Revised: Are studies that replicate cited more?

Looking at the RPP to bring data to a discussion

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Replication in the Reproducibility Project Psychology and citations

Figure 1: Does it replicate? From the Internet Archive Book Images

After his talk at the Center for Adaptive Rationality, Stephan Lewandowsky and I had a small discussion whether scientists can actually pick "winners". The discussion stemmed from a larger discussion about whether we get more research waste, if we replicate first, then publish, or publish first, and then replicate those studies that are found interesting.

If I recall correctly, we didn't really disagree that scientists *can* tell if things are off about a study, but we did disagree on whether *citation* indexes such a quality assessment, and is a useful way to find out which studies are worthy of more attention.

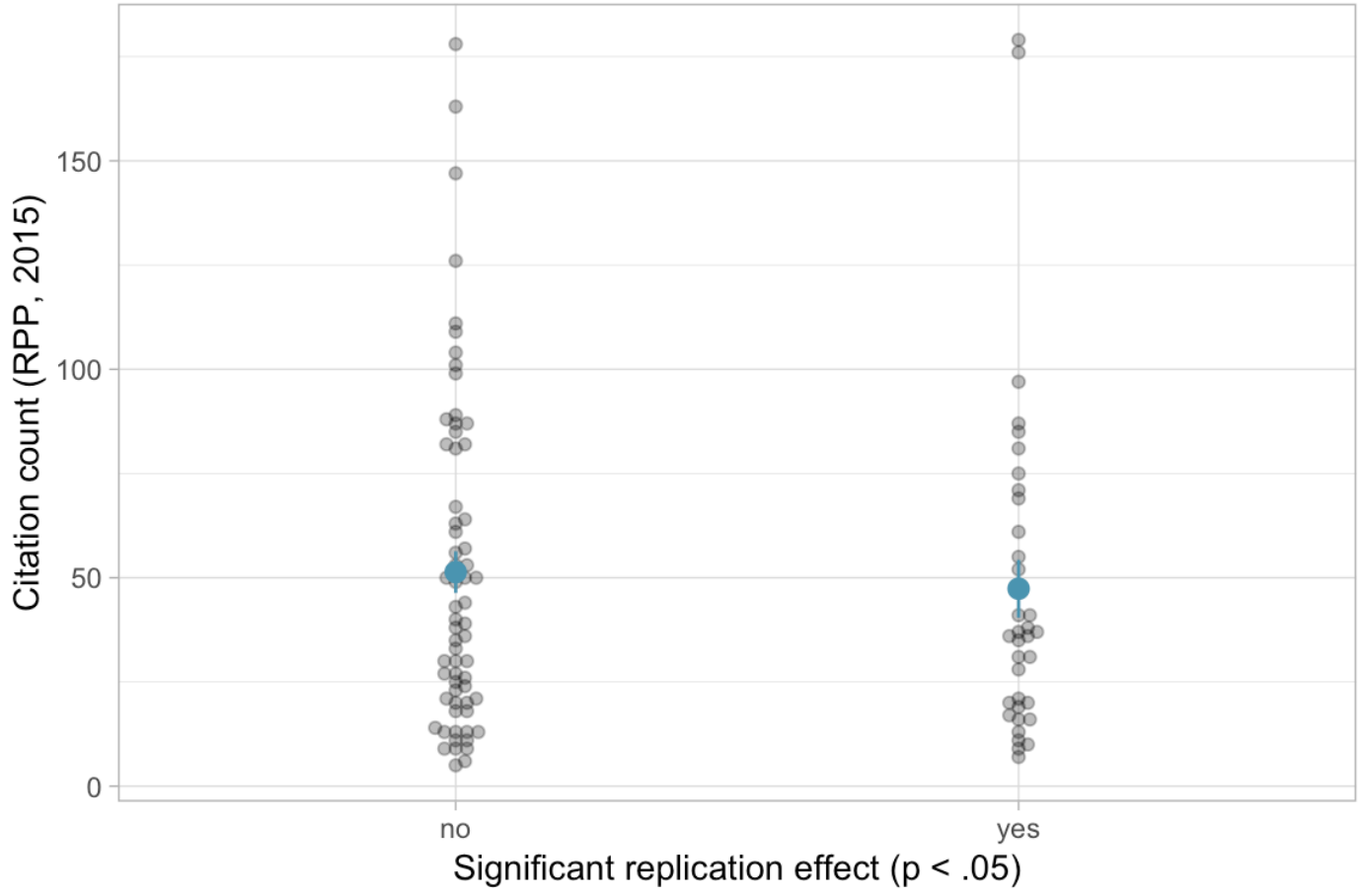
So, I ran the numbers for one of the few studies where we can find out, the Reproducibility Project: Psychology. I tweeted it back then, but felt like making the graphs nicer and playing with radix on a train ride.

We found 167 DOIs, so we had DOIs for all our studies [1] .

	scopus_pre2015	scopus_2018	scopus_post2015	gscholar_pre2015	crossref_2018	mixed_post2015
scopus_pre2015	1.00	0.98	0.93	0.97	0.97	-0.05
scopus_2018	0.98	1.00	0.98	0.97	0.98	0.04
scopus_post2015	0.93	0.98	1.00	0.93	0.97	0.13
gscholar_pre2015	0.97	0.97	0.93	1.00	0.97	-0.15
crossref_2018	0.97	0.98	0.97	0.97	1.00	0.10
mixed_post2015	-0.05	0.04	0.13	-0.15	0.10	1.00

Does replication in the RPP predict how often a paper is cited?

