

Practical Tips for Writing and Publishing Applied Economics Papers

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Context / Disclaimers

- These comments are based on our own research experiences as (flawed but hopefully improving) authors, (too frequent) reviewers, and field journal editors.
- Tim: current co-editor of *AJAE*; Jay: past co-editor of *JEEM*. We do not represent these journals in these comments.
- These comments are part of a longer course that we developed on conducting, writing, and publishing applied economics research.

Context / Disclaimers

- We focus on lessons for *applied* economic research (applied theory and empirics) especially for agricultural, environmental, resource, food, and health economics.
- We focus on the potential contributions of mere mortals. If you are a genius, this is probably not for you.
- We draw lessons from a very large number of inspiring papers. We provide a list of references at the end and try to assign attribution where obviously necessary ... but many ideas are not fully cited.
- Although many lessons will apply to a wide range of applied researchers, this discussion is targeted towards graduate students.

- **Basic Principles**

- Study what interests you.
 - You will not work hard enough, or smart enough, otherwise.
- Talk to people about research all of the time.
 - Do not be shy or humble. Attend seminars and conferences.
- Be persistent and do not get discouraged.
 - Paul Samuelson: “we must reckon with the fact that some of [our] best work would not have gotten done if it had not been an outgrowth of some of [our] less transcendental achievements.”
 - Expect and accept rejection. Akerlof’s “The Market for Lemons” was rejected from 3 journals before being published!
- Perfection is a standard that can never be achieved.
 - Work until expected MB = expected MC of continuing.
- The research process is non-linear.
 - Be prepared to go back to earlier stages of the research process and refine your questions, approach, etc.

Research \neq Analysis

- For most applied economists, writing/solving models and evaluating data is fun.
 - We are very lucky to get to do this!
- But analysis alone is not research.
 - Research is not an analytical model. Research is not the econometric estimation. Research is not (typically) new pure theory or theoretical econometric tools.
- Research is telling a “story” – to other scholars, to policymakers, to students, etc.
- Analysis is an important, but small part, of research.
- Most young researchers forget this last point.

An applied economics “story”

- There’s an important *question* about the world that we can’t yet convincingly answer.
- Answering this question is *important* ...
 - for understanding the world, and/or,
 - for understanding economics.
- Other scholars and other people have considered this, or a similar, question.
 - But I can explain why we don’t yet have a believable answer.
 - But I can explain why this exact important question is *unanswered*.
- I use the *TOOLS* of economic analysis (models and econometrics) to provide *NEW* evidence on this question.
- I find an *answer* to the question. We learn something new.

Communication is undervalued ...

- Since telling an economic “story” is what matters, crafting oral and written communication strategies is *at least as important* as the analysis itself.
- Communicating what we’ve done and why is, for many of us, much harder and much less fun than doing analysis.
- This implies, to be successful, we must spend as much or more time crafting, writing, and refining our “story” and our communication strategies.

- The publication process at economics field journals

How the process works

- Decide which journals are a *good fit* for your research.
 - Does this journal publish papers on these general subjects?
 - Does this journal have a strong bias for or against the methodologies in the paper?
 - Be ambitious but not wasteful with your time, or others', time.
 - Send the paper ~one tier above where you think it belongs.
 - Before submission, get advice from experienced colleagues and friends on where to submit.
- Submit your paper through an online portal.

How the process works

- An editor will look at the paper and decide whether to send it out for review or “desk reject.”
 - Editors do NOT read the entire paper carefully on submission, and most only carefully read the abstract and/or introduction.
 - Editors are asking:
 - Is this research interesting, novel, and important?
 - Is this research likely of interest to readers of this journal?
 - Does the paper appear competently communicated and competently executed?
 - Desk reject decisions typically take between 1 day and 1 month.
 - Do not bother arguing with desk rejections.
 - Some perspective: Desk rejects are not bad outcomes....Be reflective, but not obsessive, after desk rejections.

The paper is under review. Now what?

- Wait! 2-5 months is now typical.
- You will eventually receive 1-4 referee reports (2-3 is most common) and a decision letter from the editor.
- The editor will most often be rejecting your paper. Almost all good journals have acceptance rates well below 10% (of a highly selected sample at the good journals).
- If you are lucky (and good), you will be invited to submit a revised manuscript addressing the editor and referee comments.
- The editor can accept, conditionally accept subject to minor changes, offer another 'revise and resubmit,' or reject after that.
- The whole process takes 0.5 years to several years at good journals. Over 1 year is the norm, not accounting for rejections.

Things to know

- A biggest reason for rejection is a minimal or unclear contribution. Why is this question important? Why is your analysis *new*?
- Another big reason for rejection is poor writing. Poorly written papers confuse the reviewer and obscure contributions. You must get the abstract and intro 'right.'
- Other common reasons for rejection include:
 - Poor fit for the journal. Know the kind of papers journals publish before submitting a paper there.
 - Conclusions that do not follow from the analysis. Acknowledge your weaknesses, rather than trying hard to hide them.
 - Uncited references. Know the literature. Referees get angry if they believe their work should be cited and was not.

- Good research questions

The Starting Point

- Professor Jesse Shapiro has a great presentation entitled “how to give a applied micro talk.” Google it.
- Shapiro’s starting point:
 - People do NOT care about your research.
 - You have ~2 minutes to change their mind.
- Thought experiment: when thinking about research questions, imagine an economist friend and a non-economist friend/relative.
 - Can you convince your economist friend that your research is interesting and important?
 - Can you convince your non-economist friend/relative that your research is interesting and important?

