Reproducibility, Replicability, & Preproducibility
Boyle, Fisher, & Popper
What may we, with advantage, omit?

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Philip B. Stark

Department of Statistics, University of California, Berkeley

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Computational Reproducibility

Starting with the same data, can you produce the same tables, figures, and quantitative conclusions?

Experimental Replicability

Does repeating “the same” experiment and analyzing the resulting data the same way give “substantially the same” result?

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Variations: same lab, same reagents? different lab, different reagents?

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R. A. Fisher in the 21st Century

Invited Paper Presented at the 1996 R. A. Fisher Lecture

Bradley Efron

Abstract. Fisher is the single most important figure in 20th century statistics. This talk examines his influence on modern statistical thinking, trying to predict how Fisherian we can expect the 21st century to be. Fisher's philosophy is characterized as a series of shrewd compromises between the Bayesian and frequentist viewpoints, augmented by some unique characteristics that are particularly useful in applied problems. Several current research topics are examined with an eye toward Fisherian influence, or the lack of it, and what this portends for future statistical developments. Based on the 1996 Fisher lecture, the article closely follows the text of that talk.

Key words and phrases: Statistical inference, Bayes, frequentist, fiducial, empirical Bayes, model selection, bootstrap, confidence intervals.

1. INTRODUCTION

Even scientists need their heroes, and R. A. Fisher was certainly the hero of 20th century statistics. His ideas dominated and transformed our field to an extent a Caesar or an Alexander might have envied. Most of this happened in the second quarter of the century, but by the time of my own education Fisher had been reduced to a somewhat minor figure in American academic statistics, with the influence of Neyman and Wald rising to their high water mark.

There has been a late 20th century resurgence of interest in Fisherian statistics, in England where his influence never much waned, but also in America and the rest of the statistical world. Much of this revival has gone unnoticed because it is hidden behind the dazzle of modern computational methods. One of my main goals here will be to clarify Fisher's influence on modern statistics. Both the

to some speculations on Fisher's role in the statistical world of the 21st century.

What follows is basically the text of the Fisher lecture presented to the August 1966 Joint Statistical meetings in Chicago. The talk format has certain advantages over a standard journal article. First and foremost, it is meant to be absorbed quickly, in an hour, forcing the presentation to concentrate on main points rather than technical details. Spoken language tends to be livelier than the gray prose of a journal paper. A talk encourages bolder distinctions and personal opinions, which are dangerously vulnerable in a written article but appropriate I believe for speculations about the future. In other words, this will be a broad-brush painting, long on color but short on detail.

These advantages may be viewed in a less favorable light by the careful reader. Fisher's mathematical arguments are beautiful in their power and

Frontispiece of Fisher's *The Design of Experiments*:

I AM very sorry, Pyrophilus, that to the many (elsewhere enumerated) difficulties which you may meet with, and must therefore surmount, in the serious and effectual prosecution of experimental philosophy I must add one discouragement more, which will perhaps as much surprise as dishearten you; and it is, that besides that you will find (as we elsewhere mention) many of the experiments published by authors, or related to you by the persons you converse with, false and unsuccessful (besides this, I say), you will meet with several observations and experiments which, though communicated for true by candid authors or undistrusted eye-witnesses, or perhaps recommended by your own experience may, upon further trial, disappoint your expectation, either not at all succeeding constantly or at least varying much from what you expected.

–Robert Boyle, 1673, Concerning the Unsuccessfulness of Experiments.

Fisher on experimental "proof"

> . . . [N]o isolated experiment, however significant in itself, can suffice for the experimental demonstration of any natural phenomenon; for the "one chance in a million" will undoubtedly occur, with no less and no more than, its appropriate frequency, however surprised we may be that it should occur to *us*. **In order to assert that a natural phenomenon is experimentally demonstrable we need, not an isolated record, but a reliable method of procedure.** In relation to the test of significance, we may say that a phenomenon is experimentally demonstrable when we know how to conduct an experiment which will rarely fail to give us a statistically significant result.