No, not for the citation count recorded in the RPP.




```
Call:
glm(formula = citations_2015 ~ replicated_p_lt_05, family = quasipoisson(),
    data = .)

Deviance Residuals:
   Min      1Q  Median      3Q     Max
-8.331  -4.810  -1.726   3.067  14.583

Coefficients:
                    Estimate Std. Error t value Pr(>|t|)
(Intercept)          3.93854    0.09962  39.534   <2e-16 ***
replicated_p_lt_05yes -0.08052    0.17203  -0.468    0.641
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

```
(Dispersion parameter for quasipoisson family taken to be 32.61327)

    Null deviance: 2813.6  on 98  degrees of freedom
Residual deviance: 2806.4  on 97  degrees of freedom
AIC: NA

Number of Fisher Scoring iterations: 5
```

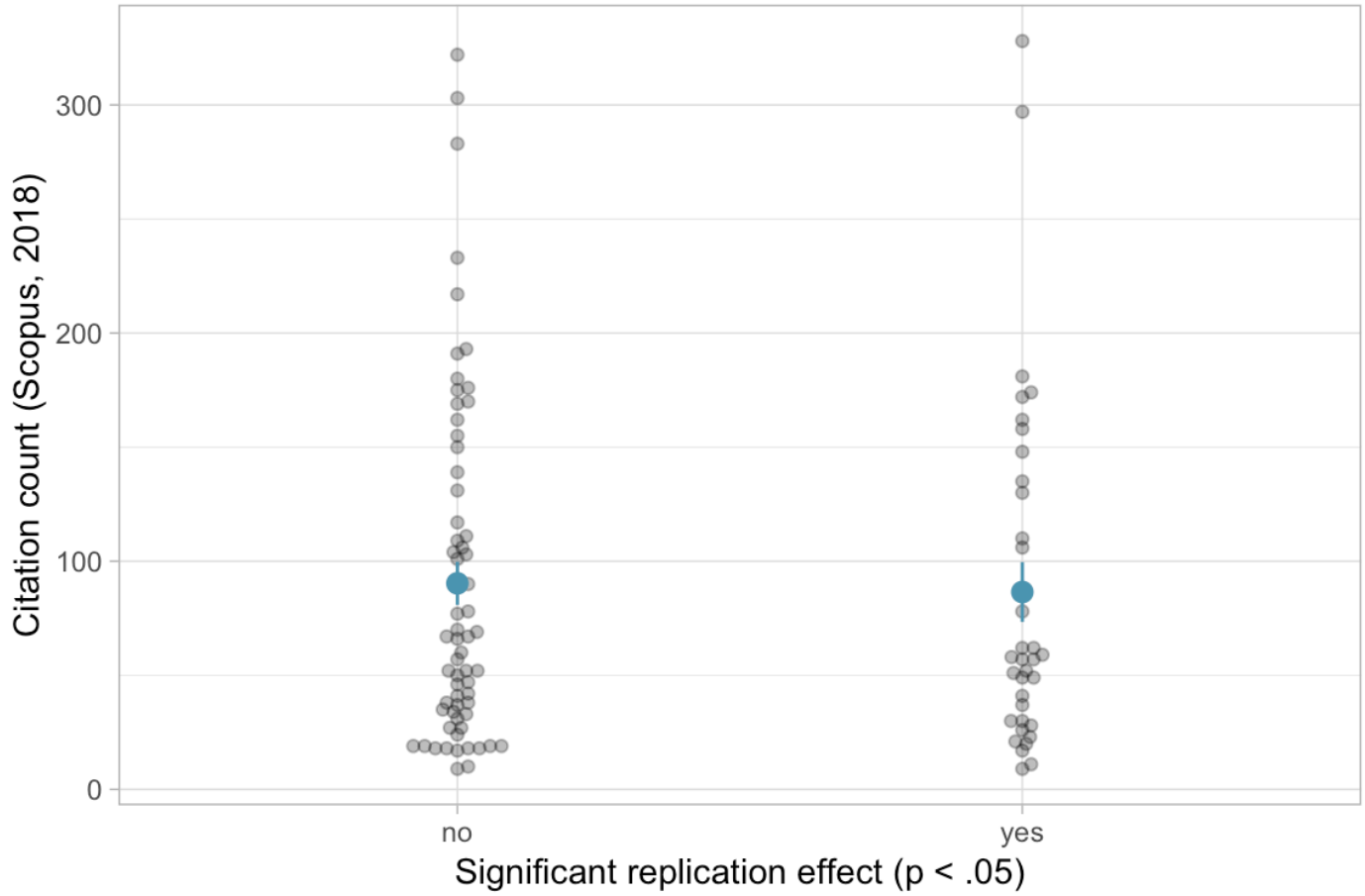
Does replication predict 2018 citation counts?

Details: I got DOIs, which were missing from the RPP data, by searching Crossref on titles, authors, and dates. I did some checking to see if matches were proper. Next, I got citation counts from Scopus and validated those against those in the RPP and Crossref. Find the improved dataset with DOIs below.

I used the Crossref API to get DOIs and the Scopus API to get yearly citation counts for the papers contained in the RPP.

Edit: The SCOPUS citation count up to 2015 was highly correlated with the one stored in the dataset (based on Google Scholar). Rank order were also very similar for citations pre and post 2015 using Scopus, CrossRef, or Google Scholar. However, subtracting CrossRef citation counts from Google Scholar counts amplified error (to get citations after the publication of the RPP) - the correlation with the "citations after 2015 (Scopus)" variable was low. Therefore, the revised version of this blog post uses only the Scopus numbers.

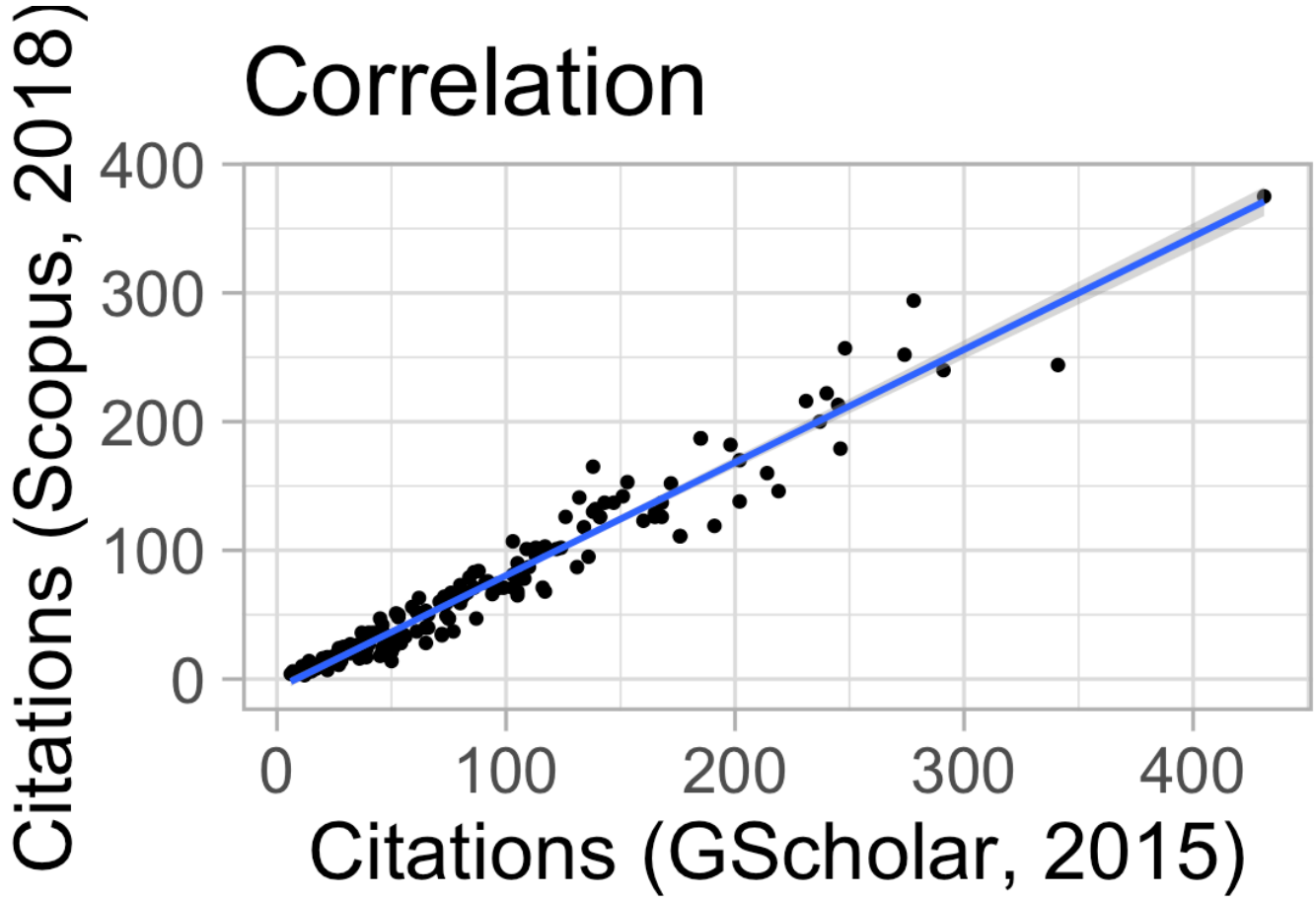
Again, there was no association with replication status for 2018 citation counts.