If you can not convince both friends that your research is interesting, in a very small amount of time, you probably need a new question...or at least a new way to ‘market’ the question.

What makes a *good research question*?

- The question is important. If X changes:
 - Many people are worse off or better off
 - Important subpopulations are far better off or far worse off
- The question is controversial or puzzling.
 - Test of an important theoretical prediction.
- The subject is very common or involves large outlays of social resources.
- A good question will often complete the following statements:
“I wonder if” ; “It is interesting that ...”
- [Example here...](#)
- **If you can't convince yourself that your research question falls into one of these categories, you may need a new question.**
- (material above adapted from Keith Head's Intro formula)

A useful exercise.

- If you are far enough along, think about a topic of your research. If not, think about a paper you like.
- State the question as a yes/no question:
 - Example. “Do soda taxes reduce obesity?”
- Establish how the question is important.
 - Are many people worse off or better off?
 - Are some people far better off or far worse off?
 - Does the subject involve large public or private resource expenditures?
 - If empirical: does this question help resolve an important theoretical puzzle?
 - Does the topic involve extremely common situations?

- Conduct this exercise for papers you read.
- Conduct this exercise **MANY TIMES** for every paper you write.

Good research questions are often found in the real world.

- Learn the institutional details of your research area.
- Learn what things the general public finds important about your topic. Follow the news, TV, radio related to your topic.
- Learn what business people, government officials, etc. want to know about your topic. Talk to non-scholars about your topic!
- You can also look for ideas in your own life.
 - [Example here...](#)

Good questions are informed by the economics literature.

- Learn what questions have been asked – and what questions have not been answered – in the literature on your topic.
- Peruse abstracts and introductions from the scholarly literature. Look for gaps in knowledge.
- Identify a good survey article related to your broad interests.
 - Potentially good outlets for this purpose include, but are not limited to, the *Journal of Economic Literature*, the *Journal of Economic Surveys*, and the *Annual Review of Economics*.
 - Look for unanswered, controversial or unresolved, or incompletely answered questions.

Extending an important paper can produce a good research question.

- Go to your library's electronic journals and search for papers broadly related to your interests.
- Look for a paper published in a **top** journal (e.g. AER, QJE, JPE, etc.) that can be extended in some interesting way.... To new countries, to different time periods.
- Are there *interesting* aspects of the research question that are incompletely or imperfectly answered?
- If you are extending existing papers, make sure your extension says something *new*.
 - [Example here](#) ...

Look outside of economics

- Another great way to find research ideas is to look outside of economics scholarship.
 - What are non-economic scholars interested in?
 - Can you bring an *economic angle* to the questions other scholars find important?
 - [Example here....](#)

Remember your comparative advantage

- Can you offer something that is new or unique?
 - Do you know more about an interesting topic that nearly all other economists? [Example here...](#)
 - Do you have relationships that allow you to access unique data? [Example here...](#)
 - Do you have access to policymakers that might be open to experimentation, like an RCT? [Example here...](#)
- This is obvious ex-post but not necessarily ex-ante.

Applied research in developing nations

- In the *past*, economics research on North America was easier to publish than research on the rest of the world.
- It is now very easy to make the case that developing countries are more relevant and important.
 - For most policies, many people are worse off or better off.
 - For some policies, some people are made far better/worse off.
 - Many topics involve very large outlays of social resources.
 - Social choices in developing countries are made for complicated political economy reasons and therefore may have significant unintended consequences.
- Still, you must explain *why* your research question is important for other settings too.

Additional thoughts about research questions

- We are trying to learn about the world.
 - Many desk rejected papers (theory and empirics) are defined by assumption or method. They do not offer new insights.
 - Unless you are a pure mathematical theorist or a pure econometrician, avoid defining your contribution by technique, model, or skill.
- As noted by K. Head and J. Shapiro, the following are NOT good research questions:
 - What happens if I apply a new econometric method to estimate the relationship between X and Y?
 - What happens if I change an assumption in the famous Model X?
- You cannot imagine how many papers submitted to good journals have this motivation.

Additional thoughts about contribution

- Many desk rejected empirical papers are simply documenting empirical regularities.
- This is not typically not enough ... To the extent possible, explain and provide evidence on *why* it is true.
- Economics journals are for ideas that have bearing for *economics* research. What is the economic mechanism and where is the evidence for that economic mechanism?
- Also, having a contribution is not the same as communicating a contribution. DO NOT ASSUME YOUR CONTRIBUTION WILL BE OBVIOUS. [Examples here..](#)

- Implications...

(1) Master the 'elevator pitch.'

- What is the precise research question in <25 words? Ideally it will be in the 'yes/no' format.
- Why is this topic important to the real world? Why should someone that is not a scholar in your area think this is interesting?
- Why is this topic worthwhile to scholarship? Relative to what scholars in the area already know, what is *new* about your work? Don't assume someone knows this.
- Summarize your research approach in 2 short sentences or less. If your work is empirical, reference the dataset and the research design.
- What is the most important result?

Exercise

- If you are far enough along in your own research, can you sketch an elevator pitch for one of your own research projects?
- If not, can you sketch an elevator pitch for a research paper that you find especially interesting or inspiring?

(2) The introduction is the most important section of a written paper.