–Fisher, 1935, *The Design of Experiments*

NAOMI A. P. STARK

No reproducibility without preproducibility

Instead of arguing about whether results hold up, let's push to provide enough information for others to repeat the experiments, says **Philip Stark**.

From time to time over the past few years, I've politely refused requests to referee an article on the grounds that it lacks enough information for me to check the work. This can be a hard thing to explain.

Our lack of a precise vocabulary — in particular the fact that we don't have a word for 'you didn't tell me what you did in sufficient detail for me to check it' — contributes to the crisis of scientific reproducibility. In computational science, 'reproducible' often means that enough information is provided to allow a dedicated reader to repeat the calculations in the paper for herself. In biomedical disciplines, 'reproducible' often means that a different lab, starting the experiment from scratch, would get roughly the same experimental result.

In 1992, philosopher Karl Popper wrote: "Science may be described as the art of systematic oversimplification — the art of discerning what we may with advantage omit." What may be omitted depends on the discipline. Results that generalize to all universes (or perhaps do not even require a universe) are part of mathematics. Results that generalize to our Universe belong to physics. Results that generalize to all life on Earth underpin molecular biology. Results that generalize to all mice are murine biology. And results that hold only for a particular mouse in a particular lab in a particular experiment are arguably not science.

Communicating a scientific result requires enumerating, recording and reporting those

SCIENCE SHOULD BE 'SHOW ME', NOT 'TRUST ME'.

or analysis is preproducible if it has been described in adequate detail for others to undertake it. Preproducibility is a prerequisite for reproducibility, and the idea makes sense across disciplines.

The distinction between a preproducible scientific report and current common practice is like the difference between a partial list of ingredients and a recipe. To bake a good loaf of bread, it isn't enough to know that it contains flour. It isn't even enough to know that it contains flour, water, salt and yeast. The brand of flour might be omitted from the recipe with advantage, as might the day of the week on which the loaf was baked. But the ratio of ingredients, the operations, their timing and the temperature of the oven cannot.

Given preproducibility — a 'scientific recipe' — we can attempt to make a similar loaf of scientific bread. If we follow the recipe but do not get the same result, either the result is sensitive to small details that cannot be controlled, the result is incorrect or the recipe was not precise enough (things were omitted to disadvantage).

Depending on the discipline, preproducibility might require information about materials (including organisms and their care), instruments and procedures; experimental design; raw data at the instrument level; algorithms used to process the raw data; computational tools used in analyses, including any parameter settings or ad hoc choices; code, processed data and software build environments; or analyses that were tried and abandoned.

Peer review is hamstrung by lack of pre-

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In a nutshell: Show Your Work!

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Provide *evidence* that you are right and a way to check, not just a claim.

What is the purpose of scientific publishing?

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- Establish priority / get credit?
- Communicate claims?
- Provide evidence that claims are correct?
- Provide enough information that others can re-undertake and verify?
- Provide methods to others, to contribute to science as a societal undertaking?

STEVEN SHAPIN & SIMON SCHAFFER

LEVIATHAN AND THE AIR-PUMP

HOBBES, BOYLE, AND THE EXPERIMENTAL LIFE

ological Essays of 1661 were written to another nephew, Richard Jones; the *History of Colours* of 1664 was originally written to an unspecified friend.[74] The purpose of this form of communication was explicitly to proselytize. The *New Experiments* was published so "that the person I addressed them to might, without mistake, and with as little trouble as possible, be able to repeat such unusual experiments. . . ."[75] The *History of Colours* was designed "not barely to relate [the experiments], but . . . to teach a young gentleman to make them."[76] Boyle wished to encourage young gentlemen to "addict" themselves to experimental pursuits and thereby to multiply both experimental philosophers and experimental facts.

