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Are American astrophysics papers accepted more quickly than others?

Part I

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Abstract It has been shown that papers in stem cell research submitted from institutions in the USA are accepted faster than those submitted from elsewhere and that the cause might at least partly be some bias in the refereeing process. We investigate whether there is a similar difference in time scale for papers in astronomy, astrophysics, and cosmology and look briefly at some of the possible causes. We find a publication time lag of 3.8 days (out of a median time of 105 days) while in the stem cell case it is 24 days out of a median of 83 days. One of many possible causes is a difference in how useful the papers are to the community, and we will assess this in a second paper making use of citation analysis.

Keywords Astronomical journals · Publications · Citations

Introduction

Every scientist of the past 100 years, published or unpublished, has probably complained about some aspect of the process by which papers are selected to appear in a particular journal: its referees are slow or biased or unreasonable in what they ask. The editor shares these faults and probably has some of his own, like hostility toward some particular subdiscipline, unwillingness to consider new ideas or large compilations of data, or whatever. Worse behavior, on up to plagiarism by referees, has sometimes been suspected.

About a year ago, a group of people working on the kind of stem cell research that involves reprogramming skin or other cells to act like early embryonic ones drafted a new

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kind of complaint, saying that journals in their field, especially the high profile ones, were more likely to accept and quicker to accept papers from American authors than papers coming from other countries, even when the author was Shinya Yamanaka from Japan, who more or less invented the field. The complaint is described and a relevant set of data analyzed by Aldhous (2010a, with some statistical details in Aldhous, 2010b).

The core sample is 148 papers published since 2006 in a number of high-profile journals (including *Science*, *Nature*, *Proceedings of the US National Academy of Science*, and *Cell Stem Cell*), 94 of whose communicating authors came from the US and 54 from other countries. A few papers for which a national affiliation for the communicating author could not be established were left out of the sample.

The data definitely show that, of the published papers, 78% of the American ones, but only 54% of the others, were accepted within 100 days of submission. The fraction accepted crosses 50% at 72 and 96 days for US and non-US authors, respectively. The samples saturate (100% published) at 235 days for the American and 295 days for the other papers. This is 100% because no information was available on papers submitted but never published. The saturation time is short by astronomical standards, and we suspect that these journals must have some sort of guillotine policy.

The stem cell researchers suspect that the difference may be due to prejudice in favor of American institutions, authors, and papers (and against others) on the part of editors and/or referees. One would particularly like to know what fraction of the referees were US-based, but this information is not available.

Aldhous (2010a) notes that there are other, less reprehensible, possibilities, including that the American papers are easier to read, review, and propose improvements to (a language issue), or that the papers that are accepted most rapidly may actually report the most important work. He says that there is no way to judge whether this last is true. We think there is such a way, and Abt (2009) agrees.

Some input was provided by the editors (who generally denied bias), but no raw data, so that it is not possible to say whether the fraction of total US submissions eventually published is larger than the fraction for non-US submissions. There is some evidence that this is true within cardiology (Resnik 2010). In particular, differences could arise because Americans have shorter fuses, get annoyed, withdraw their papers, and go on other (probably less prestigious) journals faster than other nationalities. That such withdrawals occur is widely known, because from time to time a scientist asked to review a paper for journal X will write back to the editor to say that he has already rejected the paper for journal Y, and his opinion has not changed.

The astrophysical case

It was at this point that we asked (a) is there a similar discordance in elapsed times from submission to acceptance in astrophysics and (b) is there a correlation between elapsed time and the number of citations later collected by the papers, suggesting some connection with the usefulness of the results presented?

It should be confessed at this point that author VT was, through most of the 1990's, one of the associate editors (starting when there were only two) of the journal whose later papers we discuss here and has served on the publication boards of two astronomy and one physics society. These boards quite often receive complaints from unhappy authors. She has, therefore, some personal impressions of the range of behaviors exhibited by authors, referees, and other editors. She never collected statistics by nationality, gender, or anything

else, but the impressions can be summarized by saying that there are tiresome people (but also many wonderful people) of all nations, genders, generations, and types of institutions in the scientific community.

