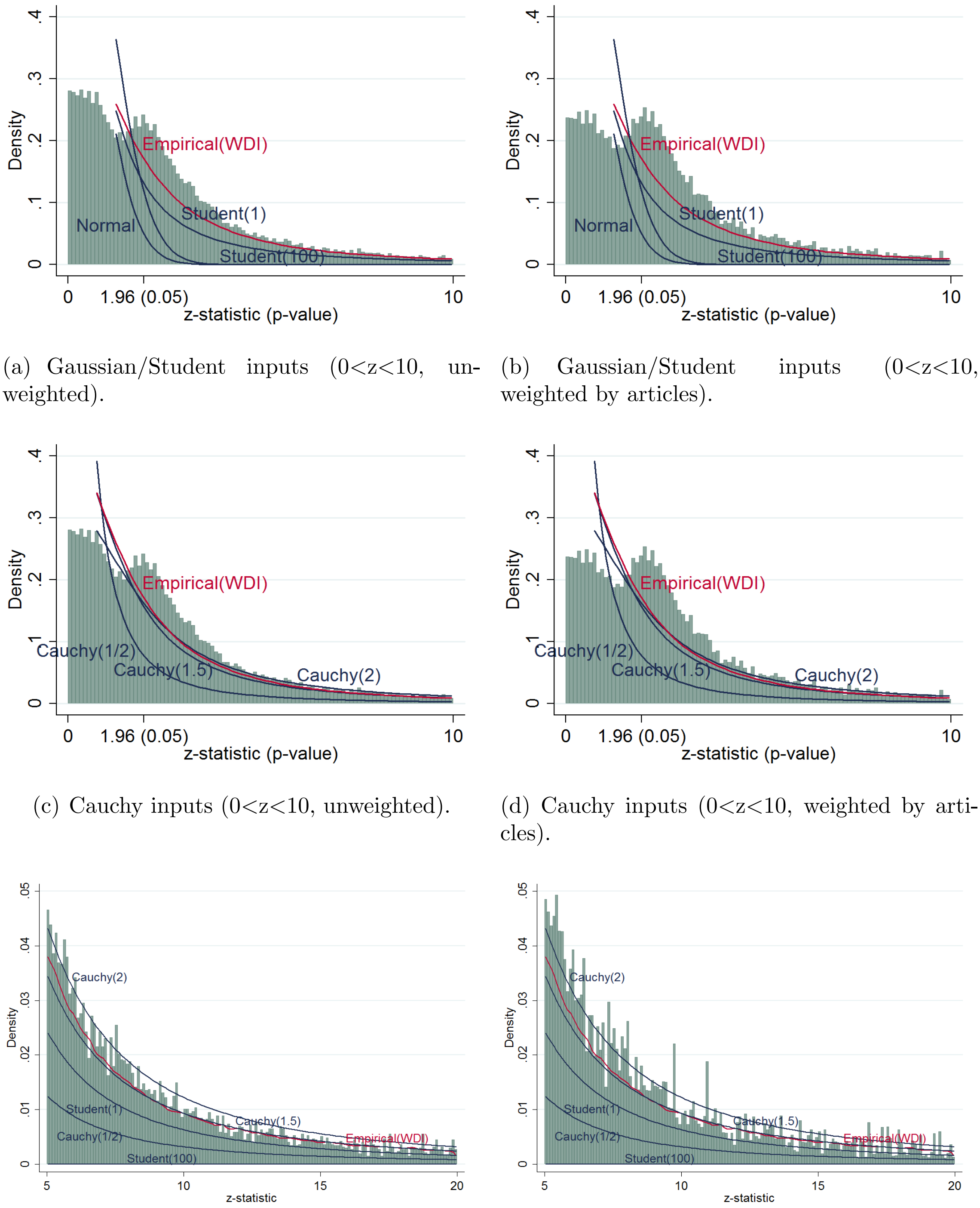
(b) De-rounded distribution of z-statistics.

(c) De-rounded distribution of z-statistics, weighted by articles.

(d) De-rounded distribution of z-statistics, weighted by articles and tables.

Sources: AER, JPE, and QJE (2005-2011). See the text for the de-rounding method. The distribution presented in sub-figure (c) uses the inverse of the number of tests presented in the same article to weight observations. The distribution presented in sub-figure (d) uses the inverse of the number of tests presented in the same table (or result) multiplied by the inverse of the number of tables in the article to weight observations. Lines correspond to kernel density estimates.

Figure 2: Unweighted and weighted distributions of the universe of z-statistics and candidate exogenous inputs.



(a) Gaussian/Student inputs (0<z<10, unweighted).

(b) Gaussian/Student inputs (0<z<10, weighted by articles).

(c) Cauchy inputs (0<z<10, unweighted).

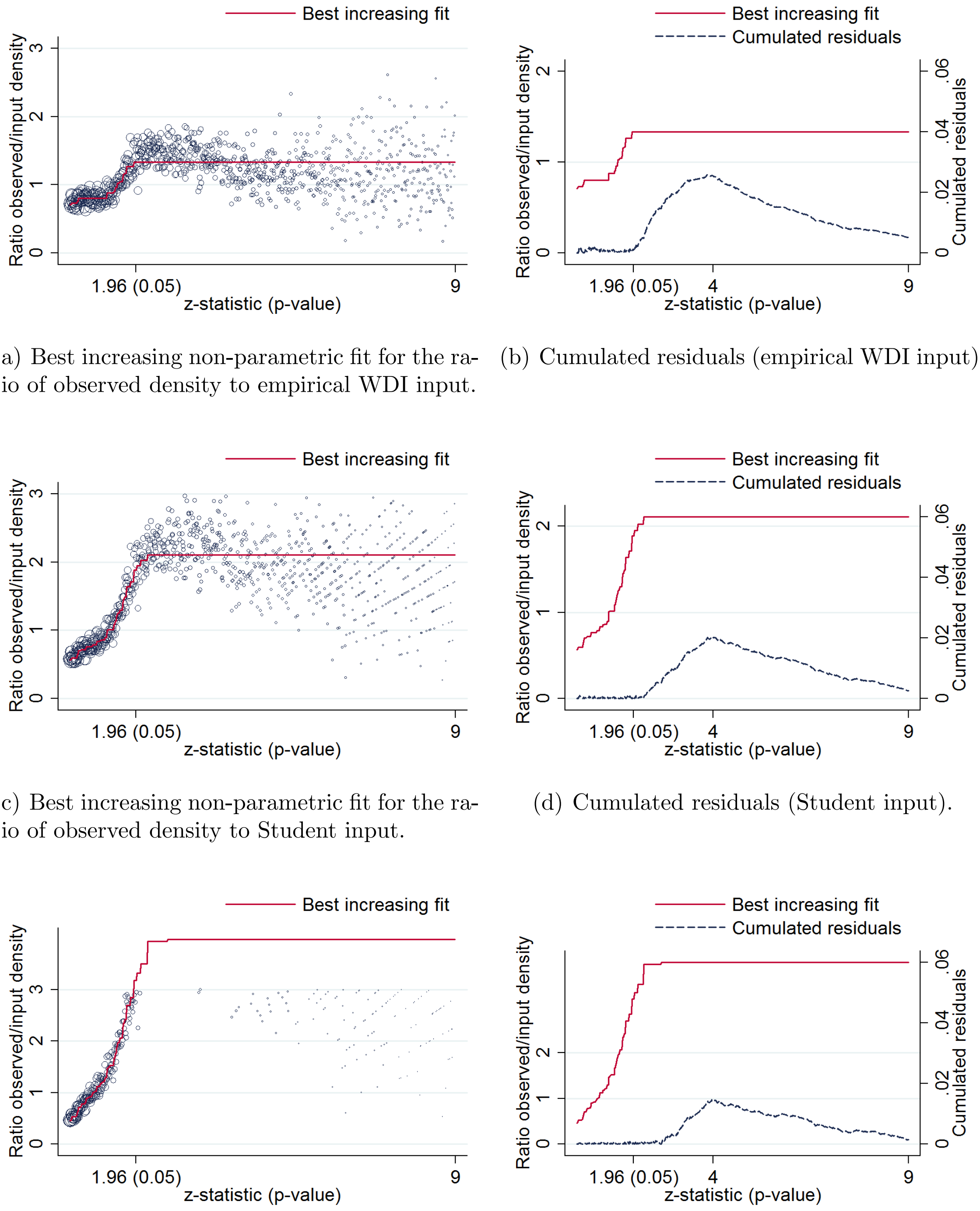
(d) Cauchy inputs (0<z<10, weighted by articles).

(e) All inputs (5<z<20, unweighted).

(f) All inputs (5<z<20, weighted by articles).

Sources: AER, JPE, and QJE (2005-2011). Distributions are plotted using de-rounded statistics. Weighted distributions use the inverse of the number of tests presented in the same article to weight observations.

Figure 3: Non-parametric estimation of selection and inflation.



(a) Best increasing non-parametric fit for the ratio of observed density to empirical WDI input.

(b) Cumulated residuals (empirical WDI input).

(c) Best increasing non-parametric fit for the ratio of observed density to Student input.

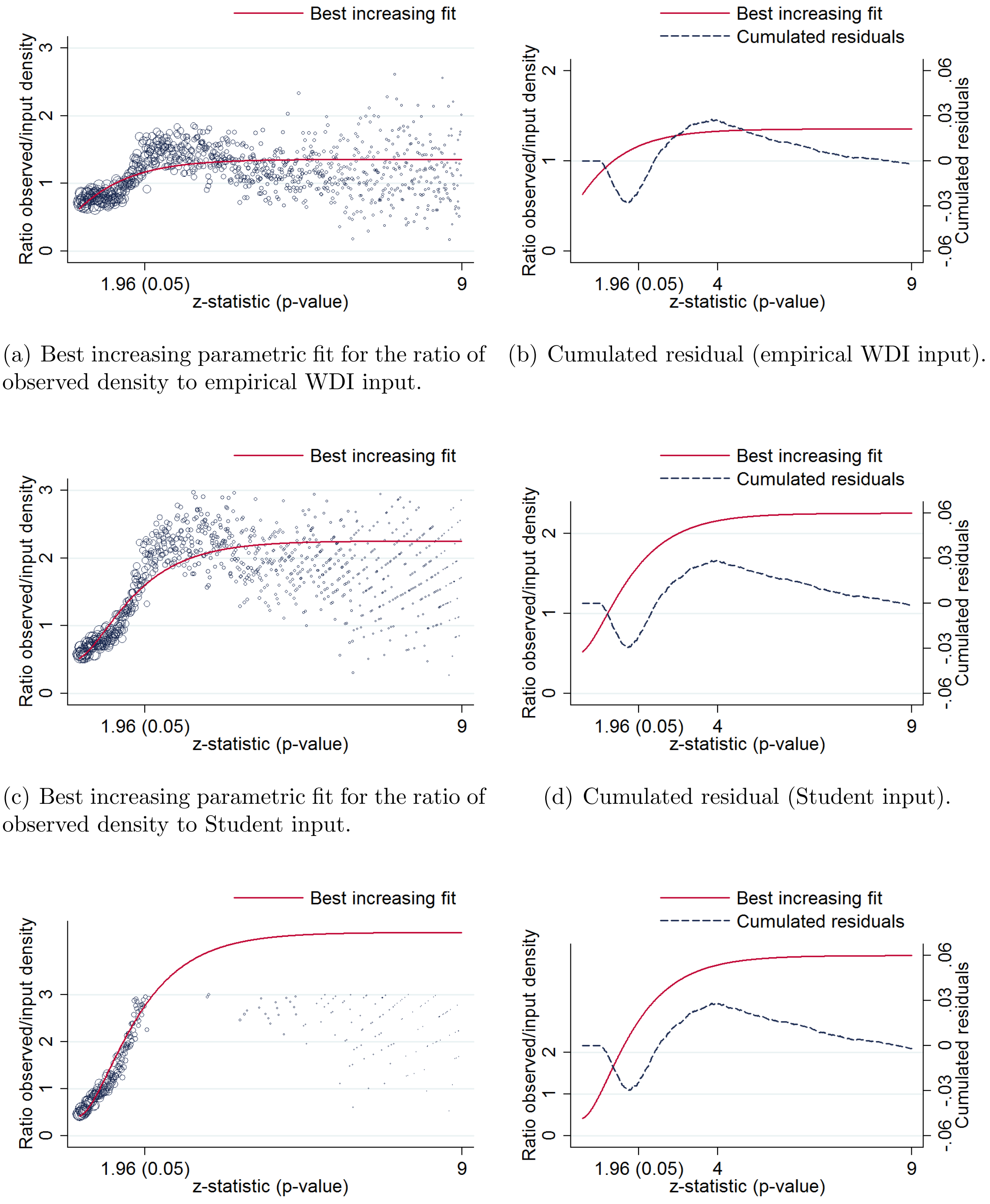
(d) Cumulated residuals (Student input).