In Boyle's view, replication was rarely accomplished. When he came to publish the *Continuation of New Experiments* more than eight years after the original air-pump trials, Boyle admitted that, despite his care in communicating details of the engine and his procedures, there had been few successful replications.[77] This situation had not materially changed by the mid-1670s. In the seven or eight years after the *Continuation*, Boyle said that he had heard "of very few experiments made, either in the engine

I used, or in any other made after the model thereof." Boyle now expressed despair that these experiments would ever be replicated. He said that he was now even more willing "to set down divers things with their minute circumstances" because "probably many of these experiments would be never either re-examined by others, or reiterated by myself." Anyone who set about trying to replicate such experiments, Boyle said, "will find it no easy task."[78]

Prolixity and Iconography

The third way by which witnesses could be multiplied is far more important than the performance of experiments before direct witnesses or the facilitating of their replication: it is what we shall call *virtual witnessing*. The technology of virtual witnessing involves the production in a *reader's* mind of such an image of an experimental scene as obviates the necessity for either direct witness or replication.[79] Through virtual witnessing the multiplication of witnesses could be, in principle, unlimited. It was therefore the most powerful technology for constituting intellectual collective had mutually to assure themselves and others that belief in an empirical experience was warranted. Matters of fact were the outcome of the process of having an empirical experience, warranting it to oneself, and assuring others that grounds for their belief were adequate. In that process a multiplication of the witnessing experience was fundamental. An experience, even of a rigidly controlled experimental performance, that one man alone witnessed was not adequate to make a matter of fact. If that experience could be extended to many, and in principle to all men, then the result could be constituted as a matter of fact. In this way, the matter of fact is to be seen as both an epistemological and a social category. The foundational item of experimental knowledge, and of what counted as properly grounded knowledge generally, was an artifact of communication and whatever social forms were deemed necessary to sustain and enhance communication.

of trust and assurance that the things had been done and done in the way claimed.

The technology of virtual witnessing was not different in kind to that used to facilitate actual replication. One could deploy the same linguistic resources in order to encourage the physical replication of experiments or to trigger in the reader's mind a naturalistic image of the experimental scene. Of course, actual replication was to be preferred, for this eliminated reliance upon testimony altogether. Yet, because of natural and legitimate suspicion among those who were neither direct witnesses nor replicators, a greater degree of assurance was required to produce assent in virtual witnesses. Boyle's literary technology was crafted to secure this assent.

Buckheit and Donoho, 1995

An article about computational result is advertising, not scholarship. The actual scholarship is the full software environment, code and data, that produced the result.

By working preproducibly, you . . .

- allow others “without mistake, and with as little trouble as possible, to be able to repeat such unusual experiments”
- make “multiplication” and “virtual witnessing” possible
- provide evidence that your claim is a fact

If you do not work preproducibly, you . . .

- merely advertise the result
- ask others to take the result on faith
- withhold crucial evidence needed to check or repeat your work
- make actual replication/reproduction even less likely

Science should be *show me*, not *trust me*.

Nullius in verba

Many concepts, many labels, used inconsistently

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No term for “not enough information to try.”

Preproducibility versus Reproducibility and Replicability

- A failure of *preproducibility* is often a failure of scientific *communication*.
- A failure of *reproducibility* or *replicability* could be a false discovery, a failure of practice, or a sign of something scientifically interesting

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- How stable?

What *ceteris* need not be *paribus*?

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Level of generalization *defines* scientific disciplines**

- If you want to generalize to all time and all universes: math
- If you want to generalize to our universe: physics
- If you want to generalize to all life on Earth: molecular and cell biology
- If you want to generalize to all fish: ichthyology
- If you want to generalize to TL strain of *Danio rerio*: I don't know
- This animal in this lab in this experiment today: maybe not science?

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** "All science is either physics or stamp collecting." —Lord Rutherford

JBS Haldane, 1926. “On Being the Right Size,” *Harper’s Magazine*

> *You can drop a mouse down a thousand-yard mine shaft; and, on arriving at the bottom, it gets a slight shock and walks away, provided that the ground is fairly soft. A rat is killed, a man is broken, a horse splashes. For the resistance presented to movement by the air is proportional to the surface of the moving object. . . .*

Is this physics, biology, or what?