```
Call:
glm(formula = citations_2018 ~ replicated_p_lt_05, family = quasipoisson(),
    data = .)

Deviance Residuals:
    Min       1Q   Median       3Q      Max
-11.005   -6.988   -3.263    4.546   19.781

Coefficients:
                     Estimate Std. Error t value Pr(>|t|)
(Intercept)           4.50328    0.10635  42.344   <2e-16 ***
replicated_p_lt_05yes -0.04297    0.18138  -0.237    0.813
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

(Dispersion parameter for quasipoisson family taken to be 65.37293)

    Null deviance: 5660.0  on 98  degrees of freedom
Residual deviance: 5656.3  on 97  degrees of freedom
AIC: NA

Number of Fisher Scoring iterations: 5
```

Does replication predict subsequent citation counts (ie. 2015-2018)?

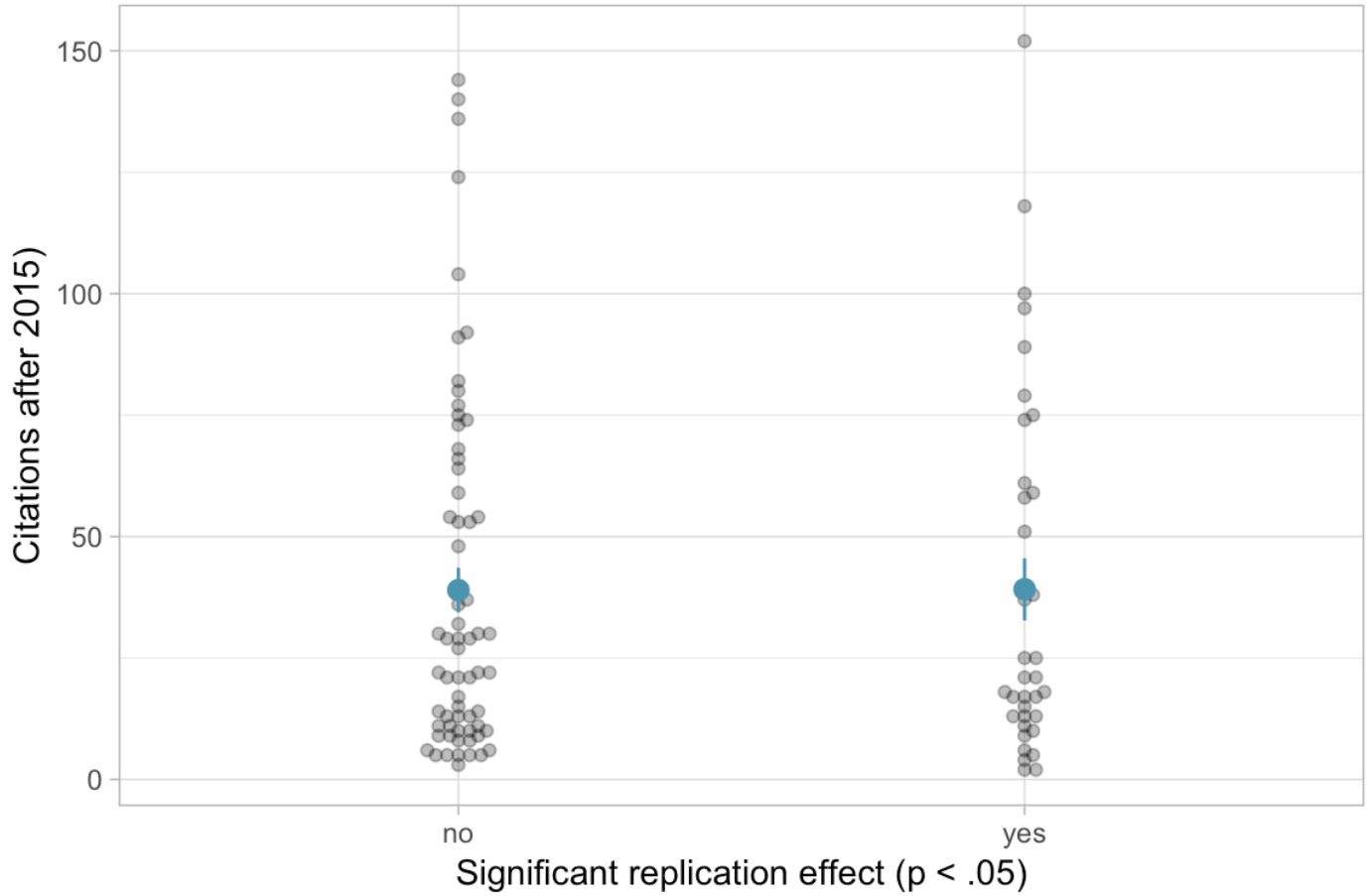