- The introduction is often the only section of a paper that people actually read after publication.
- Referees almost always decide whether to recommend or reject while reading the introduction.
- Editors typically read introductions very carefully. If they are not excited, the paper has a very low chance of getting published in that journal.

Principles

- The introduction should be edited or rewritten many, many times. Fullerton suggests it should be written first and rewritten or edited every time you write other section.
- **Imitate good writers for style.** Do so closely until you are naturally good at writing introductions.
 - It is plagiarism to use others' words directly.
 - It is not plagiarism to imitate others' paragraph structure, organization, etc.
 - Deconstruct the abstracts and introductions of papers you think are convincing.
 - Pick a few inspiring authors and see if you can reverse engineer systematic writing styles or signatures.
- The key content of the introduction is the same material in the elevator pitch.

The Introduction Formula for applied economic research

- This is Keith Head's "Introduction Formula".... Google it!
- Paragraph 1. Why is general subject interesting and important? Why might someone not in your discipline think this is important?
- Paragraph 2. What is the specific question and what does this paper do? You should actually write, "This paper address the question of" or "This paper explores..."
- Paragraph 3. The contribution or value-added. Describe approximately 3 contributions of the paper relative to the studies that have gone before it. What is new and novel, and why is this new material important for knowledge.

The Introduction Formula

(adaptation of Keith Head's formula)

- Paragraph 4. What exactly does the paper do? Summarize key research strategies (and key data). If a reader would anticipate an obvious problem with your approach (i.e. endogeneity), signal how you address the problem.
- Paragraph 5. Results. What do you find? If possible, restate how the findings are novel and important, perhaps signaling but not necessarily detailing implications for policy or the real world.
- Paragraph 6. The roadmap. Outline the organization of the remainder of the paper. Be specific enough, though, to avoid an outline that could go in any paper.

Okay, it's not quite a “formula”

- The introduction formula is a great place to *start* for applied microeconomics research.
- Until you become very good at writing papers, many of my colleagues will tell you to start with this formula.
- Then ask, what does this formula miss for my paper?
- What do I need to add? What do I need to subtract?
- Will reorganizing any key points make tell the economic “story” more clearly?

Common introduction problems

- Promising too much, or “bait and switch.” [Example here..](#)
- Waiting too long to state the question.
 - Reviewers, editors, etc. are impatient.
 - State the research question very clearly, typically no later than the 3rd paragraph.
 - [Example here...](#) Also, in your last paper, when did you state the question?
- Burying the question.
 - Do not be subtle. Say “This paper explores ...”
- Not articulating a forceful contribution.
 - Saying a paper is important because the topic is well studied is not a good strategy.
 - Restating the research question is not a contribution.
 - **Do not make it difficult to understand what is new.**

An example of the introduction formula in practice - Currie and Neidell (QJE 2005)

Air quality regulations are costly to both producers and consumers, and the optimal level of pollution abatement is hotly contested. Pollution abatement is often justified as something that will promote health, yet there is still much to be learned about the specific health effects. The EPA did not include infant mortality in the primary quantitative benefit analysis of the 1990 Clean Air Act Amendments in 1999 [Environmental Protection Agency 1999] because the weight of the scientific evidence linking infant health to air pollution was viewed as insufficient.¹

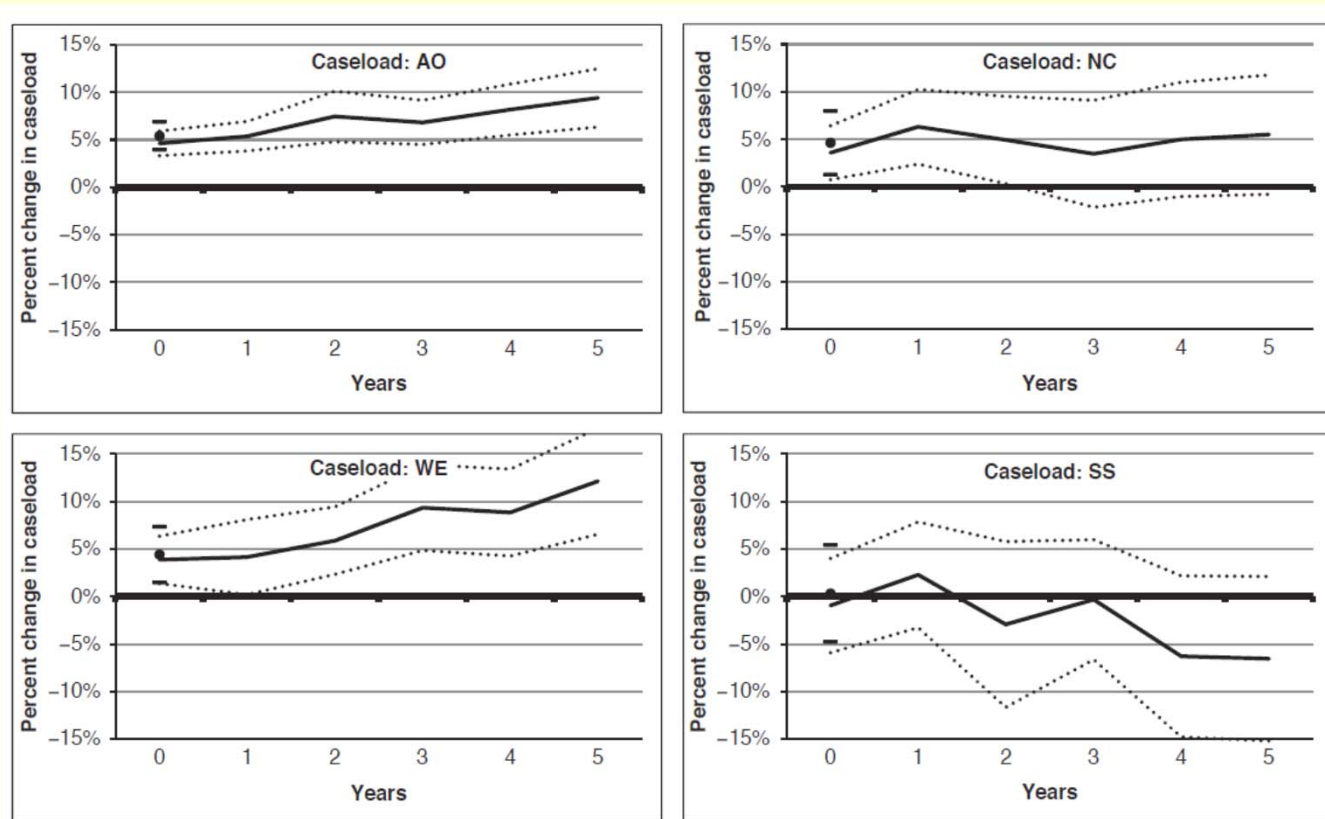
This paper addresses this issue by examining the impact of air pollution on infant health in California over the 1990s. Infants are of interest for two reasons. First, policy-makers and the public are highly motivated to protect these most vulnerable members of society. Second, in the case of infant death, the link between cause and effect is immediate, whereas for adults, diseases today may reflect pollution exposure that occurred many years ago.