Abstraction and Replicability

- If something only happens under *exactly* the same circumstances, unlikely to be useful.
- What factors may we, with advantage, omit?
- If attempt to replicate/reproduce fails, *why* did it fail? (cf Newton)
  - effect is intrinsically variable or intermittent
  - result is a statistical fluke or “false discovery”
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If the necessary qualification is too restrictive, the result might change disciplines.

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- how generally do the results hold? how stable are the results to perturbations of the experiment?

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- Were there *ad hoc* choices? Did they matter?
- What other analyses were tried? How was multiplicity treated?
- Can someone else use the procedures and tools?

Variation: wanted and unwanted

- genotype, biology, lab, procedures, handlers, reagents, feed/diet, water circulation, water quality, temperature, pH, conductivity, noise, visual background, size of cross-breeding cohorts, subclinical infections . . .

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- Want results stable wrt *some* kinds of variability
- OTOH, variability *itself* can be scientifically interesting

Genotype–environment interactions in mouse behavior: A way out of the problem

Neri Kafkafi*†‡, Yoav Benjamini†§, Anat Sakov§, Greg I. Elmer*, and Ilan Golani¶

*Department of Psychiatry, Maryland Psychiatric Research Center, University of Maryland School of Medicine, Baltimore, MD 21228; and §Department of Statistics and Operations Research, The Sackler Faculty of Exact Sciences, and ¶Department of Zoology, George S. Wise Faculty of Life Sciences, Tel Aviv University, Tel Aviv 69978, Israel

Communicated by Philip Teitelbaum, University of Florida, Gainesville, FL, December 21, 2004 (received for review December 4, 2003)

In behavior genetics, behavioral patterns of mouse genotypes, such as inbred strains, crosses, and knockouts, are characterized and compared to associate them with particular gene loci. Such genotype differences, however, are usually established in single-laboratory experiments, and questions have been raised regarding the replicability of the results in other laboratories. A recent multilaboratory experiment found significant laboratory effects and genotype × laboratory interactions even after rigorous standardization, raising the concern that results are idiosyncratic to a particular laboratory. This finding may be regarded by some critics as a serious shortcoming in behavior genetics. A different strategy is offered here: (*i*) recognize that even after investing much effort in identifying and eliminating causes for laboratory differences, genotype × laboratory interaction is an unavoidable fact of life. (*ii*) Incorporate this understanding into the statistical analysis of multilaboratory experiments using the mixed model. Such a statistical approach sets a higher benchmark for finding significant genotype differences. (*iii*) Develop behavioral assays and endpoints that are able to discriminate genetic differences even over the background of the interaction. (*iv*) Use the publicly available multilaboratory results in single-laboratory experiments. We use

the A/J strain in two laboratories, whereas it is lower in the third (see Fig. 2). Such a genotype × laboratory interaction (*GxL*) might arise if a particular genotype reacts differently than another genotype, for no identifiable cause, to the peculiarities of a specific laboratory, and therefore cannot be eliminated by using a common genotype as a local control. Crabbe *et al.* (3) thus concluded: "experiments characterizing mutants may yield results that are idiosyncratic to a particular laboratory." The lack of across-laboratory replicability demonstrated in their study might be interpreted by some critics as a serious shortcoming in behavior genetics at large (4) because currently almost all experiments are conducted within a single laboratory.

When analysis reveals a substantial *GxL* effect, this effect might be caused by some methodological artifact in the test or the laboratory environment, which is in no way edifying and in every way misleading. It would be seen as bad science, once the artifact is traced to its origins. Successful correction of this artifact will be reflected by a great reduction in the size of the interaction.

The main remedy advocated to date for the *GxL* problem is thus a more careful standardization of test protocol, housing

Computational p/reproducibility

- I worry about variation with analysis/methodology & *implementation* of tools
- Undesirable for analysis to be unstable, but algorithms matter, numerics matter, . . .
- Relying on packaged/commercial tools can be a problem
- Adopt tools from software development world:
  - revision control systems (not, eg, Dropbox or Google Docs)
  - documentation, documentation, documentation
  - coding standards/conventions
  - pair programming
  - issue trackers
  - code reviews (and in teaching, grade students' *code*, not just their *output*)
  - unit testing
  - code coverage testing and continuous integration
  - regression testing
  - scripted analyses: no point-and-click tools, *especially* spreadsheet calculations

Spreadsheets might be OK for data entry. Not for calculations

- Conflates input, code, output, presentation

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- Other bugs in Excel +, *, statistical routines; PRNG still won't accept a seed; etc.