(e) Best increasing non-parametric fit for the ratio of observed density to Cauchy(0.5) input.

(f) Cumulated residuals (Cauchy(0.5) input).

Sources: AER, JPE, and QJE (2005-2011).

Figure 4: Parametric estimation of selection and inflation.



(a) Best increasing parametric fit for the ratio of observed density to empirical WDI input.

(b) Cumulated residual (empirical WDI input).

(c) Best increasing parametric fit for the ratio of observed density to Student input.

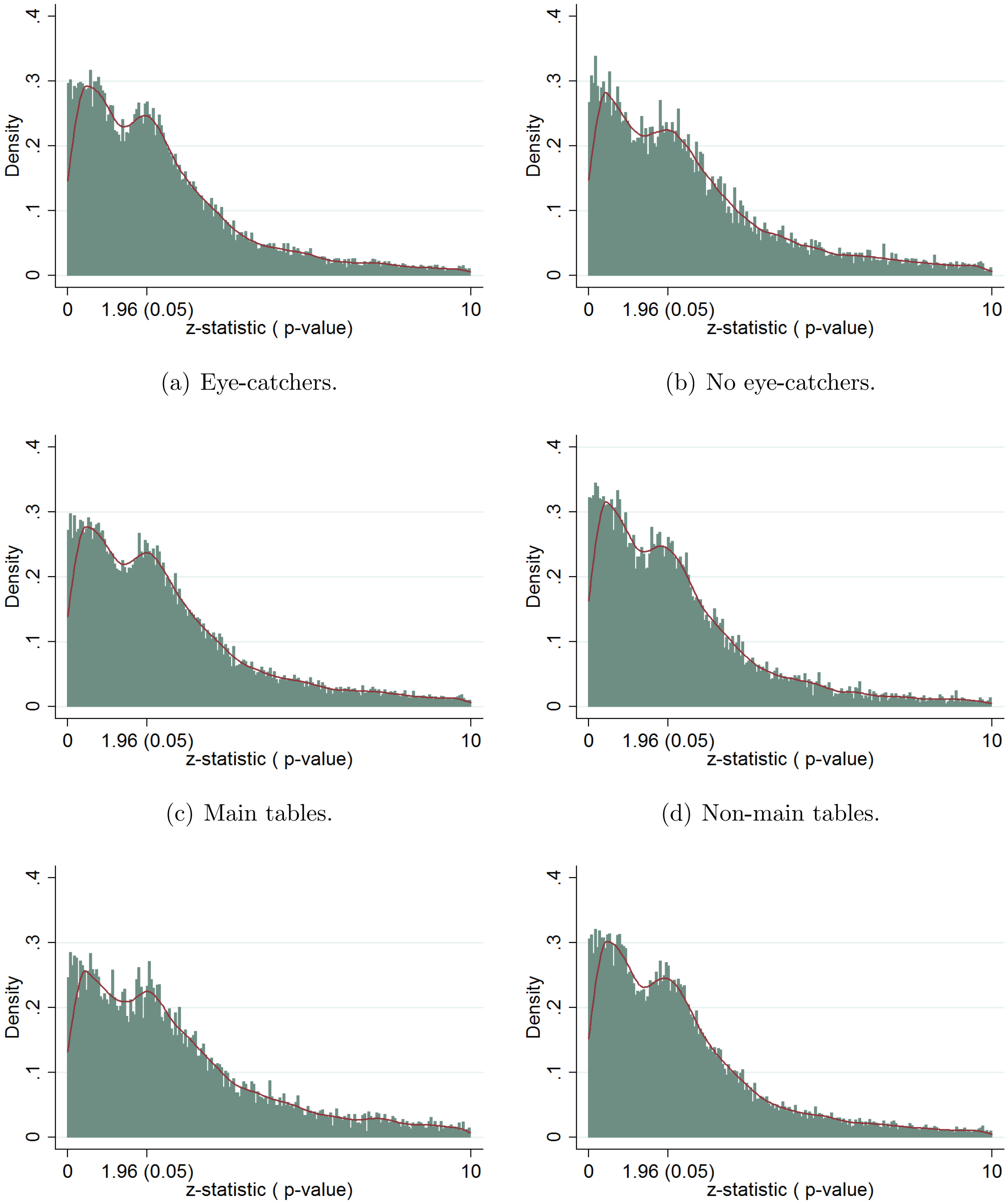
(d) Cumulated residual (Student input).

(e) Best increasing parametric fit for the ratio of observed density to Cauchy(0.5) input.

(f) Cumulated residual (Cauchy(0.5) input).

Sources: AER, JPE, and QJE (2005-2011).

Figure 5: Distributions of z-statistics for different sub-samples: eye-catchers, theoretical contribution and status of result.



(a) Eye-catchers.

(b) No eye-catchers.

(c) Main tables.

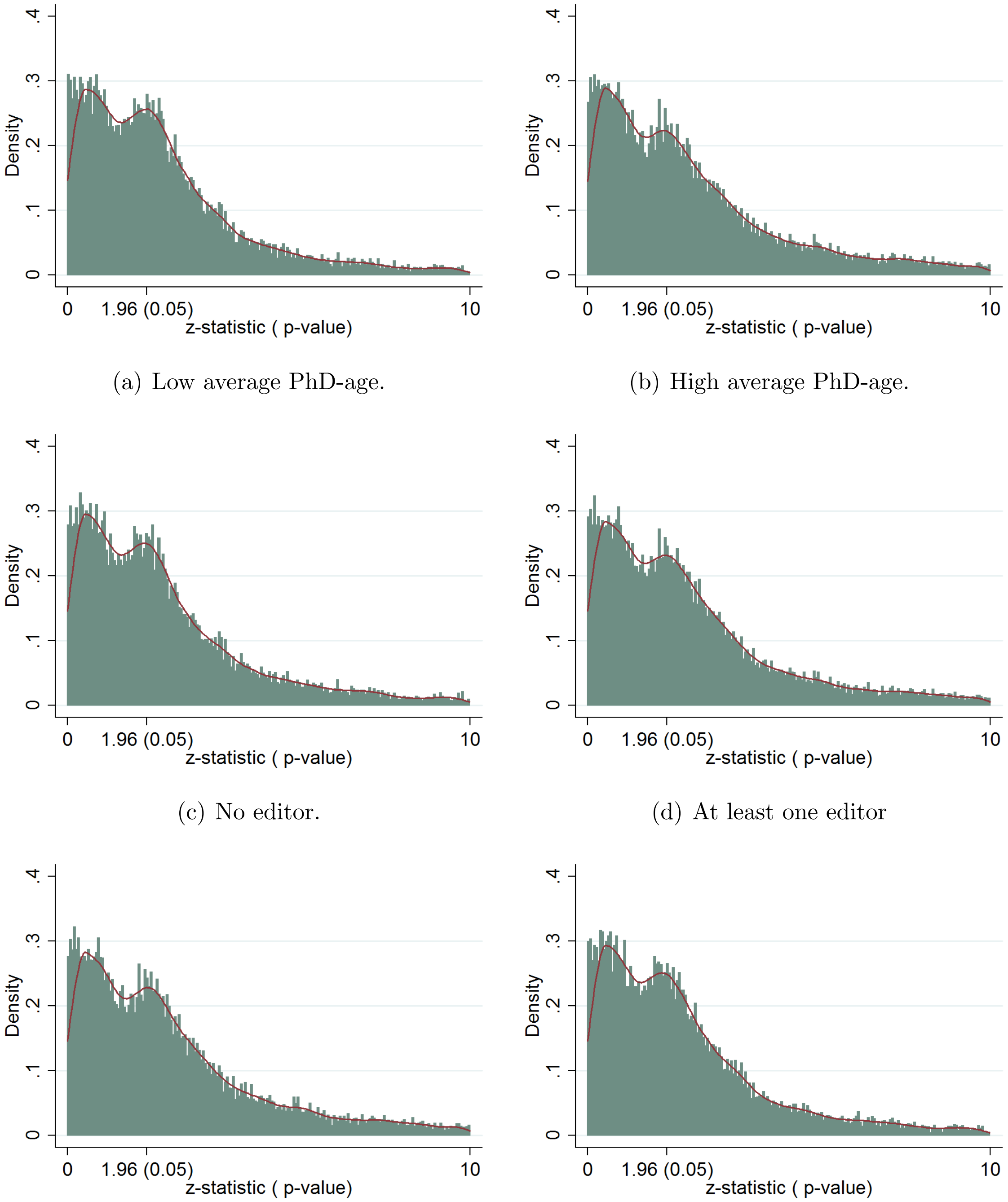
(d) Non-main tables.

(e) Model.

(f) No model.

Sources: AER, JPE, and QJE (2005-2011). Distributions are unweighted and plotted using de-rounded statistics. Lines correspond to kernel density estimates. See figures 12 and 13 in the online appendix for cumulated residuals from non-parametric and parametric estimations using the empirical WDI input.

Figure 6: Distributions of z-statistics for different sub-samples: PhD-age, presence of editors or tenured researchers among authors.



(a) Low average PhD-age.

(b) High average PhD-age.

(c) No editor.

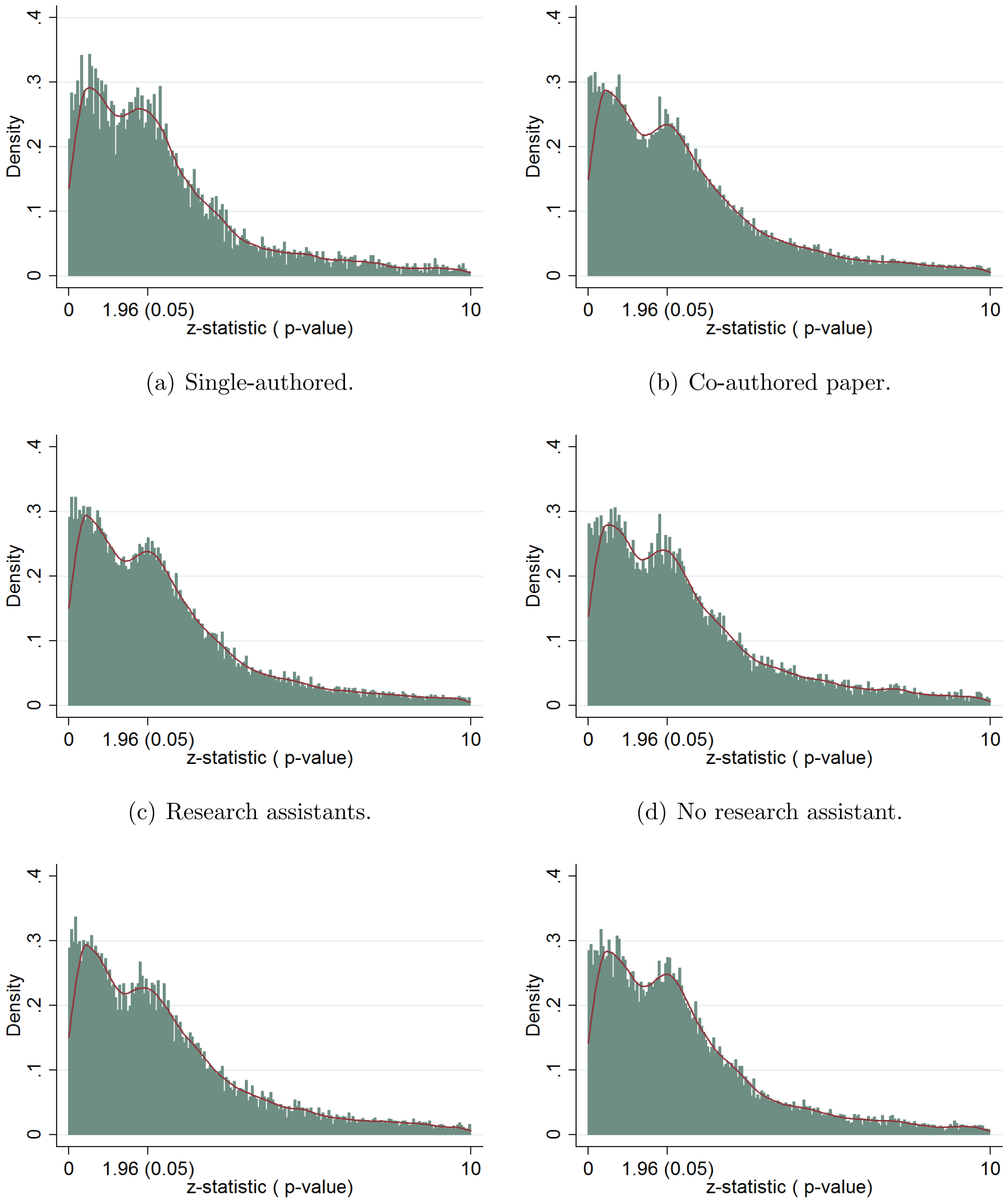
(d) At least one editor

(e) At least one of the authors is surely tenured 3 years before publication.