Our work offers several innovations over the existing literature. First, many previous studies examine populations subject to greater levels of pollution because they lived further in the past or in some more heavily polluted place. In contrast, the experience of California in the 1990s is clearly relevant to the contemporary debate over pollution levels in the United States. Second, many studies examine a single pollutant in isolation (usually particulate matter), generally because of data limitations. We examine three “criteria” pollutants that are commonly monitored in the United States: Ozone (O₃), carbon monoxide (CO), and particulate matter (PM₁₀).² Third, we exploit weekly pollution data and rich individual-level data to estimate linear models that approximate hazard models, where the risk of death is defined over weeks of life and we control for the length of life as a flexible nonparametric spline.

Our framework allows us to control for a wide array of potential confounders in an effort to identify causal effects. In addition to controls for both postnatal and prenatal pollution exposure, we control for weather, the age of the child, observable characteristics of the mother and child (such as race, maternal age, child gender), and for a variety of unobservable characteristics that can be captured by month, year, zip code fixed effects (or combinations of these fixed effects). In our richest specification, the effects of pollution are identified using only variation within cells defined at the zip code, month, and year level. Of these controls, we find that those for the age of the child appear to be most important, which reflects the fact that the probability of death in the first weeks is much higher than the probability of death later in the first year.

Our estimates confirm that air pollution has a significant effect on infant mortality even at the relatively low levels of pollution experienced in recent years, and suggest that previous studies may have overlooked a potentially important role for CO. In particular, we estimate that the reductions in CO that occurred over the 1990s saved approximately 1000 infant lives in California. This finding is robust to many sensitivity analyses, and does not appear to be due to mortality displacement, or “harvesting.” In keeping with some of the previous research in this area, we also find an effect of PM10 in some specifications, but this finding is not as robust as the finding for CO.

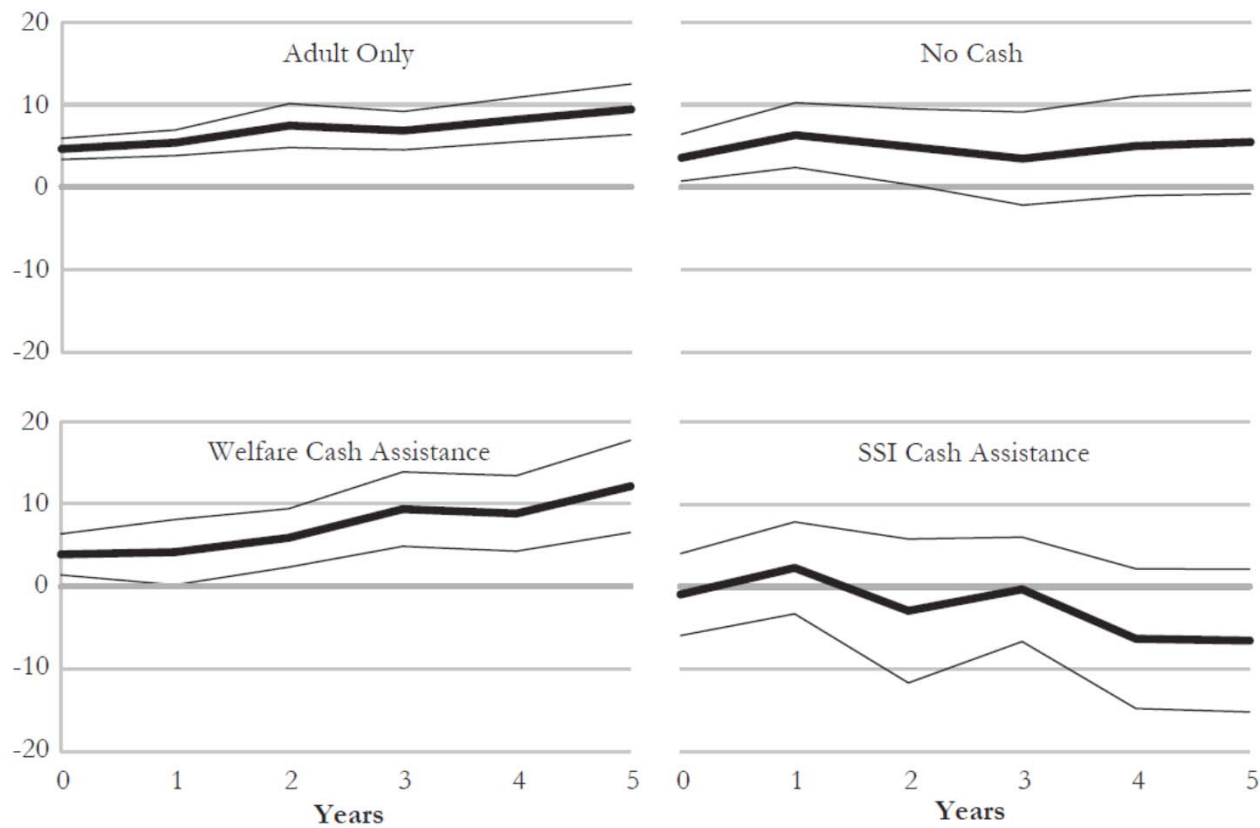
(3) Every part of your paper should quickly support the story. Read Schwabish JEP14. This graph....