The correlation between 2018 Scopus and 2015 Google Scholar counts is 0.97, but the means differ (2018 Scopus = 72, Scholar 2015 = 90). Can citations go *down*? No, but Google Scholar includes more sources than Scopus, leading to the mean being higher. Still, these sources don't seem to be systematically different, leading to the maintained rank order.



~~This is pretty dirty work, because I'm subtracting citation counts from one source with another, so most papers are cited less in 2018 than in 2015. But haven't found a quick way to get citation counts in 2015 from `rcrossref`. I've requested the necessary access to Scopus, where I could check, but Elsevier is being annoying.~~

~~Again, no association. So, assuming the dirtiness of the analysis doesn't matter,~~

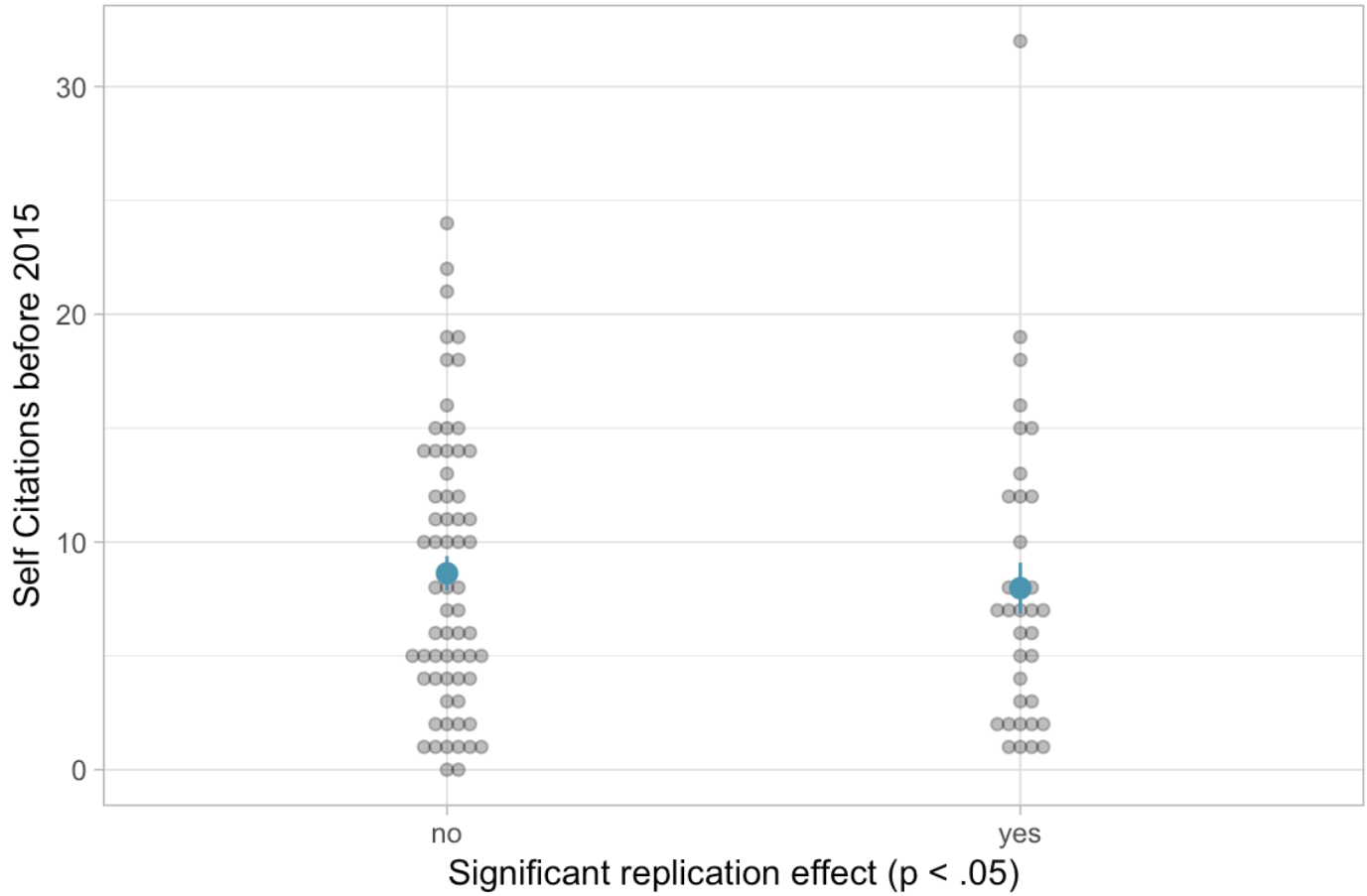
The literature hasn't reacted at all to the presumably important bit of information that a study doesn't replicate.



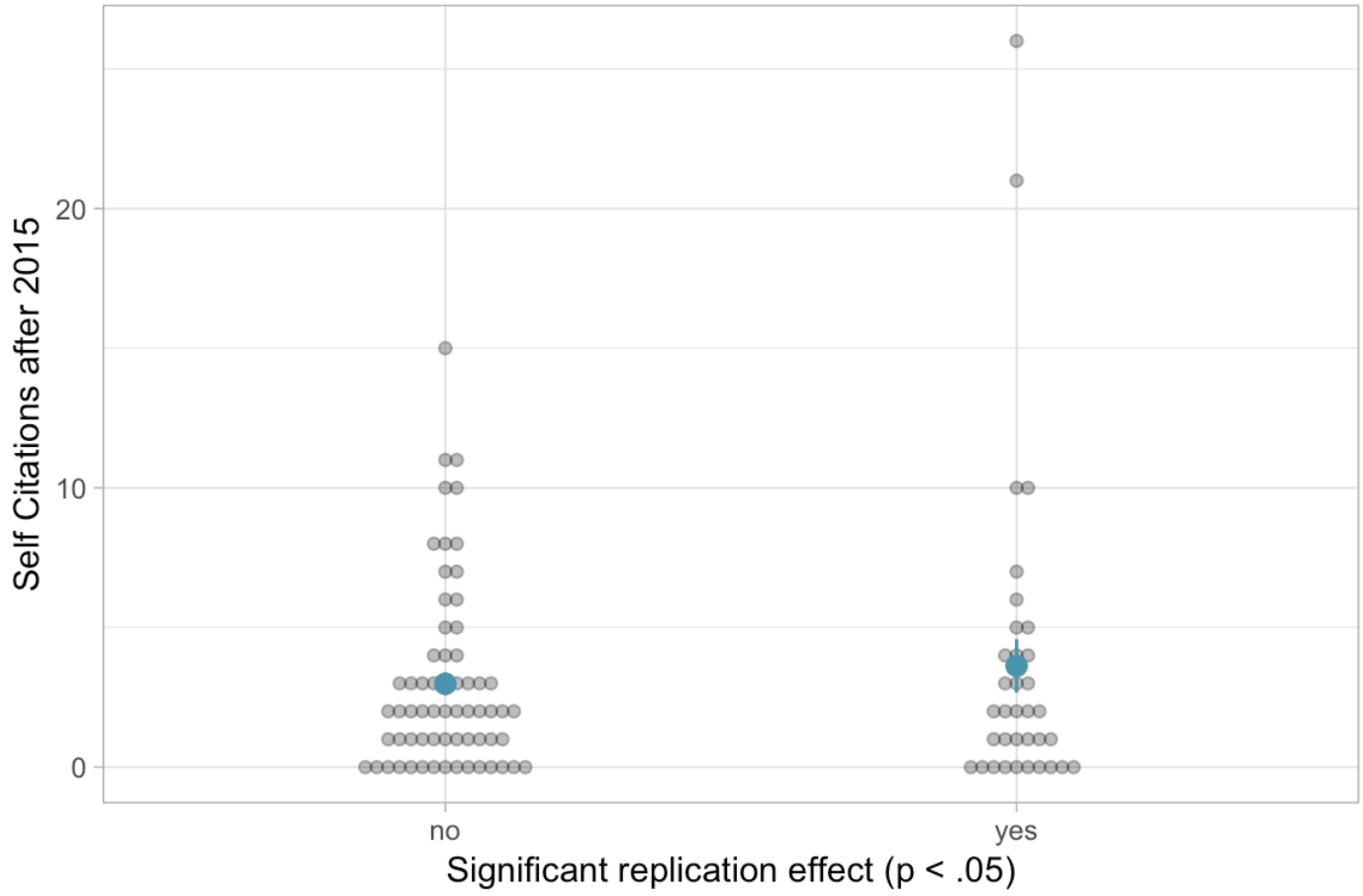