(f) None of the authors are surely tenured 3 years before publication.

Sources: AER, JPE, and QJE (2005-2011). Distributions are unweighted and plotted using de-rounded statistics. Lines correspond to kernel density estimates. See figures 14 and 15 in the online appendix for cumulated residuals from non-parametric and parametric estimations using the empirical WDI input.

Figure 7: Distributions of z-statistics for different sub-samples: co-authorship, use of research assistants, number of thanks.



(a) Single-authored.

(b) Co-authored paper.

(c) Research assistants.

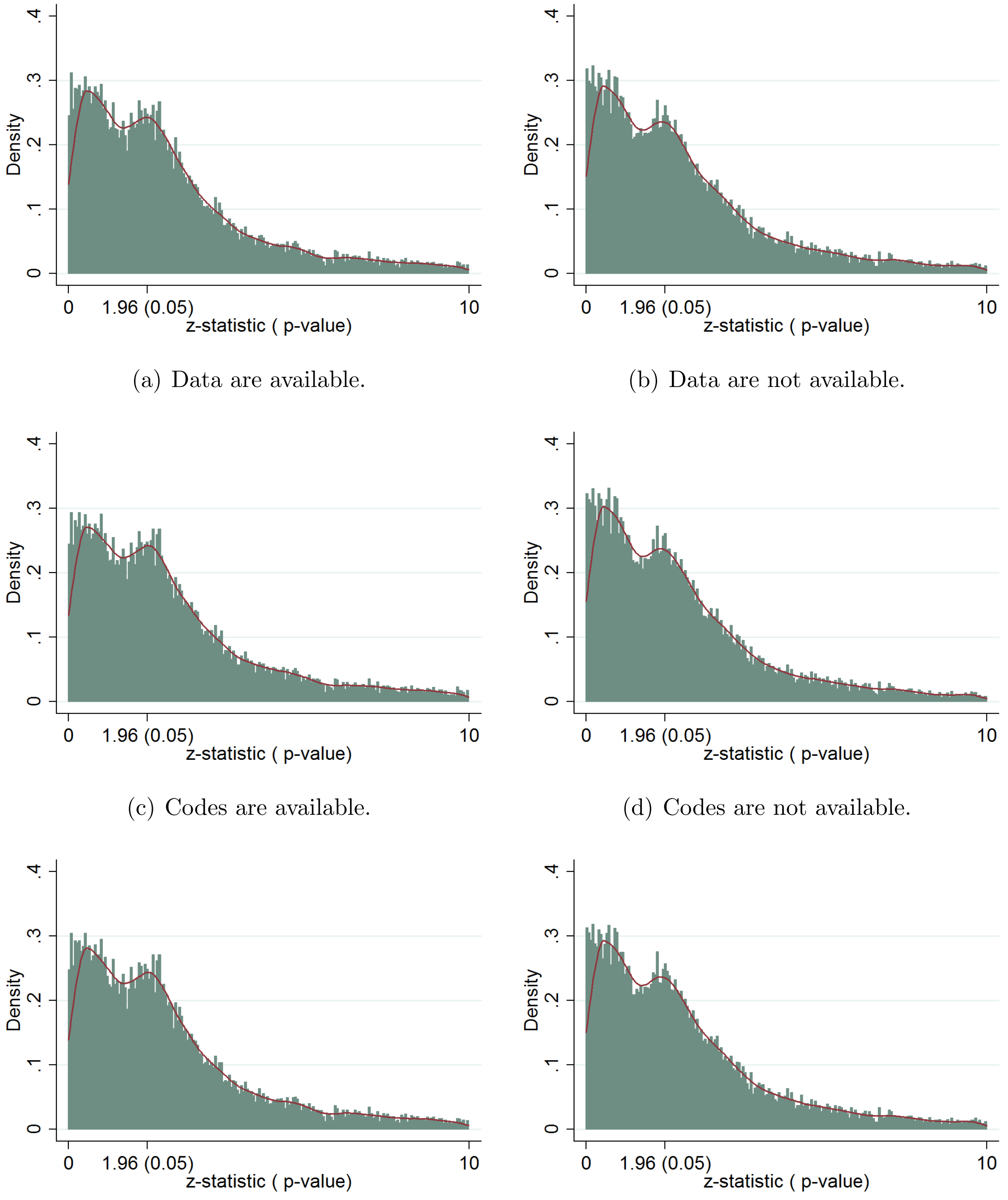
(d) No research assistant.

(e) Low number of thanks.

(f) High number of thanks.

Sources: AER, JPE, and QJE (2005-2011). Distributions are unweighted and plotted using de-rounded statistics. Lines correspond to kernel density estimates. See figures 16 and 17 in the online appendix for cumulated residuals from non-parametric and parametric estimations using the empirical WDI input.

Figure 8: Distributions of z-statistics for different sub-samples: availability of data and codes on the journal's website.



(a) Data are available.

(b) Data are not available.

(c) Codes are available.

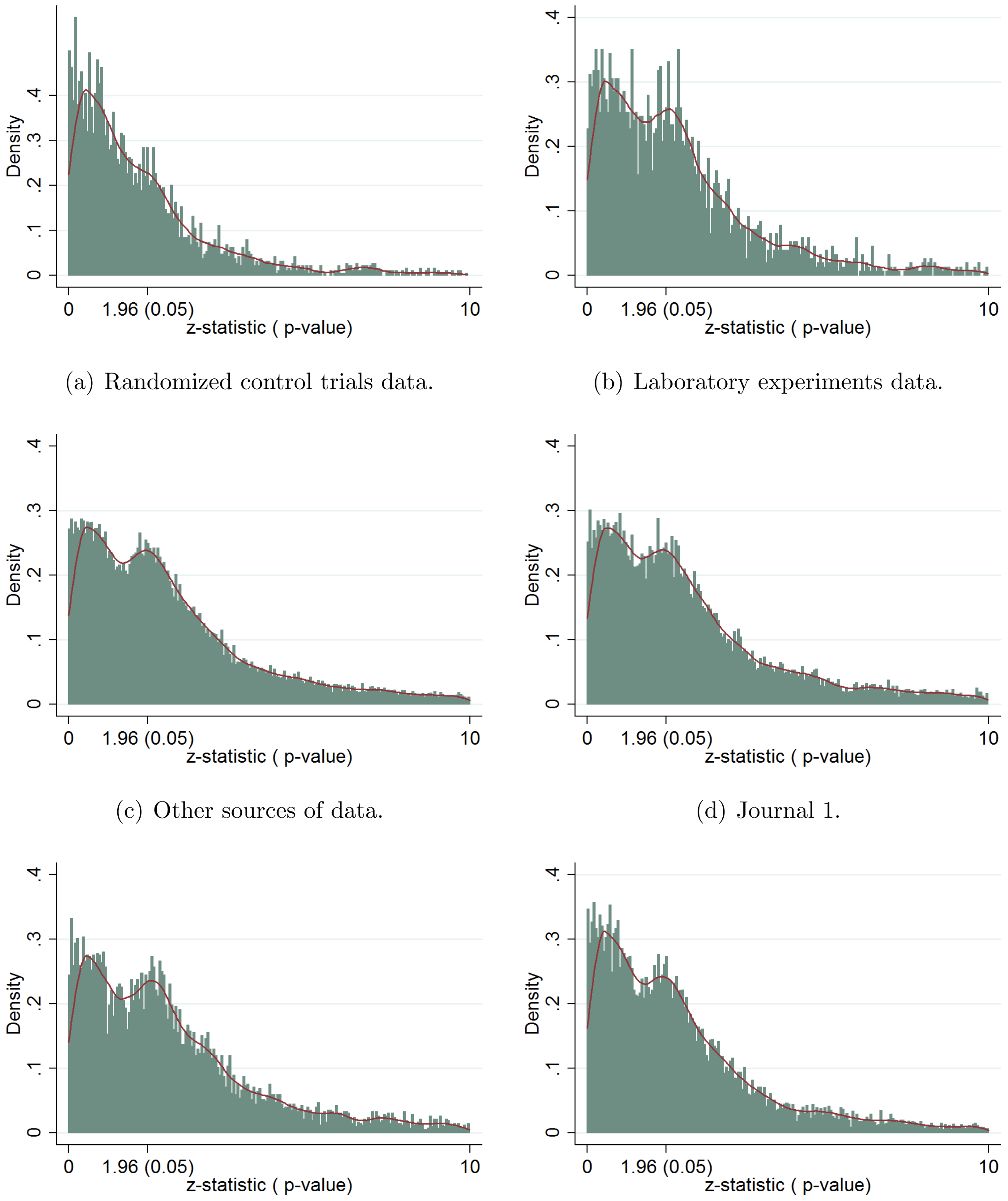
(d) Codes are not available.

(e) Data and codes are available.

(f) Data or codes are not available.

Sources: AER, JPE, and QJE (2005-2011). Distributions are unweighted and plotted using de-rounded statistics. Lines correspond to kernel density estimates. See figures 18 and 19 in the online appendix for cumulated residuals from non-parametric and parametric estimations using the empirical WDI input.

Figure 9: Distributions of z-statistics for different sub-samples: type of data and journal.



(a) Randomized control trials data.

(b) Laboratory experiments data.

(c) Other sources of data.

(d) Journal 1.

(e) Journal 2.

(f) Journal 3.

Sources: AER, JPE, and QJE (2005-2011). Distributions are unweighted and plotted using de-rounded statistics. Lines correspond to kernel density estimates.

Appendix

Proof. Lemma 1.

As f is strictly increasing in e for any given z, there exists a unique h_z such that:

$$f(z,e) \geq F \Leftrightarrow e \geq h_z$$

Note that the function $h : z \mapsto h_z$ should be non-increasing. Otherwise, there would exist $z_1 < z_2$ such that $h_{z_1} < h_{z_2}$. This is absurd as $F = f(z_1, h_{z_1}) \leq f(z_2, h_{z_1}) < f(z_2, h_{z_2}) = F$. This part shows that an increasing function $\tilde{G}$ verifying $\tilde{G}(h(z)) = 1 - g(z)$ can easily be constructed and is uniquely defined on the image of h. Note that G is not uniquely defined outside of this set. This illustrates that G can take any values in the range of contributions where articles are always rejected or accepted irrespectively of their t-statistics.

Finally, we need to show that such a function $\tilde{G}$ can be defined as a surjection $(-\infty, \infty) \mapsto [0,1]$, i.e. $\tilde{G}$ can be the cumulative of a distribution. To verify this, note that on the image of h, $\tilde{G}$ is equal to $1 - g(z)$. Consequently, $\tilde{G}(h([0, T_{lim}])) \subset [0,1]$ and $\tilde{G}$ can always be completed outside of this set to be a surjection.

Note that for any given observed output and any selection function, an infinite sequence $\{G_z\}_z$ may transform the input into the output through f. The intuition is the following: for any given z, the only crucial quantity is how many ε would help pass the threshold. The shape of the distribution above or below the key quality $h(z)$ does not matter. When we limit ourselves to an invariant distribution, G is uniquely determined as $h(z)$ covers the interval of contribution. □

Proof. Corollary 1.

Given lemma 1, the only argument that needs to be made is that the image of the function $\int_0^\infty \int_0^\infty \left[1_{f(z,\varepsilon)\geq F} dG_z(\varepsilon)d\varepsilon\right] \varphi(z)dz \times \tilde{f}$ is in $[0,1]$. To prove this, remark first that the image of $\int_0^\infty \int_0^\infty \left[1_{f(z,\varepsilon)\geq F} dG_z(\varepsilon)d\varepsilon\right] \varphi(z)dz \times \psi/\varphi$ is in $[0,1]$ as it is equal to $\int_0^\infty \left[1_{f(z,\varepsilon)\geq F} dG_z(\varepsilon)d\varepsilon\right]$. Finally, note that $\max_{[0,\infty)}(f) \leq \max_{[0,\infty)}(\psi/\varphi)$ and $\min_{[0,\infty)}(f) \geq \min_{[0,\infty)}(\psi/\varphi)$. Otherwise, the function equal to $\tilde{f}$ but bounded by the bounds of ψ/φ would be a better increasing fit of the ratio ψ/φ. □

Online appendix [For online publication]

Reporting guidelines and rules

Rules we used during the data collecting process are described below.[24] The reporting guidelines for each information are presented in table 3.

