The replicability crisis; The way forward

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- Replication Crisis [2015; OSC]
- Theory Crisis [2019; Oberauer & Lewandowsky]
- Validity Crisis [2019; Schimmack]
- Measurement Crisis [2020; Flake & Fried]
- Generalizability Crisis [2021; Yarkoni]
- Practicality Crisis [2021; Berkman & Wilson]

We are very good at pointing out what is going wrong, but not very good at fixing it.

Fixes will make us feel uncomfortable, and scientists will need to give up some freedom.

How much time do you need to do research that is **replicable**, builds strong **theories**, uses **valid measures**, is **generalizable**, and can be applied in **practice**?

How many researchers need to coordinate their research, and work together to create valuable knowledge?

"If it isn't worth doing, it isn't worth doing well"

(Donald Hebb, quoted by Daniel Dennett)

Really raising the bar means asking: Which research is worth doing well?

Want to know if something replicates?

Then you have to replicate it.

Want to know if a measure is valid?

Then you have to validate it.

Want to know if an effect generalizes?

Then you have to test it.

Want to know if an effect is applicable?

Then you have to apply it.

If it is worth doing well, it needs to be worth spending a lot more resources on.

It might be somewhat uncomfortable to admit your research is **not valuable enough to do well**.

We will not get better at fixing **crises** unless we are willing to talk about the value of our research.

Three causes of the replication crisis: P-hacking, low power, publication bias.

P-hacking: Your work is so inconsequential no one will notice if you are wrong too often.

Low power: The scientific community does not think work is valuable enough to team up and collect large enough samples.

Publication bias: Your research is not valuable enough to write up (even if it changes what we believe is true).

We will not get better at fixing **replicability** unless we are willing to talk about the value of our research.

We should want to do research that is worth doing well.

This requires collective discussions about what is valuable, team science, and consensus. And we need to **put science first**.

sated of his kindness and goodness to admit the human spirit for his playfellow at that game. Lastly, I would address one general admonition to all; that they consider what are the true ends of knowledge, and that they seek it not either for pleasure of the mind, or for contention, or for superiority to others, or for profit, or fame, or power, or any of these inferior things; but for the benefit and use of life; and that they perfect and govern it in charity. For it was from lust of power that the angels fell, from lust of knowledge that man fell; but of charity there can be no excess, neither did angel or man ever come in danger by it. [Bacon, 1620]

The requests I have to make are these. Of myself

Doing valuable science means doing what needs to be done, not what you want to do.

If a science community decides it is valuable to check code, you should check code.

If a science community decides it is valuable to share data, you should share data.

If a science community decides it is valuable to do replications, you should do replications.

If science comes first, we give up freedom in the service of a science worth doing.

The way forward after a replication crisis is **not** incremental change.

There is no fix for people who do not think their research is worth doing well.

Incremental change will just be a decades long game of whac-a-mole.

My latest failure: Getting people to be honest about sample size justifications.

The way forward after a replication crisis is **asking uncomfortable questions about the value of our research.**

The way forward after a replication crisis is **giving up some freedom, and do research worth doing.**

The way forward after a replication crisis is **putting science first**.

Thanks!

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