The data base

In deciding which papers to consider, we wanted (a) a large number published over a short enough period that editorial policies would not change, (b) papers from a single journal, since there are real differences in processes among the three largest journals in astronomy/astrophysics and between them and the high profile Nature and Science, (c) papers submitted from many countries, with neither the US nor Europe totally dominating the sample, and (d) papers for which it was fairly easy to determine the elapsed time from submission to acceptance, the location from which the papers were submitted, and (for possible future use) the lengths of the papers, number of authors, and subdisciplines represented (from comets to cosmology).

These requirements, particularly (c), dictated use of The Astrophysical Journal (ApJ), which (apart from Nature, Science, and Annual Reviews of Astronomy and Astrophysics) has the largest number of citation per paper in the several years after publication. In addition, Abt (1988, 2009, 2010) has presented valuable closely related data from several time frames for ApJ, which he edited for about 20 years. The Section, "[Alternative methods and additional samples](#)" discusses other journals that could be added to the sample.

Knowing that we wanted to get started immediately and would need three years of citation data to see how papers fared in the medium term meant selecting the papers published from the beginning of July 2007 to the end of June 2008. There were 2,190 of these (plus a couple for which either nationality or elapsed time could not be determined). Nationality was taken to be the first address listed by the first author, because astronomers, unlike some other disciplines, generally put the person who did most of the work and wrote the paper first. There is at least one caveat. ApJ, unlike many other journals, requires payment of page charges in excess of \$100 per page published. American astronomy research budgets take account of this, while other countries may not. Thus a first author who has been at institutions in two or more countries while the work was done is quite likely to submit from the one in the US. Elapsed time was simply the number of months and days between submission and acceptance dates given on the published paper. At no point did we attempt to establish whether there might have been multiple rewrites, multiple referees, deaths of lead authors, or other confounders.

The sample then contained 1,317 American papers and 873 others. Of these, 388 came from European countries (meaning those that sponsor the European journal Astronomy and Astrophysics (A&A), so that their scientists can publish there with no page charges, or that belong to the European Southern Observatory, or both); 255 came from other prosperous countries (Japan, Canada, Australia, Israel, New Zealand), and 230 from less prosperous countries (China, India, Taiwan, Hong Kong, Korea, Mexico, Chile, Brazil, Iran, Pakistan, Vietnam, Thailand, Indonesia, Turkey, South Africa, Russia and other parts of the former Soviet Union and Eastern Europe that have not yet declared themselves to be part of Europe; Estonia supports A&A: Ukraine does not, for instance; and yes, there are astronomers in all of these places and many others). Just what to call this last group is fraught. Not all are "less developed" or "developing." Indeed, Korea is probably not now even "less prosperous," but unlike Israel, it does not have a tradition of modern astronomy.

The fraction of US papers in ApJ has declined from 90% to about half over the past half century (Abt 2010), but with sizable year-to-year fluctuations.

It is the usual practice at ApJ to seek only a single reviewer, who generally, but not always, chooses to remain anonymous, at least until after convergence has occurred and the paper has been accepted. A request for a second, independent reviewer, when the author feels that the first one is hopeless, is nearly always honored. Authors are entitled to provide short lists of people they feel would not be fair, but not to say that only some very small group of individuals would be appropriate reviewers. The underlying idea is that if there are only three people who can understand the paper, it is unlikely to be sufficient interest to justify publication, and yes this policy would have resulted in rejection of Einstein's first paper on general relativity.

An occasional paper is never sent out at all for refereeing, because the scientific editor handling it (or the editor in chief) decides that it is so far from mainstream astronomy/astrophysics that readers would resent their subscription dollars paying for it. (Page charges cover roughly half the journal and subscriptions the other half.) Complaints to the publication board or the officers of the American Astronomical Society (which owns and operates ApJ) are an occasional result of a paper not having been sent to a reviewer.