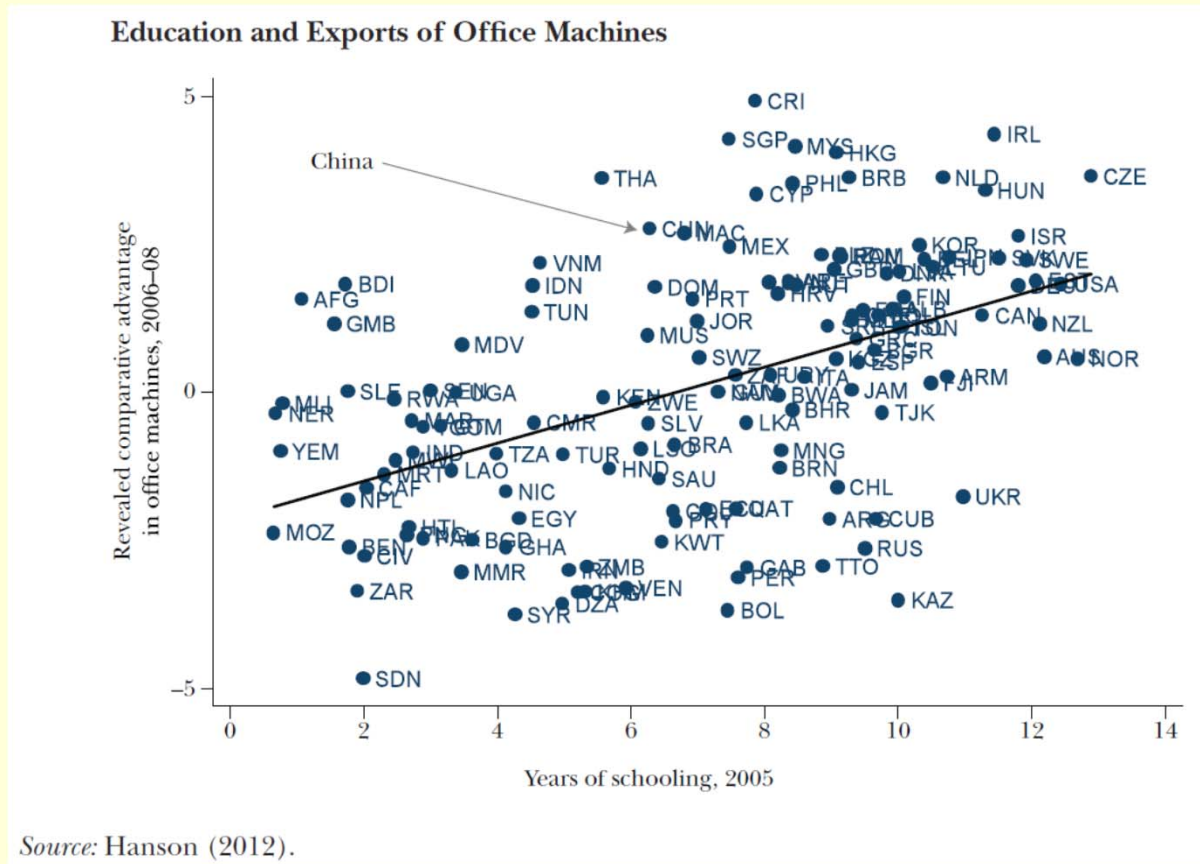
Source: Klerman and Danielson (2011).

Vs this graph...