A Guide to IMF Stress Testing: Methods and Models

A Guide to IMF Stress Testing: Methods and Models
Ancillary Materials

To go back to the book, please **click here**.

- Toolkit Files

Note to readers: *Ancillary materials are arranged based on the chapter in which they appear in the book.*

The files listed below are also available on the companion CD.

Chapter 3
Stress Tester 3.0

Chapter 4
Excel Spreadsheet Macro for the Breaking Point Method

Chapter 5
Excel Spreadsheet Macro for the Next-Generation Solvency Stress Test

Chapter 6
Excel Spreadsheet Macro for the Market and Funding Liquidity Stress Tests

Chapter 7
Excel Spreadsheet Macro for the Next-Generation Systemwide Liquidity Stress Test

Chapter 10
Excel Add-in for the CreditRisk+ Model

Chapter 12
Excel Spreadsheet Macro for Stress Testing Defined Benefit Pension Plans

Chapter 14
Excel-based Program for Bank Network Analysis

Home
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EuSpRIG 2018 Annual Conference
Register for EuSpRIG 2018 Conference
EuSpRIG 2018 Call for Papers & Presentations
Basic Research
Best Practice
Horror Stories
Regulators' Presentations
Conference Abstracts, Papers & Indexes
Conference Reports & Videos
Delegates

Constitution
History
Our Talks & Presentations
Quotable Quotes
Press & Website
Committees
Yahoo Group
Training Videos
Humour
Sponsors
Useful Links
Contact

EuSpRiG
European Spreadsheet
Risks Interest Group

EuSpRIG Horror Stories

Spreadsheet mistakes - news stories

Public reports of spreadsheet errors have been sought out on behalf of EuSpRIG by Patrick O'Beirne of Systems Modelling for many years. There are very many reports of spreadsheet related errors and they seem to appear in the global media at a fairly consistent rate.

These stories illustrate common problems that occur with the uncontrolled use of spreadsheets. In many cases, we identify the area of risk involved and then say how we think the problem might have been avoided.

Figure 3: image

Bargain Booze owner Conviviality must raise £125m to halt bankruptcy

Firm issues third profits warning; says it will meet investors to raise funds via a share placing

▲ Diana Hunter, chief executive of Conviviality, has stepped down. Photograph: Rex/Shutterstock

Bargain Booze's owner, Conviviality, has made clear it is likely to go bust unless it can raise £125m, as it issued its third profits warning in a month.

Relying on spreadsheets for important calculations is like driving drunk:

No matter how carefully you do it, a wreck is likely.

2014 Coverity study

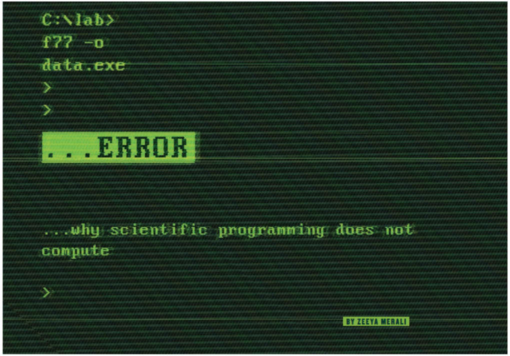
- 0.61 errors per 1,000 lines of source code in open-source projects

2014 Coverity study

- 0.61 errors per 1,000 lines of source code in open-source projects
- 0.76 errors per 1,000 lines of source code in commercial software

2014 Coverity study

- 0.61 errors per 1,000 lines of source code in open-source projects
- 0.76 errors per 1,000 lines of source code in commercial software
- Scientists generally don't use good software engineering practices, so expect worse in practice.