The main idea here is to keep things very simple and transparent even if it implies measurement errors.

- *Reporting concerns articles published in the American Economic Review, the Journal of Political Economy, and the Quarterly Journal of Economics between 2005 and 2011, except the AER Papers and Proceedings. All articles that include at least one empirical tests should be included in the sample.*
- *Reporting should be done from the published version of papers.*
- *Report exactly what is printed in the paper. Do not allow the spreadsheet to round anything up (this may be done by setting the data format to text before entering data).*
- *Take all the variables of interest, even if authors state in the comments that it is a good thing that a variable is not significant (an interaction term for example). Define variables of interest by looking at the purpose of each table and at comments of the table in the text. A rule of thumb may be that a variable of interest is commented in the text alongside the paper. Explicit control variables are not variables of interest. However, variables use to investigate "the determinants of" something are variables of interest. When the status of a variable of a test is unclear, be conservative and report it.*
- *Only report the main tables of the paper. That is, report results, but do not report descriptive statistics, group comparisons, and explicit placebo tests. Only report the tables that are commented under the headings "main results", "extensions", or "robustness check". Let us impose that robustness checks and*

[24] As highlighted in the introduction, the census of tests necessitates a good understanding of the argument developed in an article and a strict process avoiding any subjective selection of tests. The first observation restricts the set of potential research assistants to economists and the only economists with a sufficiently low opportunity cost were ourselves. We tackle the second issue by being as conservative as possible, and by avoiding interpretations of the intentions of authors.

extensions should be entitled as such by the authors to be considered as non-main results. Regarding first-stages in a two-stage regression, keep only the second-stage except if the first-stage is presented as a contribution of the paper by itself.

- *See the input mask for the reporting rules.*
- *For tests, fill up one spreadsheet per issue. For authors' data, look for their curriculum vitae using a search engine and fill up information by taking care that some are author-invariant (such as the PhD date), but other vary with published articles (such as status).*

Example of the reporting process

Below an excerpt of an imaginary example. We only report coefficients and standard deviations for the first variable. First-stage is not presented as a major finding by the imaginary researchers and other variables are explicitly presented as control variables. If the authors of this fictive table had put the last three columns in a separate table under the heading "robustness check", we would have considered this new table as a "non-main" table. The table 4 presents the displayed results of this imaginary example.

> *Some of the* 1024 *economists of our dataset engage in some project that requires heavy data capture. It would be optimal that only individuals with a low opportunity cost engage in such an activity. We want to investigate the relationship between the choice of this time-consuming activity and the intrinsic productivity of these agents in their main job. We first estimate the relationship between data capture activity and the number of research papers released by each economist (the number of papers is a proxy for productivity). As shown by column* 1 *in table 4, there is a significant and negative correlation between the probability to engage in data capture and the number of written papers. However, since data capture is a time-consuming activity, we suspect that it may also influence the number of papers written. Thus, the average number of sunny days per year at each department is used as an instrument for productivity. The first-stage regression is reported in column* 2 *and the second stage in column* 3*. The last columns reproduce the same exercise using additional co-variates. We conclude that the decision to engage in data capture is*

definitely negatively related to productivity. Note that controls have the expected sign, in particular the number of co-authors.

Discontinuities

In order to test if there are any discontinuities around the thresholds of significance, we create bins of width 0.00125 and count the number n_z of z-statistics ending up in each bin. This partition produces $1,600$ bins for approximately $20,000$ z-statistics between 0 and 2 in the full sample.

Before turning to the results, let us detail two concerns about our capacity to detect discontinuities. First, formal tests in an article are presented as a set of arguments which smooth the potential discrepancy around the threshold. Second, numbers are often roughly rounded in articles such that it is difficult to know whether a statistic is slightly above or below a certain threshold.

Figure 20 plots n_z for the full sample around 1.65 (10%) and 1.96 (5%). There does not seem to be strong discontinuities around the thresholds. The use of a regression around the thresholds with a trend and a quadratic term $P_2(z)$ confirms the visual impressions. To achieve this, we estimate the following expression:

$$n_z = \gamma \mathbb{1}_{z>\tilde{z}} + P_2(z) + \varepsilon_z, \quad z \in [\tilde{z} - \nu, \tilde{z} + \nu].$$

Table 5 details the different values taken by γ along the different thresholds $\tilde{z} \in \{1.65, 1.96, 2.57\}$ and the different windows $\nu \in \{0.05, 0.10, 0.20, 0.30\}$ around the significance thresholds. The four panels of this table document the estimated discontinuities for the *full* sample, the *eye-catcher* sample, the *no eye-catchers* sample and the *eye-catcher without theoretical contribution* sample.

The small number of observations in windows of width 0.10 and 0.05 does not allow us to seize any effect in these specifications, with very high standard errors. Nonetheless, even focusing on wider windows, the only sizable effect is at the 10% significance threshold: γ is around .02 for samples of tests reported with eye-catchers. Whichever the sample, the other discontinuities are never statistically different from 0. For articles using eye-catchers, the 10%-discontinuity is around 0.02. This implies a jump of density of $\frac{0.02}{0.24} \approx 8\%$ at the threshold.

Overall, the discontinuities are small and concentrated on articles reporting stars and around the 10% threshold. It is to be noted that, if the experimental literature tends to favor 1% or 5% as thresholds for rejecting the null, the vast majority of empirical papers now considers the 10% threshold as the first level for significance. Our results tend to illustrate the adoption of 10% as a norm.

These findings are of small amplitude, maybe because of the smoothing or because there are multiple tests in a same article. More importantly, even the 10% discontinuity is hard to interpret. Authors might select tests who pass the significance thresholds among the set of acceptable specifications and only show part of the whole range of inferences. But it might also be that journals prefer significant results or authors censor themselves, expecting journals to be harsher with unsignificant results.

An extension to the Benford's law

Following the seminal paper of Benford (1938) and its extension by Hill (1995, 1998), the leading digit of numbers taken from scale-invariant data or selected from a lot of different sources should follow a logarithmic distribution. Tests of this law are applied to situations such as tax monitoring in order to see whether reported figures are manipulated or not.

The intuition is that we are in the precise situation in which the Benford's law should hold: we group coefficients from different sources, with different units and thus different scales. According to Hill (1995, 1998), the probability for one of our collected coefficients to have a leading digit equal to $d \in \{0, \dots, 9\}$ is $\log_{10}(1 + 1/d)$.

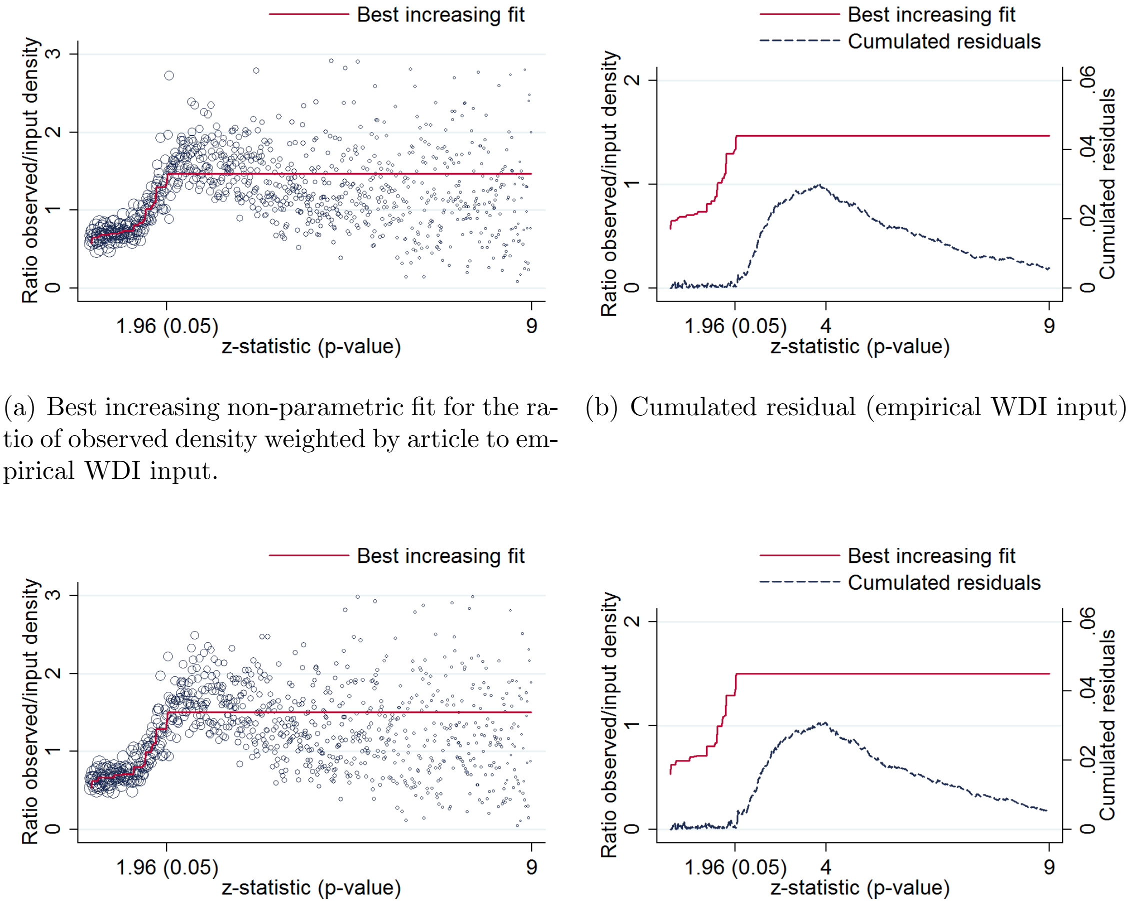
We group coefficients and standard deviations taken from our different articles (with different units). z-statistics are not used in this analysis as it is not scale-invariant and normalized across the different articles. For both coefficients and standard errors, we compute the empirical probability r_d to have a leading digit equal to $d \in \{0, \dots, 9\}$.

The first columns of table 6 (panel A for the coefficients, panel B for the standard errors) display the theoretical probabilities and the empirical counterparts for three samples: the full sample, the sample of coefficients and standard errors for which the associated z-statistic was in the $[1.65, 6]$ interval, and the others. All samples seem incredibly close to the theoretical predictions. The distance to the theoretical prediction can be summarized by the statistic $U = \sqrt{\sum_{d=1}^{\infty}(r_d - log_{10}(1 + 1/d))^2}$.

A concern is that we have no benchmark upon which we can rely: the distance to the theoretical prediction seems small but it may be because of the very large number of observations. We re-use our random regressions on WDI, and randomly extract approximately as many coefficients and standard deviations as in the real sample. We already know that the distribution of z-statistics are quite close; the decomposition of coefficients and standard errors with z-statistics between 1.65 and 6 should partition the placebo sample in similar proportions as the real sample. The three last columns of panels A and B present the results on this placebo sample.

The comparison of the two sets of results indicates that the distance of the full sample to the theoretical predictions is higher than with the placebo both for coefficients ($U_s = 0.0089$ against $U_p = 0.0058$) and for standard errors ($U_s = 0.0117$ against $U_p = 0.0046$). The analysis across subsample is less straightforward. As regards the coefficients, the relative distance compared to the placebo is particularly large between 1.65 and 6 ($U_s = 0.0161$ against $U_p = 0.0036$ for the $[1.65, 6]$ sample, and $U_s = 0.0081$ against $U_p = 0.0093$ for the rest). It seems however that this observation does not hold for standard errors. Naturally, part of this discrepancy can be explained by the differences in the number of observations: there are less numbers reported between 1.65 and 6 in the placebo test. To conclude, this Benford analysis provides some evidence indicating non-randomness in our universe of tests.

Figure 10: Non-parametric estimations of selection and inflation for weighted distributions.



(a) Best increasing non-parametric fit for the ratio of observed density weighted by article to empirical WDI input.