```
Call:
glm(formula = citations_after_2015 ~ replicated_p_lt_05, family = quasipoisson(),
    data = .)

Deviance Residuals:
   Min      1Q  Median      3Q     Max
-7.899  -5.302  -2.963   3.103  13.664

Coefficients:
                    Estimate Std. Error t value Pr(>|t|)
(Intercept)          3.662760   0.119244  30.717   <2e-16 ***
replicated_p_lt_05yes 0.004458   0.200260   0.022    0.982
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

(Dispersion parameter for quasipoisson family taken to be 35.46258)

    Null deviance: 3101.3  on 98  degrees of freedom
Residual deviance: 3101.3  on 97  degrees of freedom
AIC: NA

Number of Fisher Scoring iterations: 5
```

What about self citations?

The RPP emphasised its own overall result. Hence, some nonreplications of specific studies may have gone unnoticed by researchers in the field. But the study authors hardly have this excuse; they knew whether their study was replicated (probably even prior to 2015, but this is hard to figure out). However, there is also no significant difference in self citation count (before or after 2015) by publication status.



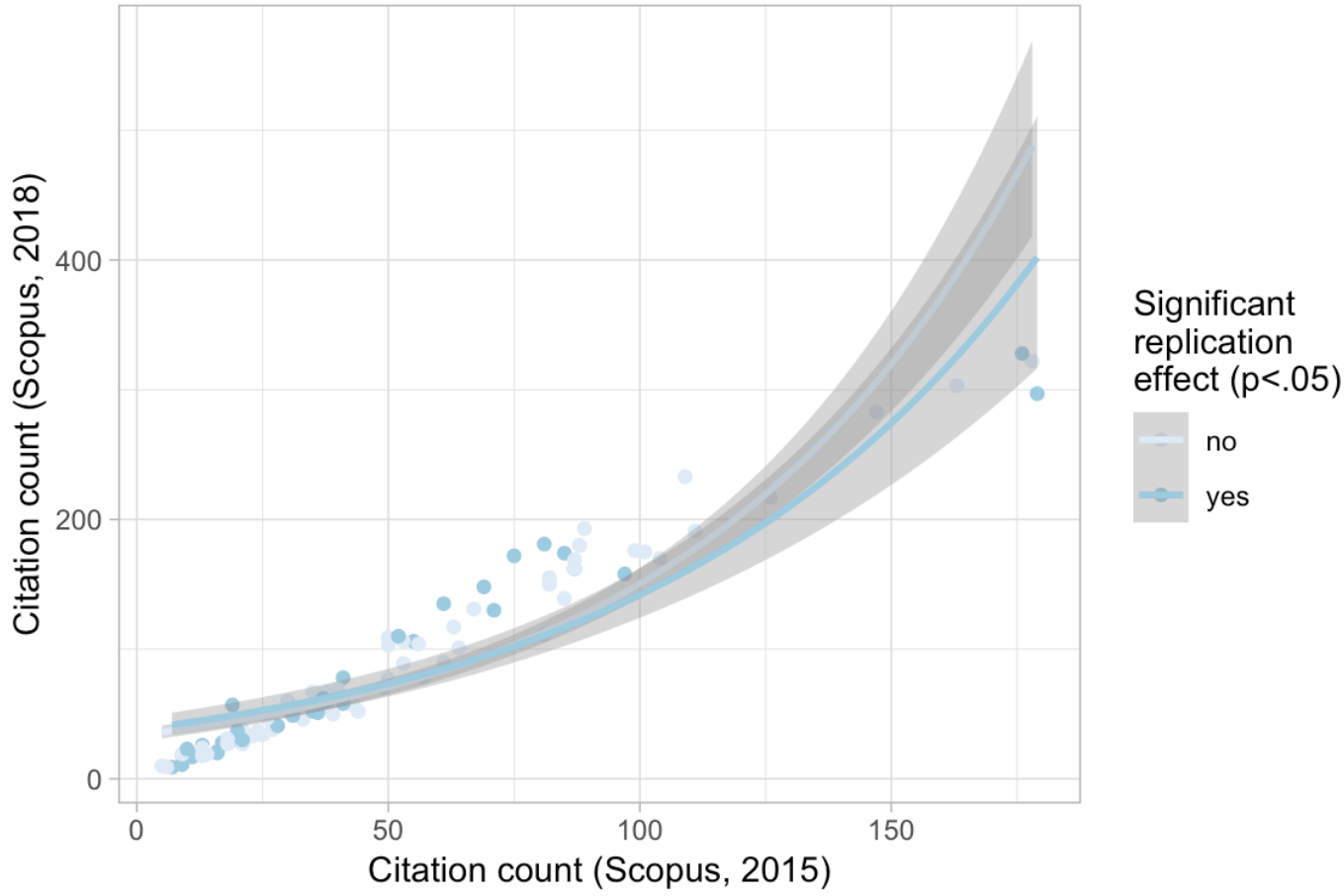

```
Call:
glm(formula = self_cites_before_2015 ~ replicated_p_lt_05, family = quasipoisson(),
    data = .)

Deviance Residuals:
    Min       1Q   Median       3Q      Max
-4.1533  -1.8906  -0.3514   1.3266   6.3949

Coefficients:
                    Estimate Std. Error t value Pr(>|t|)
(Intercept)          2.15466    0.09269  23.246   <2e-16 ***
replicated_p_lt_05yes -0.07880    0.15997  -0.493    0.623
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

(Dispersion parameter for quasipoisson family taken to be 4.742497)

    Null deviance: 463.83  on 98  degrees of freedom
Residual deviance: 462.67  on 97  degrees of freedom
AIC: NA

Number of Fisher Scoring iterations: 5
```

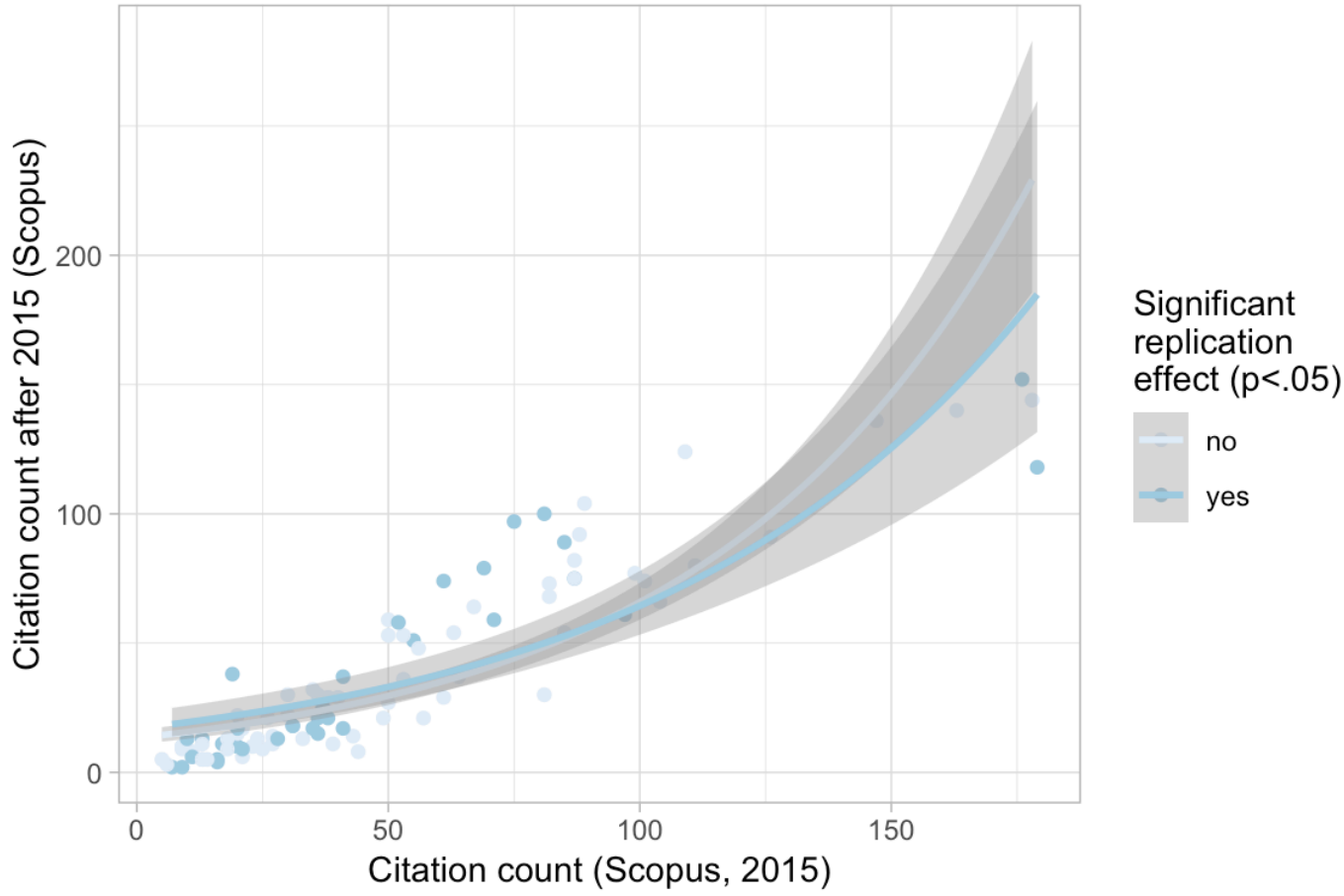