When hackers leaked thousands of e-mails from the Climatic Research Unit (CRU) at the University of East Anglia in Norwich, UK, last year, global-warming sceptics pored over the documents for signs that researchers had manipulated data. No such evidence emerged, but the e-mails did reveal another problem — one described by a CRU employee named "Harry", who often wrote of his wrestling matches with wonky computer software.

"Yup, my awful programming strikes again," Harry lamented in one of his notes, as he attempted to correct a code analysing weather-station data from Mexico.

Although Harry's frustrations did not ultimately compromise CRU's work, his difficulties will strike a chord with scientists in a wide range of disciplines who do a large amount of coding. Researchers are spending more and more time writing computer software to model biological structures, simulate the early evolution of the Universe and analyse past climate data, among other topics. But programming experts have little faith that most scientists are up to the task.

A quarter of a century ago, most of the computing work done by scientists was relatively straightforward. But as computers and programming tools have grown more complex, scientists have hit a "steep learning curve", says James Hack, director of the US National Center for Computational Sciences at Oak Ridge National Laboratory in Tennessee. "The level of effort and skills needed to keep up aren't in the wheelhouse of the average scientist."

As a general rule, researchers do not test or document their programs rigorously, and they rarely release their codes, making it almost impossible to reproduce and verify published results generated by scientific software, say computer scientists. At best, poorly written ▶

F1000Research

F1000Research 2015, 3:303 Last updated: 25 DEC 2016

OPINION ARTICLE

REVISED Rampant software errors may undermine scientific results [version 2; referees: 2 approved]

David A. W. Soergel[1,2]

[1]Department of Computer Science, University of Massachusetts Amherst, Amherst, USA
[2]Current address: Google, Inc., Mountain View, CA, USA

v2 **First published:** 11 Dec 2014, **3**:303 (doi: 10.12688/f1000research.5930.1)
Latest published: 29 Jul 2015, **3**:303 (doi: 10.12688/f1000research.5930.2)

Abstract
The opportunities for both subtle and profound errors in software and data management are boundless, yet they remain surprisingly underappreciated. Here I estimate that any reported scientific result could very well be wrong if data have passed through a computer, and that these errors may remain largely undetected. It is therefore necessary to greatly expand our efforts to validate scientific software and computed results.

Open Peer Review

Referee Status: ✓ ✓

	Invited Referees 1	2
REVISED **version 2** published	✓ report	✓ report

Thermo ML: $\sim$20% of papers that otherwise would have been accepted had serious errors.

Stodden (2010) Survey of NIPS re code & data:

Excuse	code	data
Time to document and clean up	77%	54%
Dealing with questions from users	52%	34%
Not receiving attribution	44%	42%
Possibility of patents	40%	N/A
Legal Barriers (i.e. copyright)	34%	41%
Time to verify release with admin	N/A	38%
Potential loss of future publications	30%	35%
Competitors may get an advantage	30%	33%
Web/disk space limitations	20%	29%

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Fear, greed, ignorance, & sloth

Hacking the limbic system

If I say *just trust me* and I’m wrong, I’m untrustworthy.

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If I say *just trust me* and I'm wrong, I'm untrustworthy.

If I say *here's my work* and it's wrong, I'm honest, human, and serving scientific progress.

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Science should be "help me if you can," not "catch me if you can."

Revision-control systems for teaching, research, collaboration

- Teaching use cases:
  - submit homework by pull request (can see commits)
  - collaborate on term projects
  - use for timed exams: push at a coordinated time, pull requests
  - supports automated testing of code
- Research use cases
  - 1st step of new project: create a repo
  - commits leave breadcrumbs
  - issue trackers
  - notes, code, manuscripts, etc. (not ideal for large datasets)
  - know last version that worked
- Collaboration use cases
  - parallel development & feature implementation through branches
  - can find last working version of code; *blame*

Scripts & notebook-style tools

- IPython/Jupyter notebook (Sweave and knitR are great for papers; less good for workflow), . . .