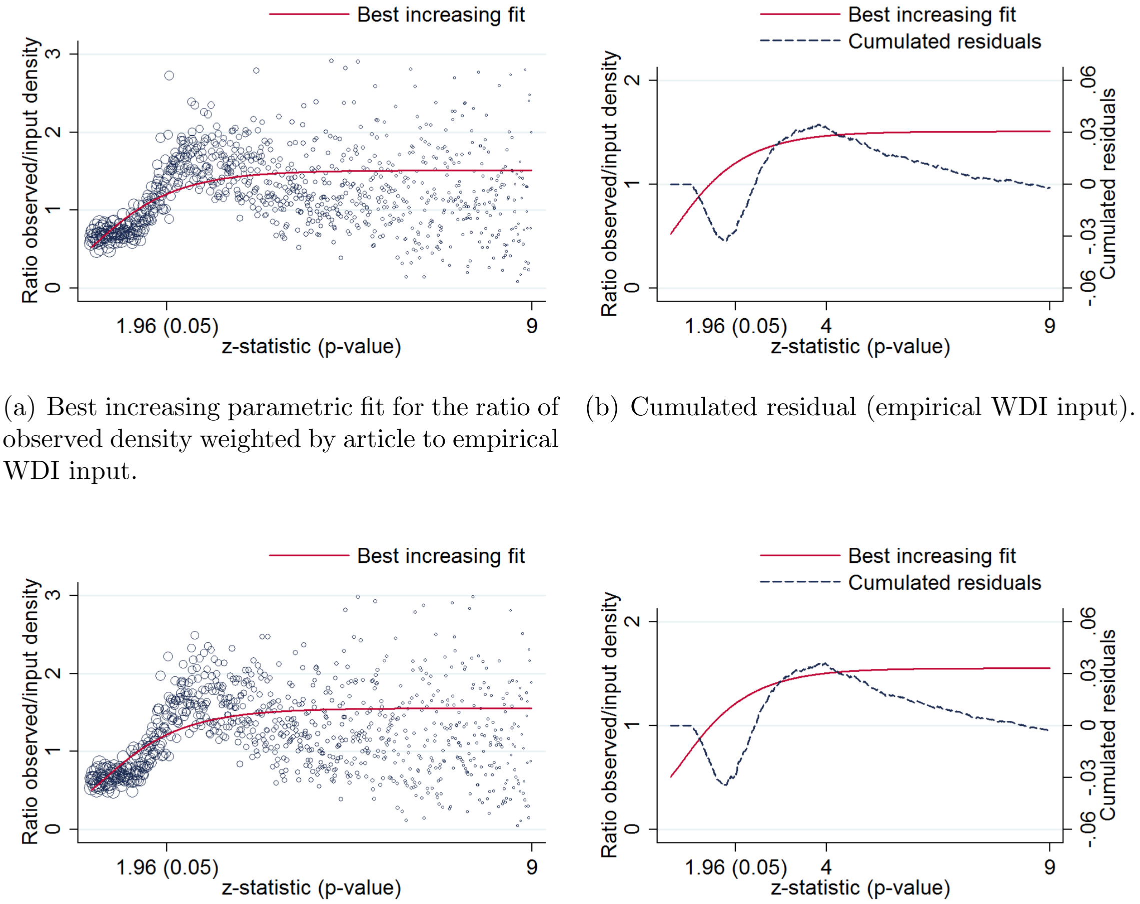
(b) Cumulated residual (empirical WDI input).

(c) Best increasing non-parametric fit for the ratio of observed density weighted by table to empirical WDI input.

(d) Cumulated residual (empirical WDI input).

Sources: AER, JPE, and QJE (2005-2011).

Figure 11: Parametric estimations of selection and inflation for weighted distributions.



(a) Best increasing parametric fit for the ratio of observed density weighted by article to empirical WDI input.

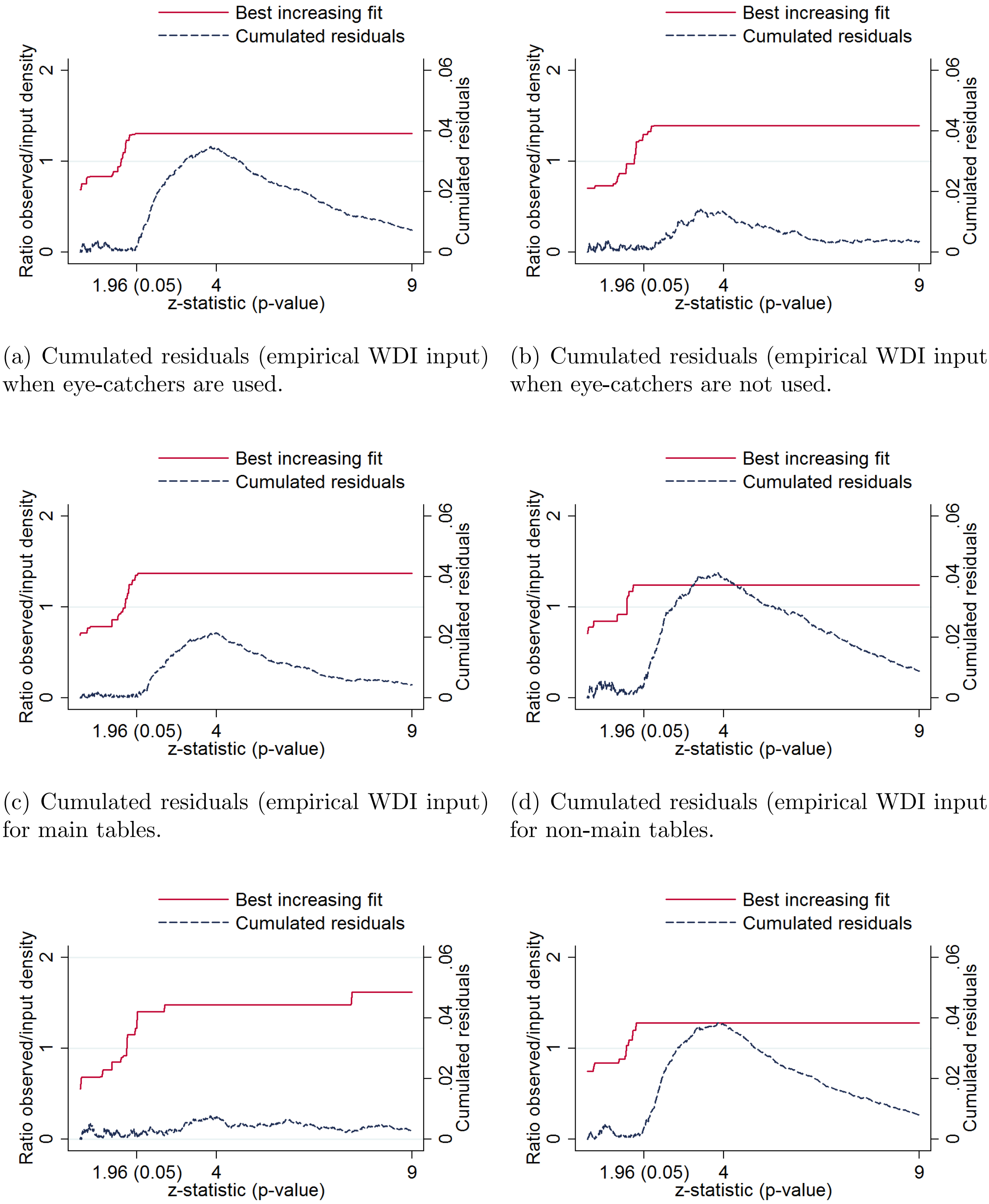
(b) Cumulated residual (empirical WDI input).

(c) Best increasing parametric fit for the ratio of observed density weighted by table to empirical WDI input.

(d) Cumulated residual (empirical WDI input).

Sources: AER, JPE, and QJE (2005-2011).

Figure 12: Cumulated residuals (from non-parametric estimation) for different subsamples: eye-catchers, theoretical contribution and status of result.



(a) Cumulated residuals (empirical WDI input) when eye-catchers are used.

(b) Cumulated residuals (empirical WDI input) when eye-catchers are not used.

(c) Cumulated residuals (empirical WDI input) for main tables.

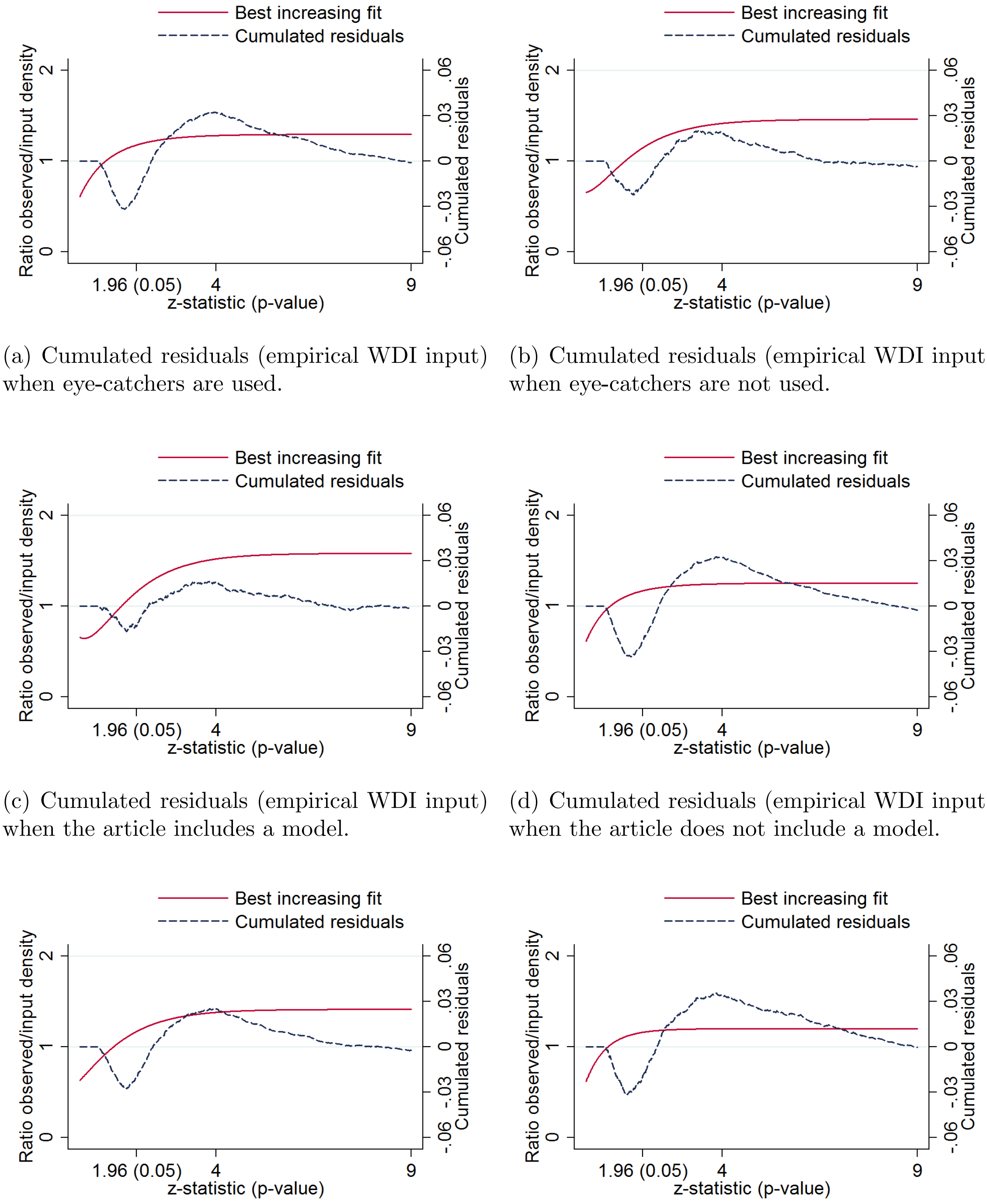
(d) Cumulated residuals (empirical WDI input) for non-main tables.

(e) Cumulated residuals (empirical WDI input) when the article includes a model.

(f) Cumulated residuals (empirical WDI input) when the article does not include a model.

Sources: AER, JPE, and QJE (2005-2011).

Figure 13: Cumulated residuals (from parametric estimation) for different subsamples: eye-catchers, theoretical contribution and status of result.



(a) Cumulated residuals (empirical WDI input) when eye-catchers are used.

(b) Cumulated residuals (empirical WDI input) when eye-catchers are not used.

(c) Cumulated residuals (empirical WDI input) when the article includes a model.