Another 15% or so of submissions do not finally get published, at least in ApJ, either after out-right rejection or because the authors withdraw it or just give up and go away. Abt (1988, 2009) has provided details of what happened to that stray 15% for smaller sets of papers submitted before and after the ones examined here. But the general assumption is that a submitted paper will eventually be published somewhere, and the referee's main job is to help make the paper as good as possible. Not all scientific journals operate on this principle.

The results

The shortest and longest elapsed times were less than a week and more than 5 years (both US papers, but it is the largest subsample and so should have the most extreme values, even if all the populations were really the same). There is no way to bin the numbers to look like Gaussian or Poisson distributions (See Fig. 1), perhaps because many factors affect the submission to publication time (see Sect. "Possible causes of bias").

So, what did we find? As Fig. 2 shows, the American papers are indeed accepted slightly faster than the others, though the plotted points overlap or cross over at a couple of time intervals. The units are percentage of papers eventually published that have been accepted up to a given elapsed time versus the logarithm (base 10) of elapsed time in months. Use of linear rather than logarithmic time bins would have stretched the right side – Aldhous (2010a) used linear bins, but he needed to show no times larger than 300 days, while our sample extends to more than 5 years. Figure 3 is a blow-up of the nearly straight part of the middle of the S-curve, now versus linear time in months, which shows that the US papers cross the 50% acceptance level about 3.8 days faster than the others, where the average is about 103 days. The difference for the stem cell papers was 24 out of a median elapsed time of 83 days. The astrophysics lag would have been zero or even slightly negative without papers from July (which included a clump of very slow papers from less developed countries) and from the last two December issues, which included a large number of rapidly-accepted US papers, where the authors had hurried their revisions to get in before the hand-over of publication from the University of Chicago Press to the Institute of Physics in the UK.

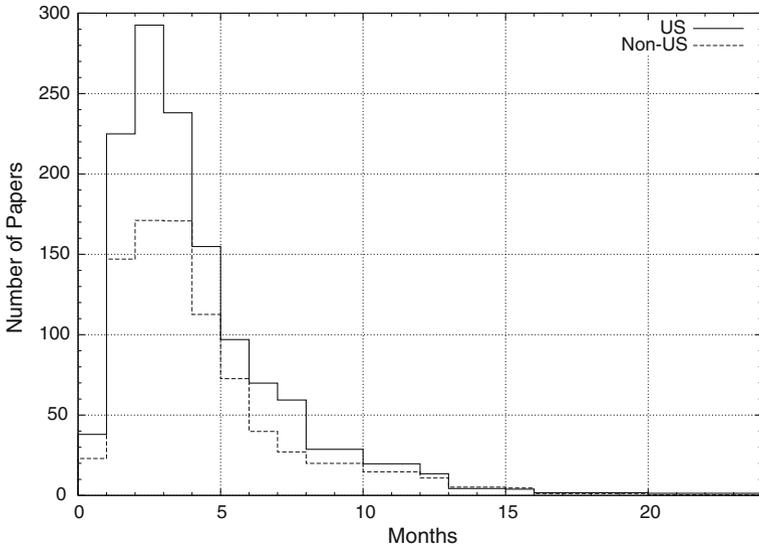


Fig. 1 Paper distribution. Bins correspond to the number of months from submission to publication; for instance, bin 3 includes papers that took less than 3 months to be published but more than two