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- leave breadcrumbs
- readable
- easy to re-run and modify analysis

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- easy to re-run and modify analysis
- easy to build on previous analyses

Preproducibility is collaboration w/ people you don't know,

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including yourself next week.

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including yourself next week.

Preproducibility & collaboration

- same habits, attitudes, principles, and tools facilitate both
- develop better work habits, *computational hygiene*
- analogue of good lab technique in wet labs

Why work p/reproducibly?

There is only one argument for doing something; the rest are arguments for doing nothing. The argument for doing something is that it is the right thing to do.
—Cornford, 1908. Microcosmographia Academica

How can we do better?

- Scripted analyses: no point-and-click tools, *especially* spreadsheet calculations

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- Issue trackers
- Code reviews (and in teaching, grade students' *code*, not just their *output*)
- Code tests: unit, integration, coverage, regression

Integration tests

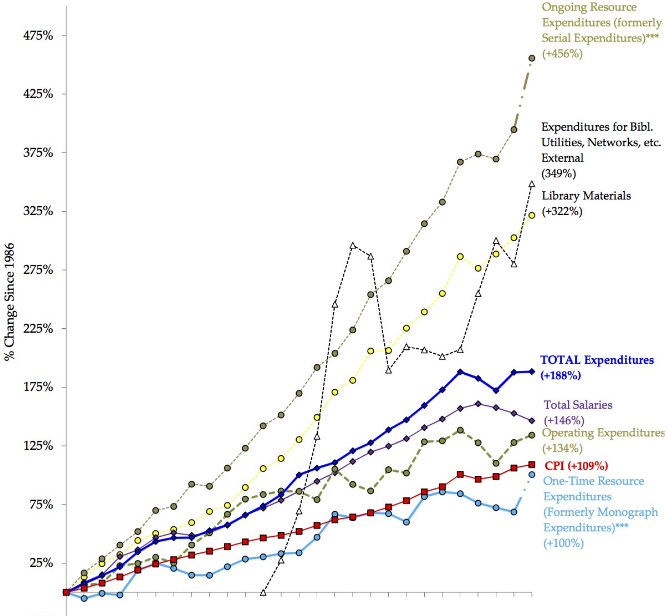
Checklist

1. Don't use spreadsheets for calculations.
2. Script your analyses, including data cleaning and munging.
3. Document your code.
4. Record and report software versions, including library dependencies.
5. Use unit tests, integration tests, coverage tests, regression tests.
6. Avoid proprietary software that doesn't have an open-source equivalent.
7. Report all analyses tried (transformations, tests, selections of variables, models, etc.) before arriving at the one emphasized.
8. Make code and code tests available.
9. Make data available in an open format; provide data dictionary.
10. Publish in open journals.

Why open publication?

- Research funded by agencies
- Conducted at universities by faculty et al.
- Refereed/edited for journal by faculty at no cost to journal
- Pages charges paid by agencies
- Exclusionary & morally questionable for readers have to pay to view

ARL 1986-2016 Also CFUCBL rept



What's the role of a journal?

- Gatekeeping/QC by editors & referees
- Dissemination
- Archive

cess were not immediately fulfilled; second, it assured the reader that the relator was not wilfully suppressing inconvenient evidence, that he was in fact being faithful to reality. Complex and circumstantial accounts were to be taken as undistorted mirrors of complex experimental outcomes.[87] So, for example, it was not legitimate to hide the fact that air-pumps sometimes did not work properly or that they often leaked: ". . . I think it becomes one, that professeth himself a faithful relator of experiments not to conceal" such unfortunate contingencies.[88] It is, however, vital to keep in mind that in his circumstantial accounts Boyle proffered only a *selection* of possible contingencies. There was not, nor can there be, any such thing as a report that notes *all* circumstances that might affect an experiment. Circumstantial, or stylized, accounts do not, therefore, exist as pure forms but as publicly acknowl-

It’s hard to teach an old dog new tricks.

It's hard to teach an old dog new tricks.

But you are scientist 'puppies!'

Teach by doing science preproducibly; don't focus on tools.

Eyes on the code, not just the output!