```
Call:
glm(formula = self_cites_after_2015 ~ replicated_p_lt_05, family = quasipoisson(),
    data = .)

Deviance Residuals:
    Min       1Q   Median       3Q      Max
-2.6939  -2.0400  -0.6064   0.3752   7.5933

Coefficients:
                   Estimate Std. Error t value Pr(>|t|)
(Intercept)          1.0934     0.1699   6.437 4.65e-09 ***
replicated_p_lt_05yes   0.1954     0.2688   0.727    0.469
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

(Dispersion parameter for quasipoisson family taken to be 5.510687)

    Null deviance: 429.28  on 98  degrees of freedom
Residual deviance: 426.40  on 97  degrees of freedom
AIC: NA

Number of Fisher Scoring iterations: 6
```

How does pre-2015 citation count predict post-2015 citations accounting for replication status?

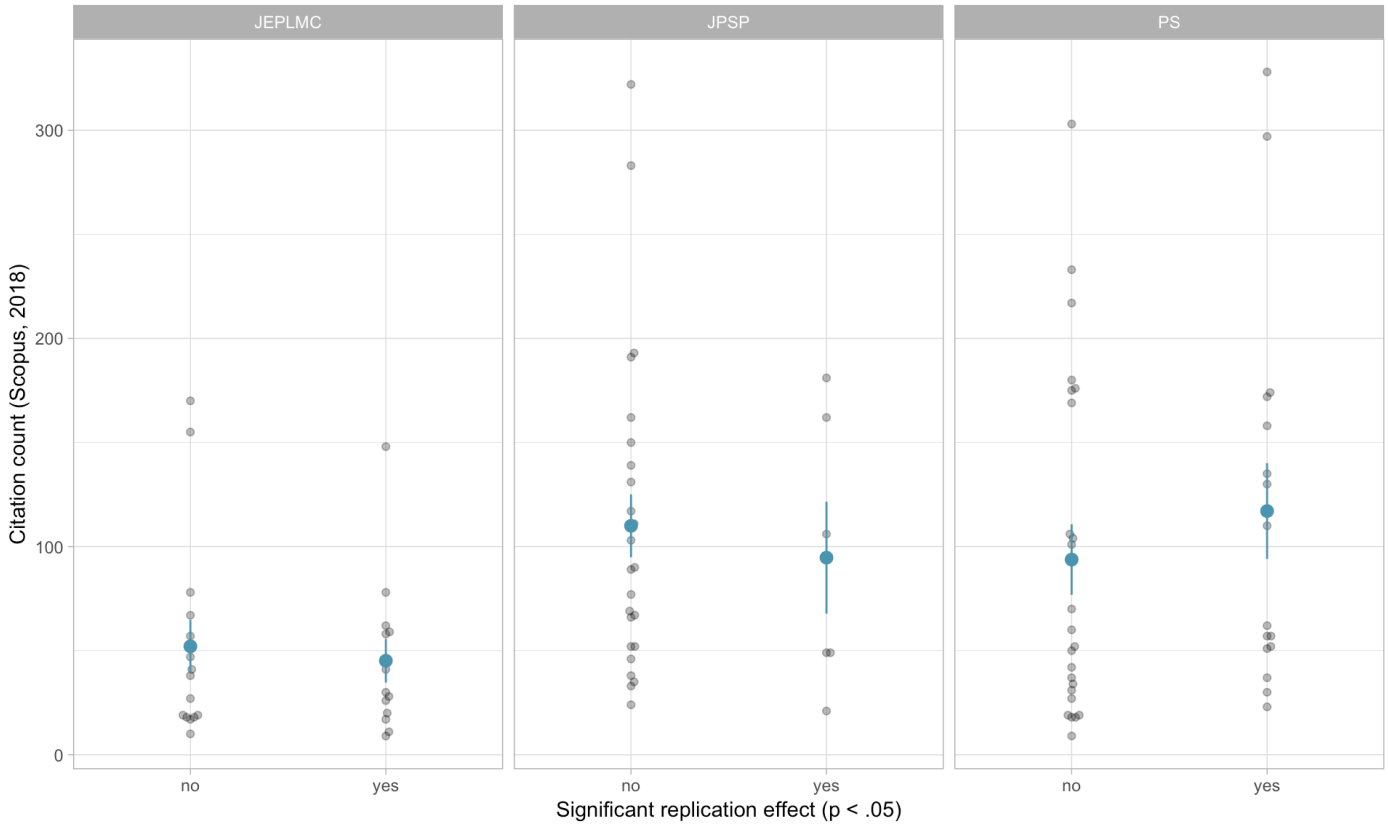
A slightly different way of looking at it does not yield different conclusions for me.





Does the association differ by journal?

Hard to tell with this little data!