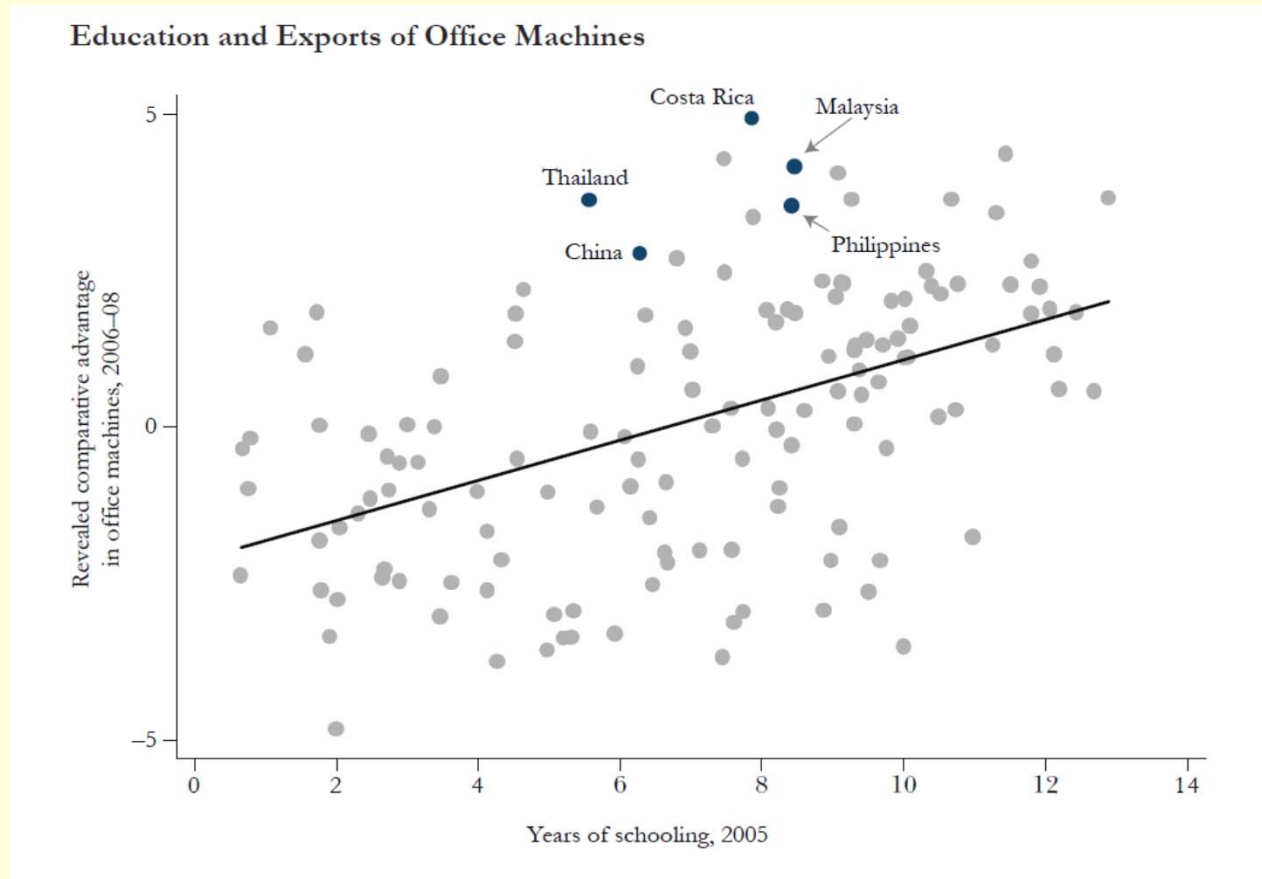
Implied Impulse Response Functions for Different Caseloads
(Percent change)



This graph...