In: Peer Review ▸

A post-publication peer review success story

September 29, 2017 * Author: Jon Tennant

In 2016, Dr. Joel Pitt and Prof. Helene Hill published an important paper in ScienceOpen Research. In their paper, they propose new statistical methods to detect scientific fraudulent data. Pitt and Hill demonstrate the use of their method on a single case of suspected fraud. Crucially, in their excellent effort to combat fraud, Pitt and Hill make the raw data on which they tested their method publicly available on the Open Science Framework (OSF). Considering that a single case of scientific fraud can cost institutions and private citizens a huge amount of money, their result is provocative, and it emphasizes how important it is to make the raw data of research papers publicly available.

The Pitt and Hill (2016) article was read and downloaded almost 100 times a day since its publication on ScienceOpen. More importantly, it now has 7 independent post-publication peer reviews and 5 comments. Although this is a single paper in ScienceOpen's vast index of 28 million research articles (all open to post-publication peer review!), the story of how this article got so much attention is worth re-telling.

Figure 9: image

- Get students thinking about alternative models for scholarly publication;
- Get students thinking about reproducibility and open science;
- Get students to work collaboratively on a data analysis project that involves thinking hard about the underlying science;
- Get students to register with ORCID;
- Get students to post their analyses on GitHub so that their own work is reproducible/extensible;
- Get students their first scientific publication.

For another step of Open Science brilliance, the reviews themselves sought to be completely reproducible, with the code for all the students' calculations is available on GitHub (eg here and here)!

Figure 10: image

Furthermore, unlike almost every other Post Publication Peer Review function out there, the peer reviews on ScienceOpen are integrated with graphics and plots. This awesome feature was added specifically for Prof. Stark's course, but note that it is now available for any peer review on ScienceOpen.

- Maurer and Mohanty, who stated that the work was an important demonstration of the use of statistical methods for detecting fraud;
- Hejazi, Schiffman and Zhou, who evaluated the work as comprehensible but largely incomplete;
- Dwivedi, Hejazi, Schiffman and Zhou, who note that the research is a strong advocate for detecting scientific fraud and the use of reproducible statistical methods;
- Stern, Gong and Zhou call the research clever in the application of the techniques t uses to address a pressing problem in science;
- Bertelli, DeGraaf and Hicks think the analysis is convincing and valuable, but with a methodology that could be refined;
- Hung, Sheehan, Chen and Liu evaluated the paper, finding a few minor discrepancies between their own results on those of the published research.

README.md

Review of Statistical Analysis of Numerical Preclinical Radio-biological Data

Raaz Dwivedi+, Antonio Iannopollo+ and Jiancong Chen*

+ Department of EECS

* Department of Civil & Environmental Engineering

University of California, Berkeley

This review reproduces tests and results presented by Pitt and Hill in the paper *Statistical Analysis of Numerical Preclinical Radio-biological Data* and discusses some other non-parametric techniques, such as Permutation Tests, which allow to analyze data with less restrictive assumptions. The focus of the review is on the statistical methodology rather than the underlying biological aspects and assumptions of the original work, which are not discussed. Although not expert in statistical methods for fraud detection, we do believe that permutation tests are promising in this context, as demonstrated by the results presented here. This review has been developed as a term project for a Graduate Level Course on Statistical Models at UC Berkeley.

The organization of this repository is the following:

- **Review** is the main review folder:
  - **Report** contains our paper review in several formats;
  - *IPython Notebooks* contains the most relevant ipython notebooks and data, used to derive the conclusions in the *Report* folder;
- *Pitt_Hill.pdf* is the paper under review;
- *README.md* is this file;
- *Scrapbook* contains some working material, and it is included for completeness and transparency.

Show your work!

Fisher on t-tests

[](./ReproPics/

Resources

- Data Carpentry, Software Carpentry

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- RunMyCode, Research Compendia, FigShare

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- Federation of American Societies for Experimental Biology

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- Federation of American Societies for Experimental Biology
- Was ist open science? http://opensciencea sap.org/open-science/