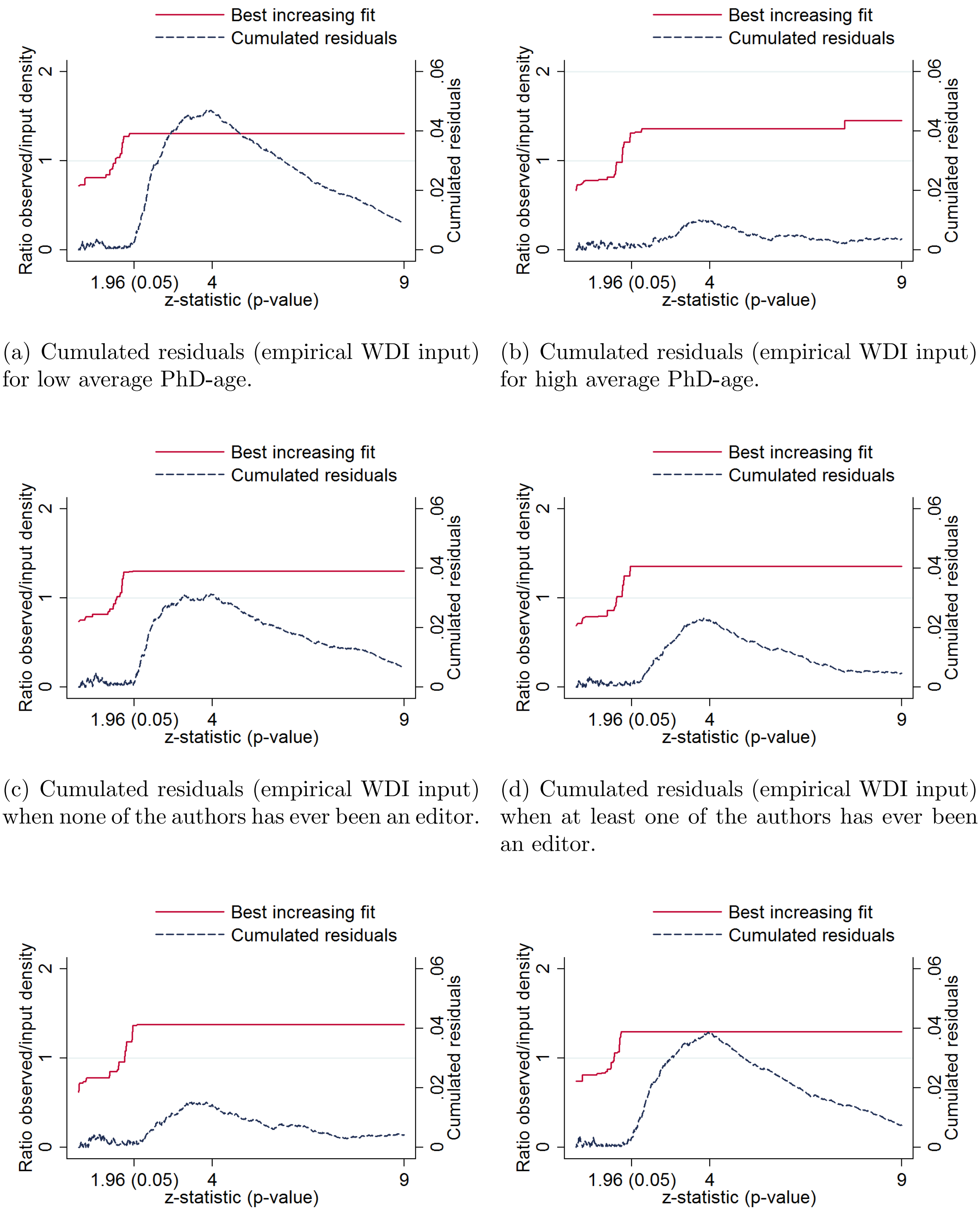
(d) Cumulated residuals (empirical WDI input) when the article does not include a model.

(e) Cumulated residuals (empirical WDI input) for main tables.

(f) Cumulated residuals (empirical WDI input) for non-main tables.

Sources: AER, JPE, and QJE (2005-2011).

Figure 14: Cumulated residuals (from non-parametric estimation) for different subsamples: PhD-age, presence of editors or tenured researchers among authors.



(a) Cumulated residuals (empirical WDI input) for low average PhD-age.

(b) Cumulated residuals (empirical WDI input) for high average PhD-age.

(c) Cumulated residuals (empirical WDI input) when none of the authors has ever been an editor.

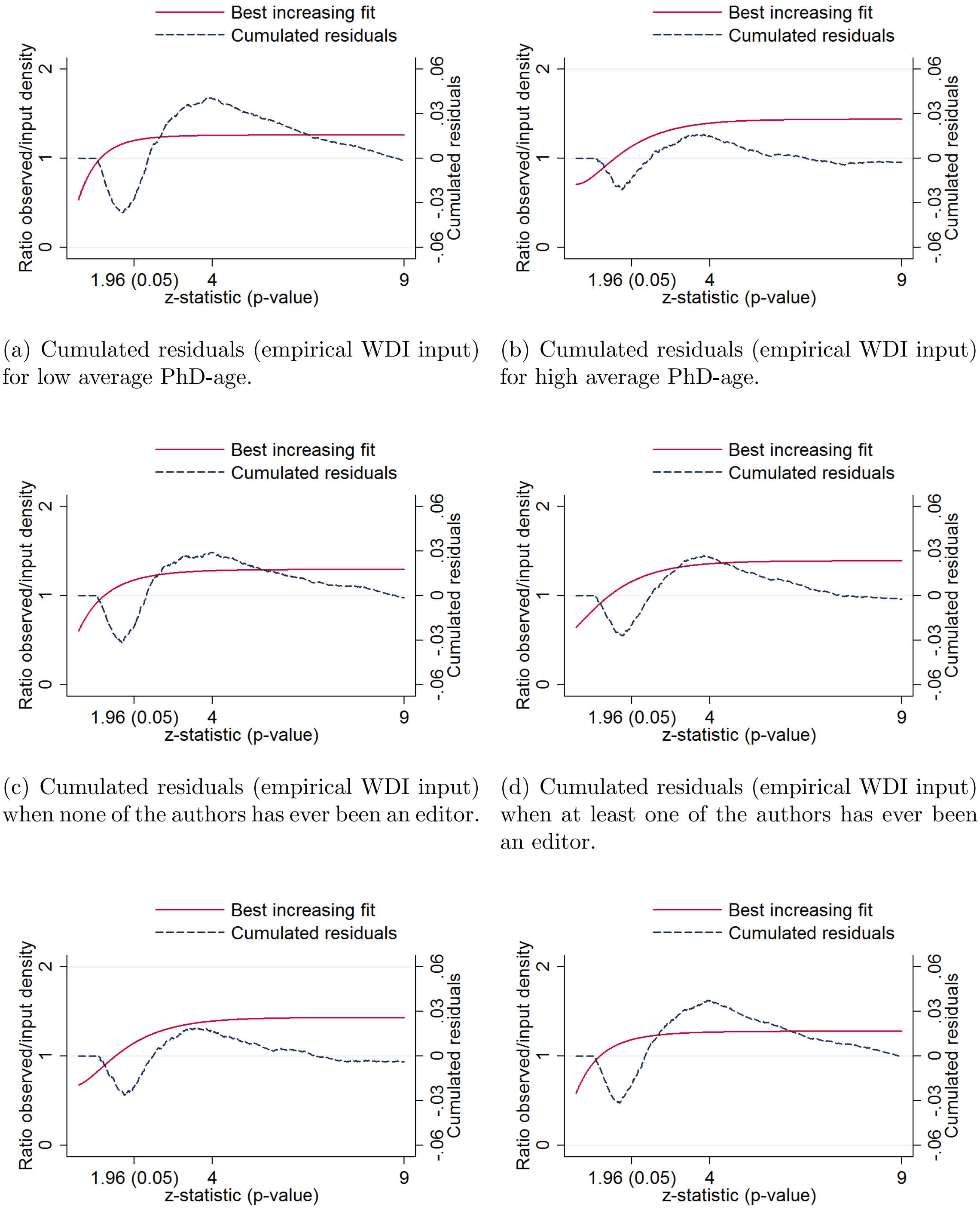
(d) Cumulated residuals (empirical WDI input) when at least one of the authors has ever been an editor.

(e) Cumulated residuals (empirical WDI input) when at least one of the authors is surely tenured 3 years before publication.

(f) Cumulated residuals (empirical WDI input) when none of the authors are surely tenured 3 years before publication.

Sources: AER, JPE, and QJE (2005-2011).

Figure 15: Cumulated residuals (from parametric estimation) for different subsamples: PhD-age, presence of editors or tenured researchers among authors.



(a) Cumulated residuals (empirical WDI input) for low average PhD-age.

(b) Cumulated residuals (empirical WDI input) for high average PhD-age.

(c) Cumulated residuals (empirical WDI input) when none of the authors has ever been an editor.

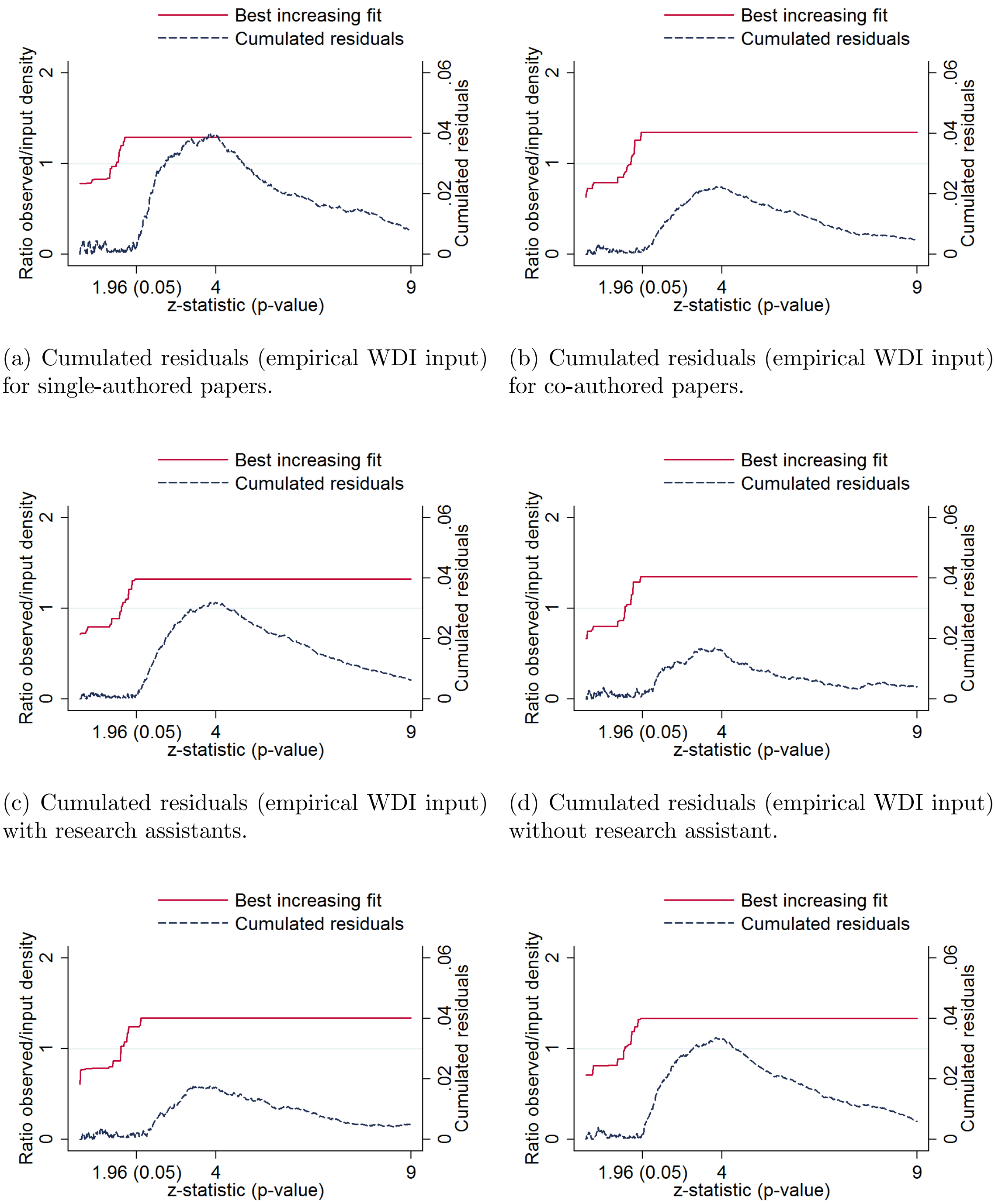
(d) Cumulated residuals (empirical WDI input) when at least one of the authors has ever been an editor.

(e) Cumulated residuals (empirical WDI input) when at least one of the authors is surely tenured 3 years before publication.

(f) Cumulated residuals (empirical WDI input) when none of the authors are surely tenured 3 years before publication.

Sources: AER, JPE, and QJE (2005-2011).

Figure 16: Cumulated residuals (from non-parametric estimation) for different subsamples: co-authorship, use of research assistants, number of thanks.



(a) Cumulated residuals (empirical WDI input) for single-authored papers.

(b) Cumulated residuals (empirical WDI input) for co-authored papers.

(c) Cumulated residuals (empirical WDI input) with research assistants.