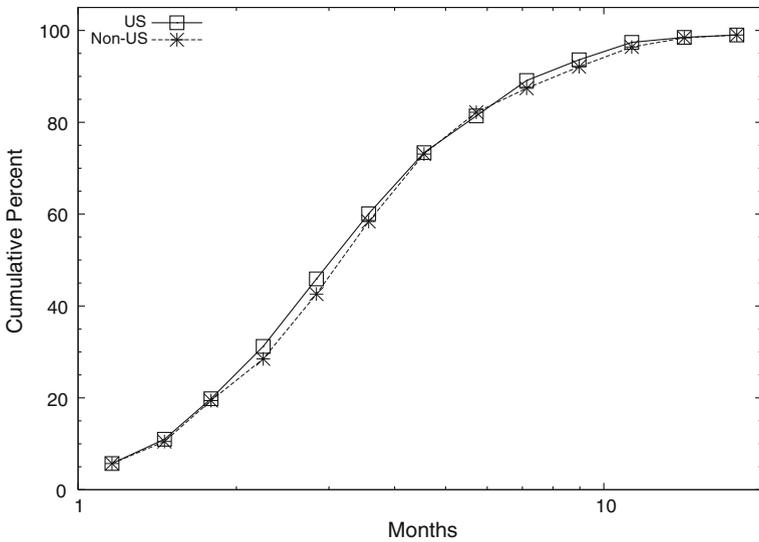


Fig. 2 Cumulative percentage of papers (ApJ, July 2007–June 2008) accepted as a function of time elapsed since submission. The 3% of papers accepted in less than 1 month and the 1.6% of papers that took more than 15 months have been clipped off from the edges (the full horizontal axis would extend from 0.20 to 63.10 months). The curve shape, like that of Aldhous (2010a), who used a linear time scale, is the S-shape you might expect

Is 3.8 days enough to affect whether you win a Nobel Prize? Somehow we doubt it. Indeed we doubt whether 3.8 days is enough for authors to feel discriminated against, though three weeks apparently is.

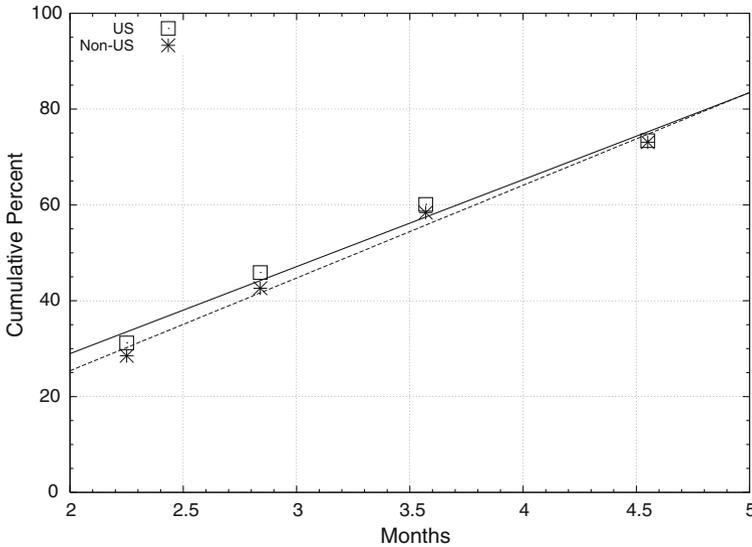


Fig. 3 Central portion of Fig. 2 above, expanded to show that percentage accepted crosses 50% about 3.8 days (slightly more than a tenth of a month) earlier for the US papers than for the others. The difference is 24 days (out of a smaller average time) for the stem cell papers

Possible causes of bias

Why is one paper accepted sooner than another? The single most important cause is frequently how prompt authors are in revising. Editors who spoke with Aldhous (2010a) said that the longest interval going into elapsed time was that between sending a report to authors and receiving a revised paper. This is also generally true for ApJ papers, which in 2008 spent a median of 44 days with referees and 55 days with authors (Abt 2009). Of course if the referee has insisted on additional work being done or extensive rewriting, this may not be all the author's fault! Conceivably authors who are part of large research groups at prestigious institutions in wealthy countries will find the revision process easier and quicker than will other sorts of authors.

The nearly-universal switch to all-on-line submission and refereeing has substantially speeded up most parts of the publication process. The median of 12 days that ApJ papers spend in editors' hands (Abt 2009) is taken up primarily in trying to find a willing referee.