```
Call:
glm(formula = citations_2015 ~ Journal * replicated_p_lt_05,
    family = quasipoisson(), data = .)

Deviance Residuals:
   Min      1Q  Median      3Q     Max
-8.331  -4.332  -1.445   2.532  12.122

Coefficients:
                                Estimate Std. Error t value
(Intercept)                      3.47197    0.24258  14.313
JournalJPSP                      0.64544    0.27814   2.321
JournalPS                        0.49518    0.28506   1.737
replicated_p_lt_05yes           -0.18759    0.37517  -0.500
JournalJPSP:replicated_p_lt_05yes -0.01447    0.50369  -0.029
JournalPS:replicated_p_lt_05yes    0.36557    0.43739   0.836
                                Pr(>|t|)
(Intercept)                       <2e-16 ***
JournalJPSP                       0.0225 *
JournalPS                         0.0857 .
replicated_p_lt_05yes             0.6182
JournalJPSP:replicated_p_lt_05yes   0.9771
JournalPS:replicated_p_lt_05yes     0.4054
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

(Dispersion parameter for quasipoisson family taken to be 28.42273)

    Null deviance: 2813.6  on 98  degrees of freedom
Residual deviance: 2419.1  on 93  degrees of freedom
AIC: NA

Number of Fisher Scoring iterations: 5
```

Conclusion

So, are citation counts a poor indicator of quality? The most common reaction I received to these results was saying that the 7 years from the publication of the studies to 2015 are probably not enough for citation counts to become more signal than noise, or at least that the 3 years from the publication of the RPP results to 2018 are not enough. These reactions mostly came from people who did not really believe in citations-as-merit before anyway.

To me, if 10 years after publication citations cannot be used to distinguish between studies that replicated and those that didn't, they're probably not a useful measure of thoroughness that can be used in assessment, hiring, and so on. They may be a useful measure of other important skills for a scientist, such as communicating their work; they may measure qualities we don't want in scientists, but it seems they are not useful to select people whose work will replicate. I think that is something we should want to do.

In addition, the literature does not react quickly to the fact that studies do not replicate. Given that people also keep citing retracted studies (albeit with a sharp drop), this does not surprise me. It will be interesting to revisit the data in a few years time and see if researchers picked up on replication status then.

Limitations

These were all studies from reputable journals, so we might have some range restriction here. On the other hand, plenty of these studies don't replicate, and citation counts go from 0 to >300.

Which studies keep being cited after not being replicated?

Hover your mouse over the dots to see the study titles.

Which authors keep citing their own studies after they do not replicate?

Hover your mouse over the dots to see the study titles.

List of studies

Copy CSV Excel Search:

Authors	Title	Journal	DOI	replicated_p_lt_05	citations_2015	citations_2018	self_cites_befo
DR Addis, AT Wong, DL Schacter	Age-related changes in the episodic simulation of future events	PS	10.1111/j.1467-9280.2008.02043.x	yes	176	328	
KE Stanovich, RF West	On the relative independence of thinking biases and cognitive ability.	JPSP	10.1037/0022-3514.94.4.672 1 comment on PubPeer (by: Statcheck)	no	178	322	
KD Vohs, JW Schooler	The value of believing in free will: Encouraging a belief in determinism increases cheating.	PS	10.1111/j.1467-9280.2008.02045.x 1 comment on PubPeer (by: Statcheck)	no	163	303	
G Tabibnia, AB Satpute, MD Lieberman	The sunny side of fairness: Preference for fairness activates reward circuitry (and disregarding unfairness activates self-control circuitry)	PS	10.1111/j.1467-9280.2008.02091.x	yes	179	297	
V Purdie-Vaughns, CM Steele, PG Davies, R Ditlmann, JR Crosby	Social identity contingencies: How diversity cues signal threat or safety for African Americans in mainstream institutions.	JPSP	10.1037/0022-3514.94.4.615 1 comment on PubPeer (by: Statcheck)	no	147	283	
N Epley, S Akalis, A Waytz, JT Cacioppo	Creating social connection through inferential reproduction: Loneliness and perceived agency in gadgets, gods, and greyhounds.	PS	10.1111/j.1467-9280.2008.02056.x 1 comment on PubPeer (by: Statcheck)	no	109	233	
S Schnall, J Benton,	With a clean conscience:	PS	10.1111/j.1467-9280.2008.02227.x	no	126	217	

Authors	Title	Journal	DOI	replicated_p_lt_05	citations_2015	citations_2018	self_cites_befo
S Harvey	Cleanliness reduces the severity of moral judgments.						
N Shnabel, A Nadler	A needs-based model of reconciliation: Satisfying the differential emotional needs of victim and perpetrator as a key to promoting reconciliation.	JPSP	10.1037/0022-3514.94.1.116 1 comment on PubPeer (by: Statcheck)	no	89	193	
PW Eastwick, EJ Finkel	Sex differences in mate preferences revisited: Do people know what they initially desire in a romantic partner?	JPSP	10.1037/0022-3514.94.2.245	no	111	191	
CJ Soto, OP John, SD Gosling, J Potter	The developmental psychometrics of big five self-reports: Acquiescence, factor structure, coherence, and differentiation from ages 10 to 20.	JPSP	10.1037/0022-3514.94.4.718 1 comment on PubPeer (by: Statcheck)	yes	81	181	

Showing 1 to 10 of 99 entries

Previous 1 2 3 4 5 ... 10 Next

Appendix

These analyses are based on Chris J. Hartgerink's script. The data and his script can be found on the OSF. Did I get the right DOIs? There are probably still some mismatches. Titles are not exactly equal for 84 studies, but on manual inspection this is only because Crossref separates out the subtitle, and 150 of 167 titles start exactly the same.

Footnotes

1. Were they they all correct? See Appendix [↩]