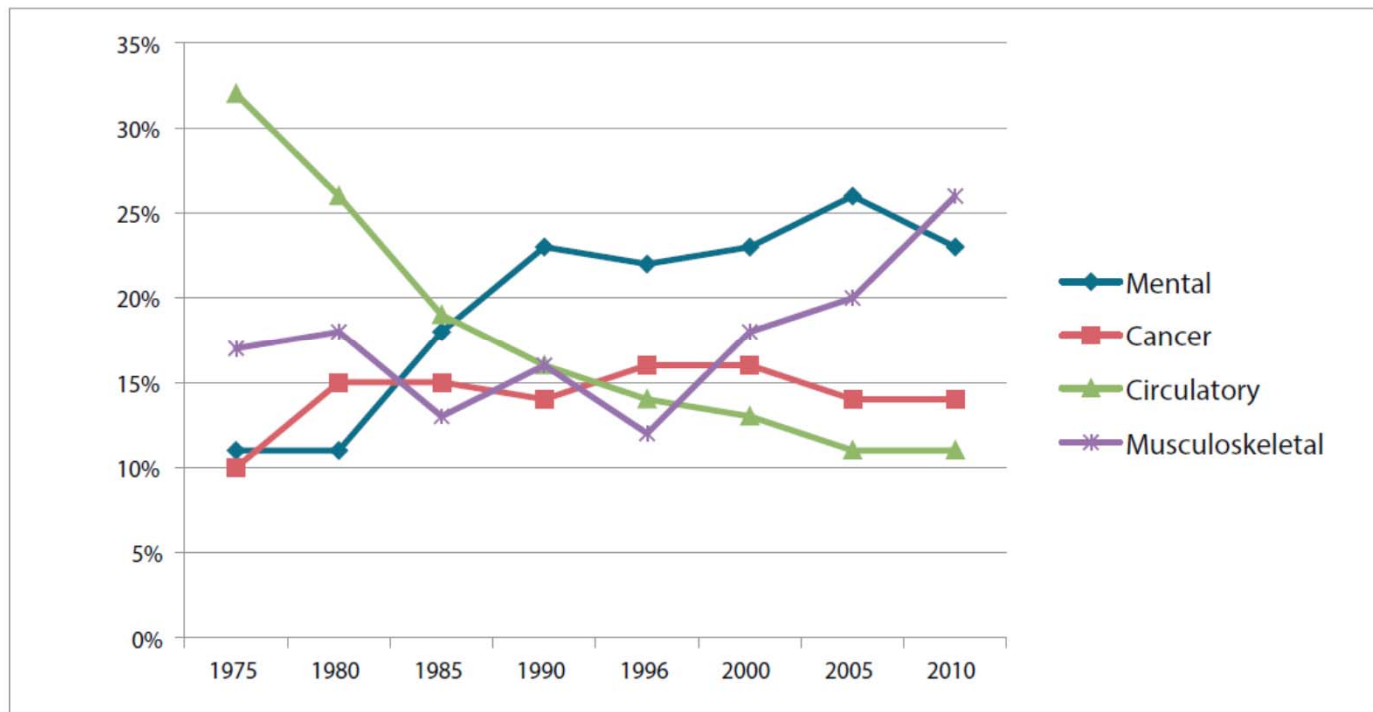


Vs. this graph...

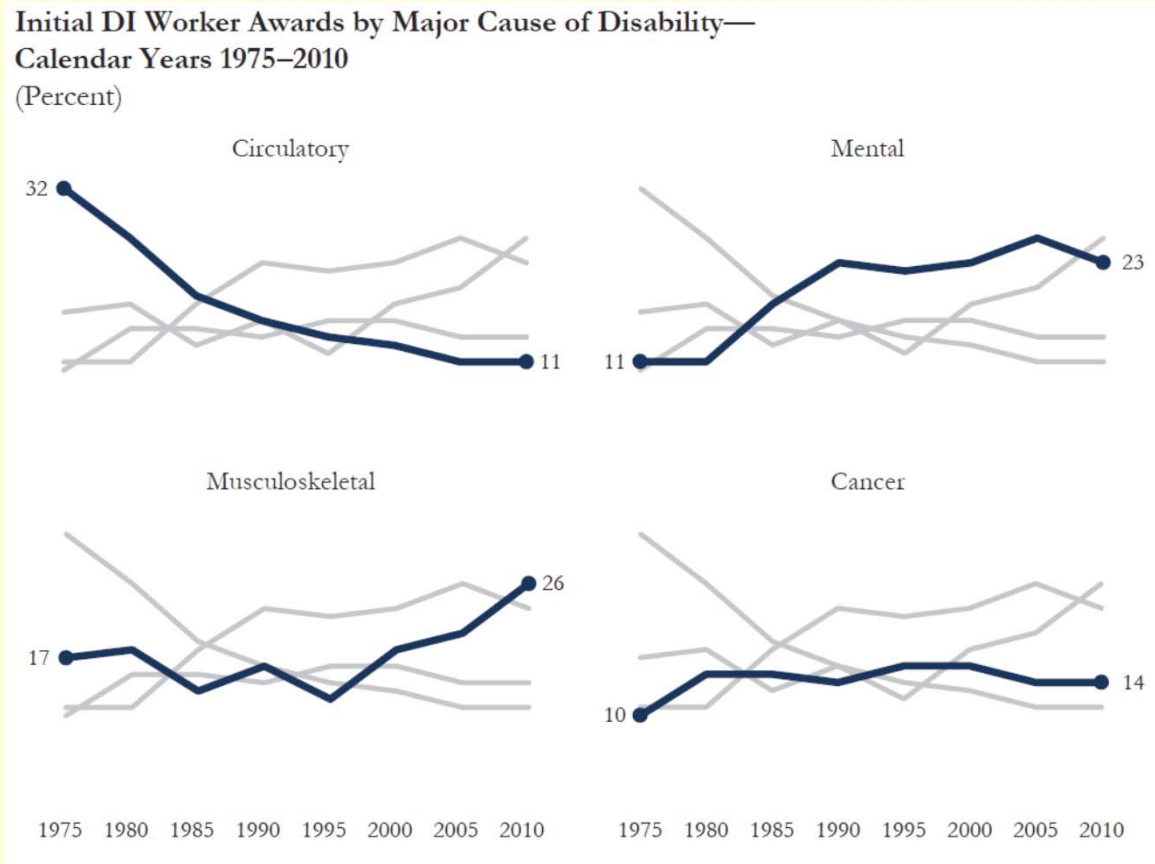


This graph...

27. Initial DI Worker Awards by Major Cause of Disability—Calendar Years 1975-2010



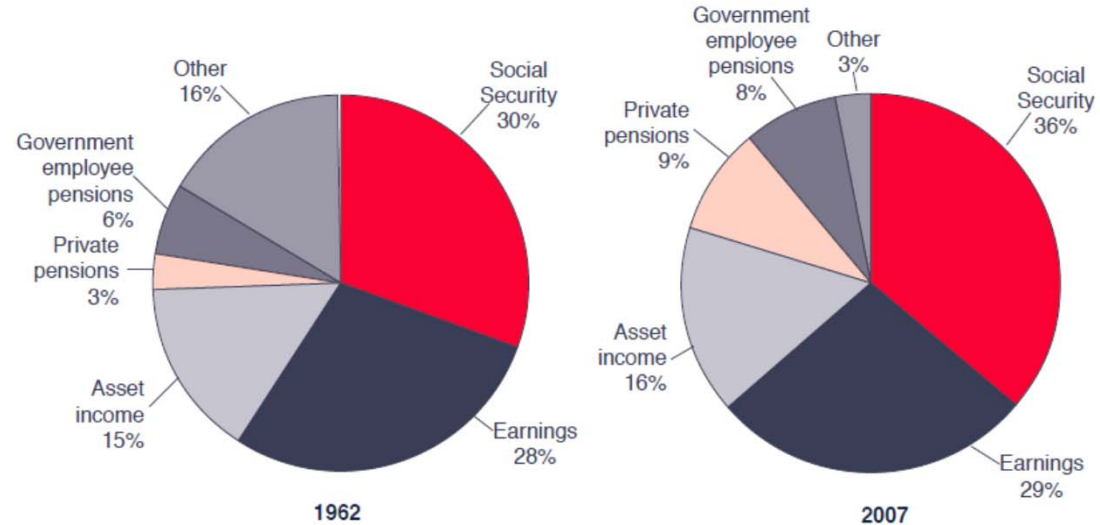
Vs. this graph...