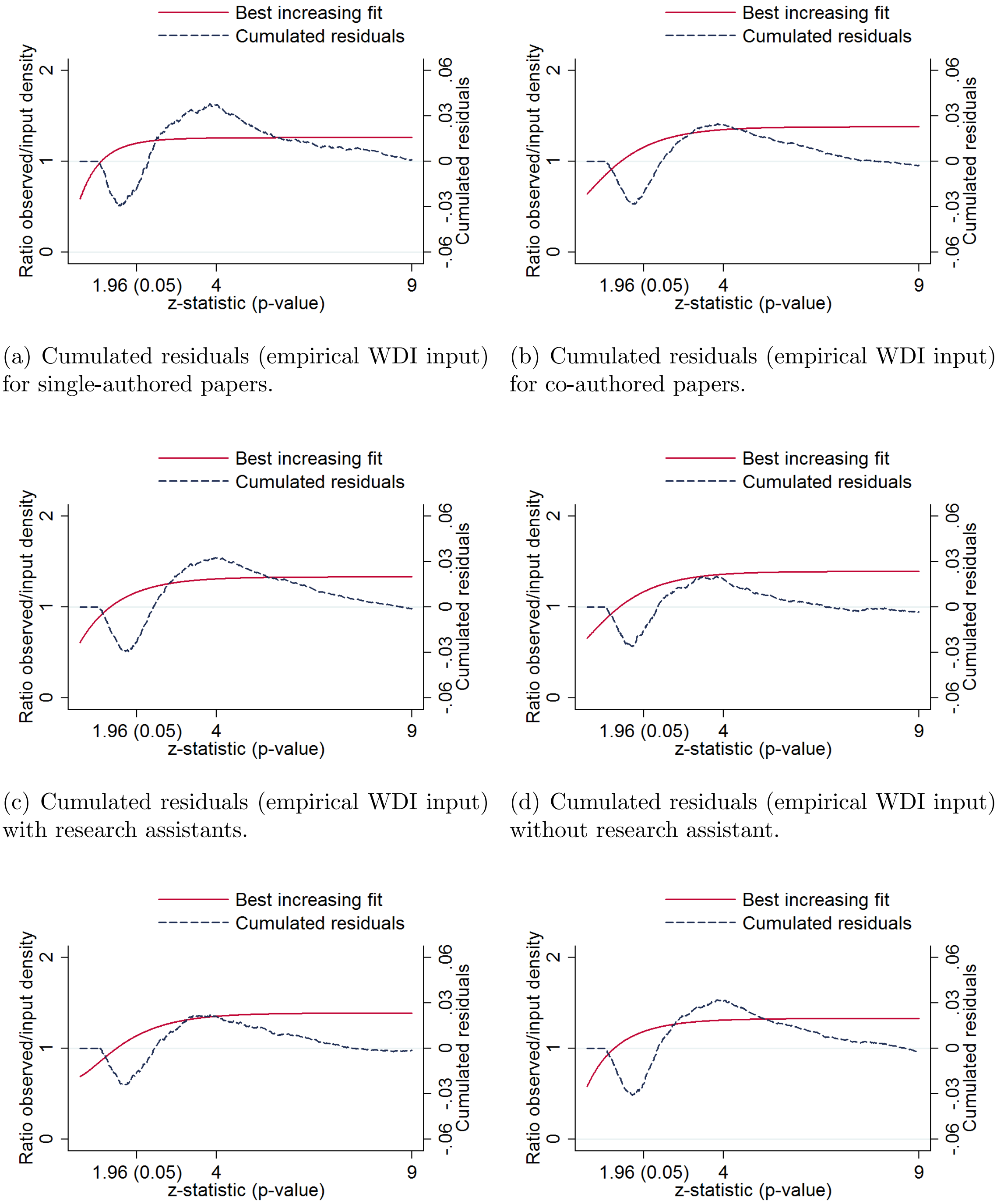
(d) Cumulated residuals (empirical WDI input) without research assistant.

(e) Cumulated residuals (empirical WDI input) for low number of thanks.

(f) Cumulated residuals (empirical WDI input) for high number of thanks.

Sources: AER, JPE, and QJE (2005-2011).

Figure 17: Cumulated residuals (from parametric estimation) for different subsamples: co-authorship, use of research assistants, number of thanks.



(a) Cumulated residuals (empirical WDI input) for single-authored papers.

(b) Cumulated residuals (empirical WDI input) for co-authored papers.

(c) Cumulated residuals (empirical WDI input) with research assistants.

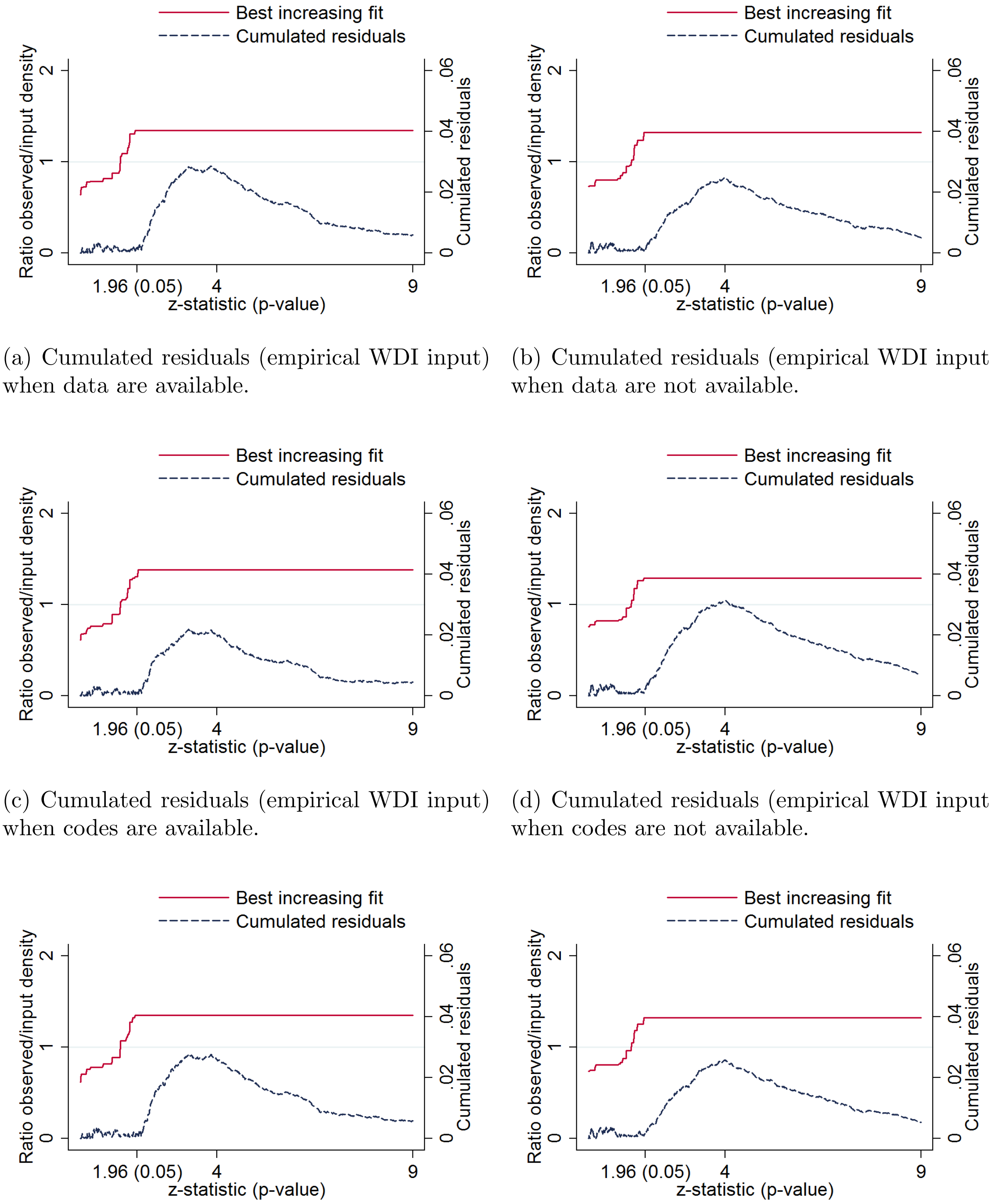
(d) Cumulated residuals (empirical WDI input) without research assistant.

(e) Cumulated residuals (empirical WDI input) for low number of thanks.

(f) Cumulated residuals (empirical WDI input) for high number of thanks.

Sources: AER, JPE, and QJE (2005-2011).

Figure 18: Cumulated residuals (from non-parametric estimation) for different subsamples: availability of data and codes on the journal's website.



(a) Cumulated residuals (empirical WDI input) when data are available.

(b) Cumulated residuals (empirical WDI input) when data are not available.

(c) Cumulated residuals (empirical WDI input) when codes are available.

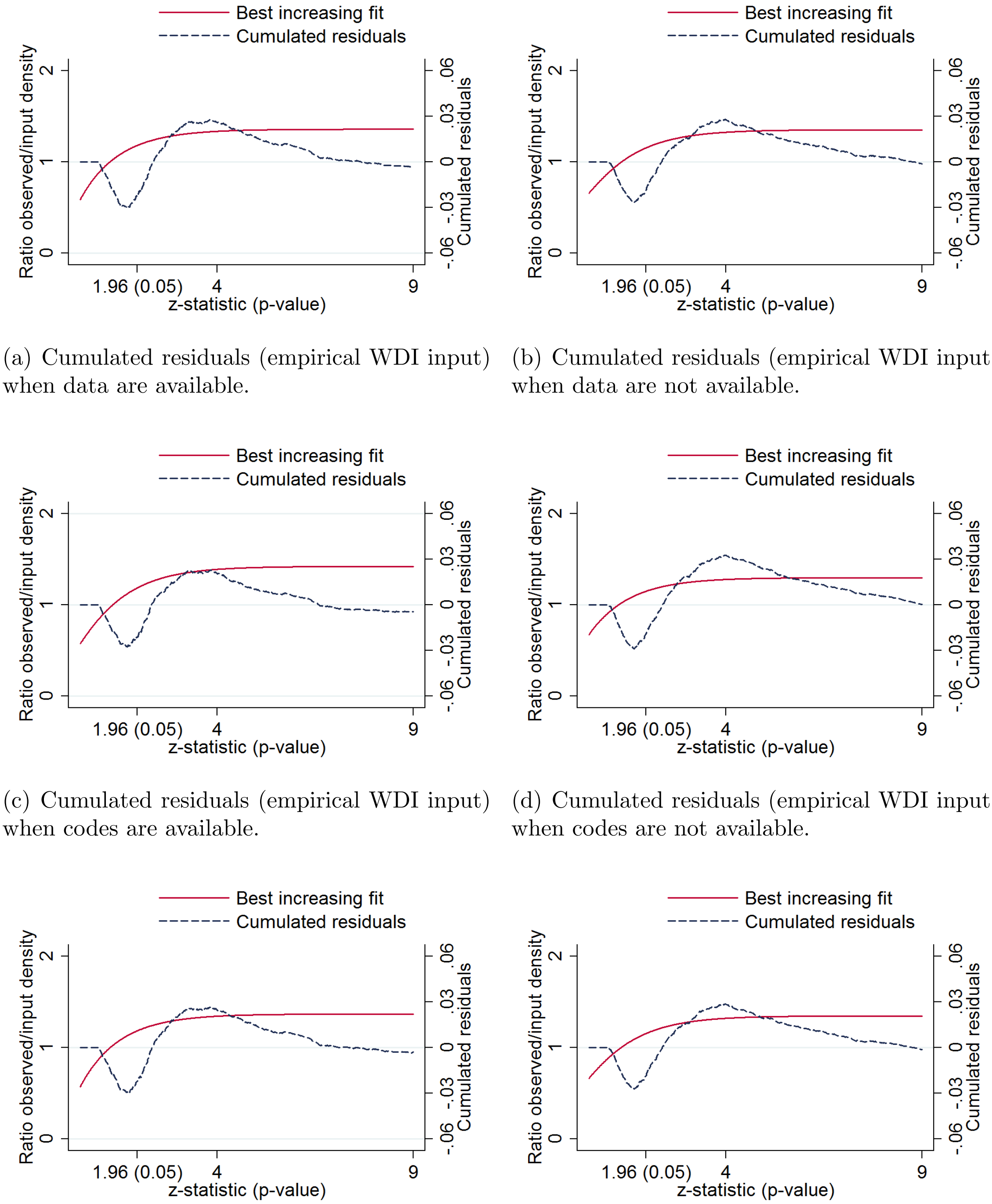
(d) Cumulated residuals (empirical WDI input) when codes are not available.

(e) Cumulated residuals (empirical WDI input) when data and codes are available.

(f) Cumulated residuals (empirical WDI input) when data or codes are not available.

Sources: AER, JPE, and QJE (2005-2011).

Figure 19: Cumulated residuals (from parametric estimation) for different subsamples: availability of data and codes on the journal's website.



(a) Cumulated residuals (empirical WDI input) when data are available.