Other causes of rapid acceptance include (a) well-written papers, which is partly, but only partly, a matter of authors' fluency in English, (b) short papers, which perhaps present a single, clear idea, (c) papers whose conclusions do not offend other workers on the topic, (d) conscientious referees who report promptly (or admit at once that they will not) and who do not ask for unreasonable amounts of additional work (for instance, a referee of this paper who might require that we add other large journals to the sample), and (e) editors who go on promptly to a second reviewer if the first one is dilatory or (at least in the eyes of the author) unreasonable. VT's record in her ApJ days was five people asked before one finally reported, and this took many months. None of this is necessarily nefarious.

Potentially reprehensible causes of slow acceptance include deliberate delays by an editor in assigning a paper to a reviewer or by a reviewer in reporting, perhaps because of biases related to institution, age, gender, or nationality of the authors, or perhaps because of

a desire to disadvantage the author relative to someone else working in the field (quite possibly the reviewer!). Plagiarism exerted in favor of a paper the reviewer is working on is the extreme limit. Aldhous (2010a) and Resnik (2010) point out that there is some evidence for all these somewhere in some community.

Journals differ in how long they are prepared to keep a paper in the “active” file (Nature and Science being particularly ruthless), and authors differ in how long they are prepared to wait for final acceptance. The former should not be biased by nationality or anything else; the latter might well be. And, of course, the assumption by an editor or referee that a paper from Professor Prizewinner or from the University of Outstandingness is bound to be correct and important would be a nefarious cause of rapid acceptance.

And, to reiterate from the introduction, rapid acceptance of papers of great value to the community would be a good thing, and the Section, “[Work ahead: Is elapsed time correlated with impact?](#)” indicates how it might be tested at some primitive level.

Alternative methods and additional samples

This sort of analysis should be carried out using data available only within the editorial offices, which would include lengths of time from submission to withdrawal or rejection for papers that are not published at all, and, of course, the percentage of submissions eventually published, which could be correlated with nationality of authors, prestige of institutions, gender, subdiscipline, and so forth. Not many editors would be likely to be happy about providing this information (though Abt [1988, 2009] has done so for two samples of ApJ papers, one during his term as editor and one following it). VT regrets not having kept enough of the paperwork from the 2000 or so ApJ papers for which she was associate editor to have anything to say on the topic, even long ago. Will these numbers depend on where the authors come from? Almost certainly, and Resnik (2010) reports one case of abstracts for cardiology meetings where a trial of double blinding suggested that there was real bias in favor of authors from the US and from prestigious institutions.

The executive officer of the American Astronomical Society (K. Marvel 2010, personal communication) suggested that we use papers published in calendar year 2009, after the transition from University of Chicago Press to Institute of Physics (UK) had been completed. This is, of course, perfectly possible (and you are invited to try it). But the three years of citation data that we think are necessary to evaluate the utility of everyday papers would then not be available until early 2013 (Trimble and Ceja 2008). In addition, we could see no particular reason to think the change of publishers would have any effect on elapsed times from submission to acceptance of papers, though of course it might well affect the time from acceptance to publication, for which the stem cell folks also suspected a pro-US bias.

There are two other large journals that publish papers across the full range of astronomy and astrophysics from all over the world. A&A, head-quartered in Europe, often nearly ties ApJ in number of papers per year. A very large fraction come from the European countries that sponsor the journal and whose authors are, therefore, assessed no page charges. Some come from authors in less developed countries, etc. (to whom the journal has said informally it wants to be hospitable), and very few from the US (5.4% in 2009, Abt 2010). Monthly Notices of the Royal Astronomical Society (MNRAS), headquartered in the UK, imposes no page charges on anyone. A majority of its papers come from Europe (especially the UK) some from (former) commonwealth nations and less developed countries, and again rather few from the US (8.4% in 2009, Abt 2010).

It would obviously be interesting to investigate correlations of source country, acceptance time, and citation rates for these journals. Their refereeing patterns are, however, somewhat different from those of ApJ (though most elapsed times fall between a month and a year for all three) so the samples should initially be kept separate. Again this can be done by anyone who is sufficiently interested. Some details of the appearance of the journals would make assembling the data a little more difficult, especially for an investigator interested in correlations with subdiscipline for MNRAS, which does not order papers by topic in its issues.

We have a prediction to offer for what might be shown for papers published in the three journals, which one might call a “home court advantage”. A&A referees are generally European, and MNRAS referees quite often from the UK, and if the 3.8 day lag we found or the 24 day one Aldhous (2010a) reports arises from a large fraction of the referees being Americans, then one might reasonably expect A&A to accept Europeans papers fastest and MNRAS to accept UK papers fastest.

Work ahead: Is elapsed time correlated with impact?

Aldhous (2010a) made the curious remark that there was no way to judge whether the more rapidly accepted papers report better work. Well, OK, there is no way but the verdict of history 50 years into the future to decide whether those papers systematically represented paradigm-changing results, and the same goes for astrophysics. Can we say anything from the past? Not here. Fifty years ago ApJ papers carried the submission (but not the acceptance) date, and 70 years ago not even the submission date. Thus such an investigation would require access to very old editorial office records which may not actually exist. Stem cell research, of course, did not exist then. In addition, the investigator would need to develop a comparison sample of so-so papers, because customs in paper selection for publication have changed enormously through the years. Einstein once told the then-editor of *Physical Review* that he had sent a paper for publication, nor for criticism, and withdrew it (Kenefick 2005; Blume 2006). The referee was right, Einstein wrong, and the version published elsewhere incorporated the corrections requested, but that is part of another story.

The ApJ records from the editorships of W.W. Morgan and S. Chandrasekhar are in the Regenstein Library of the University of Chicago and those from the Abt era in storage lockers in Tucson. All can, in principle, be consulted for statistical purposes with permission of the current editor (Abt 2011, personal communication).

In the shorter term, however, it has become customary to estimate the utility of a paper to its community from the frequency with which it is cited over some period following publications. Different periods of citation collection will probe different sorts of utility, and there are cases where again 50 years are needed. These have been called sleeping beauty papers, and they definitely happen in astronomy, for instance Refsdal (1964) on gravitational lensing and Gunn and Peterson (1965) on absorption of light by intergalactic matter (which had a very long elapsed time to acceptance for its journal). Then there are blockbusters that accumulate hundreds of citations in a few months, commoner in stem cell research than in astrophysics but not entirely unknown to us, for instance the official Hubble Space Telescope value of the Hubble constant (Freedman et al. 2001) and the WMAP numbers for other cosmological parameters (Bennett et al. 2003).

For average papers (like ours) by average authors (like us), however, 3 years seems to be both necessary and sufficient (Trimble and Ceja 2008). Very few papers are still

completely uncited in that time, and inequalities between rapidly and slowly developing subdisciplines begin to even out. Thus in the summer of 2010, we began gathering numbers of citations for the July 2007 ApJ papers for intervals of three years after publication from the *Web of Science*. The process cannot be complete before mid to late summer 2011, at which time we will look for correlations of citations per paper with elapsed time to acceptance. Other known or expected correlations (with subdiscipline, length of paper, number of authors, institutions, and nationalities) are likely to dominate. This is perhaps also a prediction, though one that is supported by the first third of the data (and indirectly by other samples, Abt 2009, Trimble and Ceja 2008).

Acknowledgments We are grateful to the editors of *New Scientist* for providing more precise numbers for the stem cell analysis than could be gleaned from the graphs in Aldhous (2010a, b) and deeply indebted to our referee, Helmut A. Abt, for his kind and valuable advice and for his commendable promptness, in the form of a report that arrived in about 10% of the time usually taken by reviewers. We cannot quite match his record.

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