(b) Cumulated residuals (empirical WDI input) when data are not available.

(c) Cumulated residuals (empirical WDI input) when codes are available.

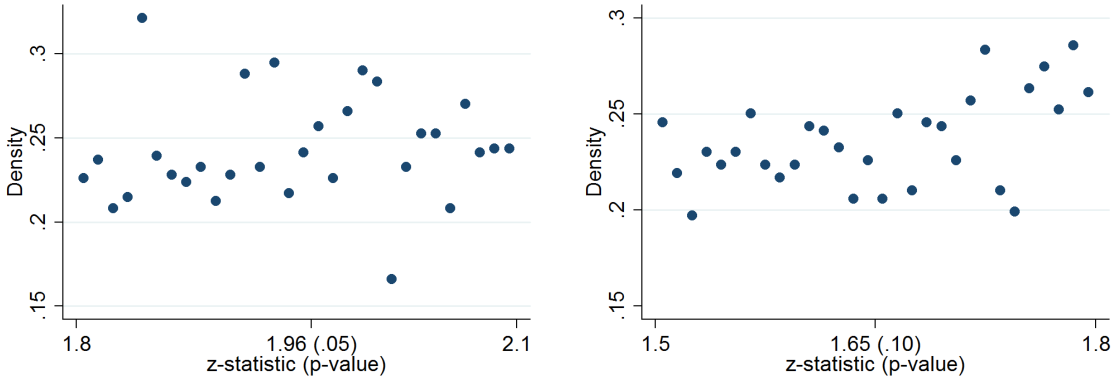
(d) Cumulated residuals (empirical WDI input) when codes are not available.

(e) Cumulated residuals (empirical WDI input) when data and codes are available.

(f) Cumulated residuals (empirical WDI input) when data or codes are not available.

Sources: AER, JPE, and QJE (2005-2011).

Figure 20: Discontinuitites around the thresholds (values grouped by bins of bandwith 0.1).



(a) Around 1.96 (.05).

(b) Around 1.65 (.10).

Sources: AER, JPE, and QJE (2005-2011).

Table 3: Reported information.

ra	"Research assistant" identifier. Use your initials.
Article-specific variables	
journal_id	Journal identifier. Use full text, e.g. "American Economic Review", "Journal of Political Economy", and "Quarterly Journal of Economics".
issue	Enter as $XXX.Y$, where XXX is *volume* and Y is *issue*.
year	Enter as ♯♯♯♯.
article_page	Enter the number of the article's first page in the issue.
first_author	Last name of the first author as it appears.
num_authors	Number of authors.
jel	Enter JEL codes as a dot-separated list if present.
model	Does the paper include a theoretical contribution? Answer by "yes" or "no".
ras	Enter the number of research assistants thanked.

Continued on next page

Table 3: Reported information (continued).

thanks	Enter the number of individuals thanked (excluding research assistant and referees).
Test-specific variables	
type	Type of data used. Three choices: “expe” for experimental data, “rct” for data obtained from randomized control trials, and “other” for any other type of data.
table_panel	Enter the table identifier. If a table includes several parts (e.g. panels), also enter the panel identifier. In case where results are presented only in the text, create a separate identifier for each “group” of results in the paper.
type_emp	Statistical method or type of test used.
row	Row identifier when referring to a table. Enter the page number when referring to results presented in the text.
column	Column identifier when referring to a table. Enter the order of appearance when referring to results presented in the text.
stars	Does the paper use eye-catchers (e.g. stars or bold printing) to highlight statistical significance? Answer by “yes” or “no”.
main	Enter “yes” or “no”. For simplicity, assign the same status to all tests presented in the same table or results’group.
coefficient	Enter the coefficient.
standard_deviation	Enter the standard deviation. Create a variable called “standard_deviation_2” if authors reported multiple standard deviations (e.g. first is clustered, the second is not).
t_stat	Enter the test statistic. Create a variable called “t_stat_2” if authors reported multiple statistics.
p_value	Enter the p-value. Create a variable called “p_value_2” if authors reported multiple p-values.

Continued on next page

Table 3: Reported information (continued).

Author-specific variables	
phd	PhD institution. Enter “Unknown” if you cannot find the information.
phd_date	Enter as ♯♯♯♯ the year at which the PhD was awarded. Enter the expected date if necessary. Enter “Unknown” if you cannot find the information.
Author×article-specific variables	
author	Author’s name
status	Enter the status of the author at time of publication. Enter “Unknown” if you cannot find the information.
status_1y	Enter the status of the author one year before publication.
status_2y	Enter the status of the author two years before publication.
status_3y	Enter the status of the author three years before publication.
editor	Is the author an editor or a member of an editorial board at time of publication? Answer by “yes” or “no”. Enter “Unknown” if you cannot find the information.
editor_before	Was the author an editor or a member of an editorial board before publication? Answer by “yes” or “no”.
affiliation_1	Enter the author’s first affiliation as it appears on the published version of the article.
affiliation_2	Enter the author’s other affiliations as they appear on the published version of the article.

Table 4: Imaginary example: Activity choice and productivity among economists.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	First stage	IV	OLS	First stage	IV
Dependent variable:	Data capture	Papers	Data capture	Data capture	Papers	Data capture
Papers	-0.183***		-0.190	-0.189***		-0.243*
	(0.024)		(0.162)	(0.018)		(0.134)
Sunny days		-0.360***			-0.372***	
		(0.068)			(0.068)	
Gender				-0.028	0.090**	-0.067**
				(0.023)	(0.039)	(0.031)
Size of department				-0.000	0.001	-0.000
				(0.001)	(0.002)	(0.002)
Number of co-authors				0.223***	0.004	0.222***
				(0.008)	(0.014)	(0.010)
Country fixed effects				Yes	Yes	Yes
Observations	1,024	1,024	1,024	1,024	1,024	1,024
R-squared	0.054	0.027		0.475	0.042	

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors in parentheses. Data are imaginary. The model is a linear probability model estimated with ordinary least squares. *Data capture* is a dummy equal to 1 if the economist captures data. *Papers* is the number of papers (including working papers). *Sunny days* is the number of sunny days per year at each department. *Size of the department* is the size of the department. *Number of co-authors* is the number of co-authors. In columns 3 and 6, *papers* is instrumented by *sunny days*. All regressions include a constant term.

Table 5: Amplitude of discontinuities around the 0.10, 0.05, and 0.01 significance thresholds.

Panel A	Full sample				Panel B	Eye-catchers			
Window width	0.30	0.20	0.10	0.05	Window width	0.30	0.20	0.10	0.05
Observations	480	320	160	80	Observations	480	320	160	80
0.10 threshold					0.10 threshold				
γ	0.0157	0.0033	0.0168	0.0262	γ	0.0268	0.0204	0.0206	0.0365
	(0.0115)	(0.0144)	(0.0207)	(0.0330)		(0.0150)	(0.0186)	(0.0266)	(0.0393)
	[0.232]	[0.232]	[0.232]	[0.228]		[0.238]	[0.238]	[0.242]	[0.240]
0.05 threshold					0.05 threshold				
γ	0.0049	0.0152	0.0064	0.0015	γ	0.0160	0.0187	0.0106	0.0018
	(0.0121)	(0.0147)	(0.0189)	(0.0247)		(0.0147)	(0.0180)	(0.0235)	(0.0310)
	[0.241]	[0.244]	[0.242]	[0.260]		[0.248]	[0.253]	[0.251]	[0.273]
0.01 threshold					0.01 threshold				
γ	0.0009	-0.0051	-0.0061	0.0103	γ	0.0106	0.0075	0.0047	0.0341
	(0.0104)	(0.0129)	(0.0187)	(0.0263)		(0.0125)	(0.0155)	(0.0232)	(0.0348)
	[0.187]	[0.187]	[0.182]	[0.176]		[0.188]	[0.187]	[0.182]	[0.184]
Panel C	No eye-catchers				Panel D	Eye-catchers and no model			
Window width	0.30	0.20	0.10	0.05	Window width	0.30	0.20	0.10	0.05
Observations	480	320	160	80	Observations	480	320	160	80
0.10 threshold					0.10 threshold				
γ	-0.0086	-0.0306	0.0130	0.0113	γ	0.0225	0.0280	0.0295	0.0293
	(0.0214)	(0.0266)	(0.0385)	(0.0613)		(0.0170)	(0.0211)	(0.0296)	(0.0448)
	[0.225]	[0.223]	[0.213]	[0.204]		[0.246]	[0.248]	[0.255]	[0.255]
0.05 threshold					0.05 threshold				
γ	-0.0279	-0.0003	-0.0148	-0.0116	γ	-0.0048	-0.0152	-0.0223	-0.0637
	(0.0222)	(0.0281)	(0.0405)	(0.0586)		(0.0173)	(0.0212)	(0.0284)	(0.0386)
	[0.230]	[0.232]	[0.232]	[0.244]		[0.251]	[0.254]	[0.251]	[0.272]
0.01 threshold					0.01 threshold				
γ	-0.0192	-0.0253	-0.0284	-0.0379	γ	0.0160	0.0236	0.0287	0.0420
	(0.0186)	(0.0229)	(0.0331)	(0.0504)		(0.0145)	(0.0179)	(0.0260)	(0.0385)
	[0.192]	[0.191]	[0.184]	[0.165]		[0.190]	[0.190]	[0.186]	[0.190]

Each element of the table is the result of a separate linear regression. $n_z = \gamma(z > \tilde{z}) + P_2(z) + \varepsilon_z, \quad z \in [\tilde{z} - \nu, \tilde{z} + \nu]$ where ν and $\tilde{z}$ are the window width and the threshold considered and n_z the density of z-statistics in each bin. Robust standard errors are reported in parentheses. The mean of the variable of interest–the density of the distribution in the window–is reported between brackets. The results are shown omitting the coefficients for the polynomial controls.

Table 6: Tests of Benford's law on the full sample, and partition $[1.65, 6]$ vs. $[0, 1.65) \cup (6, \infty)$.

Panel A		Coefficients					
			Collected data			Simulated data	
Leading digit	Theoretical	Full sample	$[1.65, 6]$	$[0, 1.65) \cup (6, \infty)$	Full sample	$[1.65, 6]$	$[0, 1.65) \cup (6, \infty)$
1	0.3010	0.3052	0.3095	0.3017	0.3016	0.3026	0.3011
2	0.1761	0.1692	0.1675	0.1706	0.1735	0.1774	0.1717
3	0.1249	0.1240	0.1166	0.1301	0.1283	0.1244	0.1302
4	0.0969	0.0981	0.0972	0.0989	0.0937	0.0973	0.0920
5	0.0792	0.0772	0.0767	0.0776	0.0809	0.0796	0.0816
6	0.0669	0.0666	0.0676	0.0657	0.0658	0.0669	0.0653
7	0.0580	0.0592	0.0604	0.0583	0.0586	0.0559	0.0599
8	0.0512	0.0524	0.0542	0.0509	0.0510	0.0520	0.0506
9	0.0458	0.0480	0.0503	0.0461	0.0464	0.0439	0.0477
U		0.0089	0.0161	0.0081	0.0058	0.0036	0.0093
Observations		43,954	19,892	24,062	43,297	14,015	29,282

Panel B		Standard errors					
			Collected data			Simulated data	
Leading digit	Theoretical	Full sample	$[1.65, 6]$	$[0, 1.65) \cup (6, \infty)$	Full sample	$[1.65, 6]$	$[0, 1.65) \cup (6, \infty)$
1	0.3010	0.2928	0.2955	0.2906	0.3002	0.3025	0.2992
2	0.1761	0.1751	0.1798	0.1711	0.1732	0.1730	0.1733
3	0.1249	0.1267	0.1283	0.1253	0.1277	0.1279	0.1276
4	0.0969	0.1046	0.1029	0.1060	0.0974	0.0945	0.0988
5	0.0792	0.0803	0.0803	0.0803	0.0801	0.0808	0.0798
6	0.0669	0.0649	0.0634	0.0662	0.0680	0.0664	0.0687
7	0.0580	0.0584	0.0556	0.0608	0.0569	0.0581	0.0564
8	0.0512	0.0510	0.0478	0.0537	0.0517	0.0533	0.0509
9	0.0458	0.0462	0.0466	0.0459	0.0448	0.0436	0.0453
U		0.0117	0.0111	0.0153	0.0046	0.0062	0.0053
Observations		42,422	19,200	23,222	42,792	14,015	28,777

This table reports the ratios of coefficients and standard errors starting with each digit $1, ..., 9$. The formula for the theoretical ratios is $\log_{10}(1 + 1/d)$ for the leading digit d. U is the square root of the sum of squares of differences between the actual and the theoretical ratio. Simulated data correspond to regressions using random variables drawn from the WDI dataset.