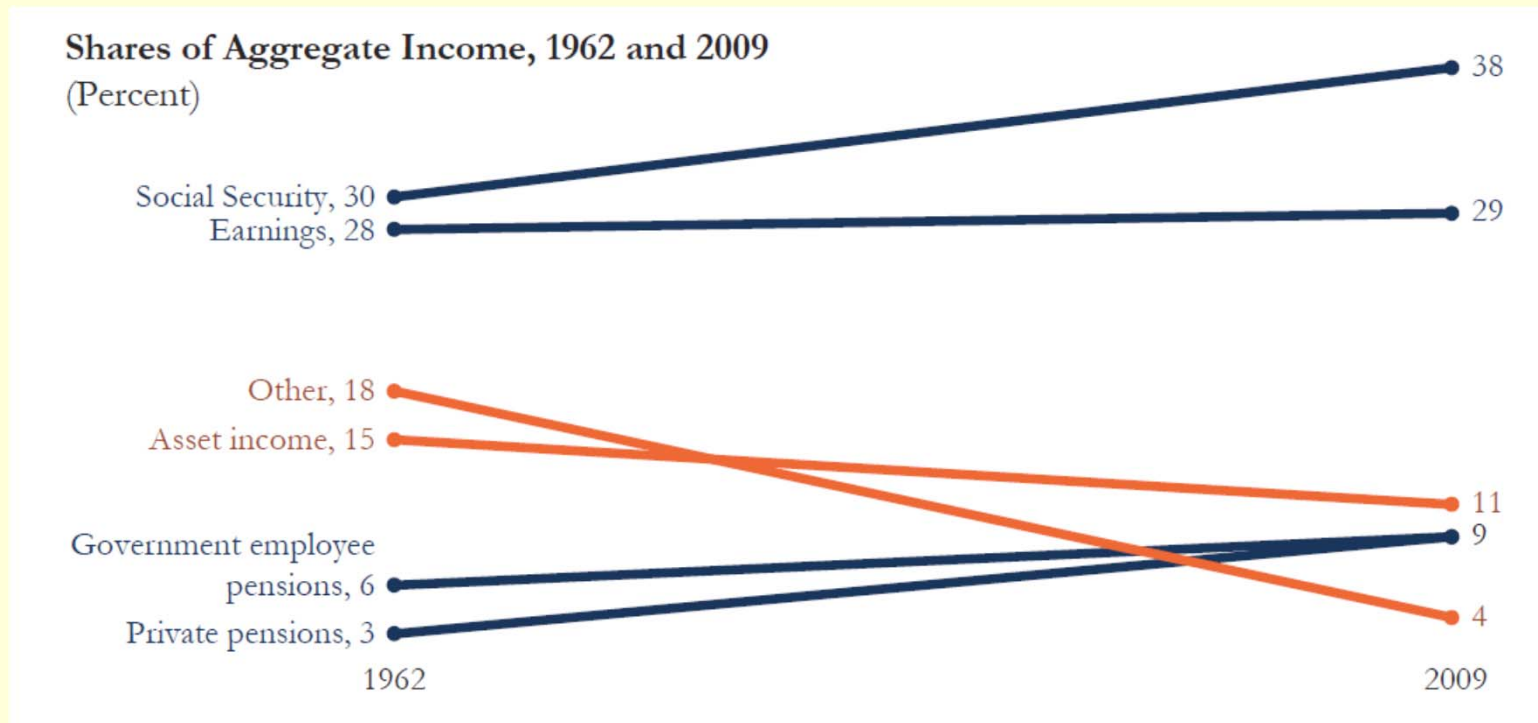
This graph...

Shares of Aggregate Income, 1962 and 2007

Aggregate income, by source



Vs. this graph..



Schwabish (JEP 2014) suggests or implies...

- Presentation matters, a lot.
- Effective visualization involves three principles:
 - show the data to *tell the economic story*
 - reduce clutter to keep the focus on the key points, the “story”.
 - integrate the text with the graphs to transfer information about the story quickly and without reference to the paper.
- All figures should include concise but clear descriptions so that they stand alone for someone that has not read the paper.

What about conclusions?

- Good conclusions vary a lot.
 - Brief summary
 - Interpretation exercises (put results in context)
 - Limitations and directions for future research
 - Implications for economics and policy
- Check out M. Bellemare's "The Conclusion Formula" (google it)

Other thoughts

- Nothing here is intended to diminish quality of methods, etc.
- Good papers (of course!) need to be reasonably correct too.
- For most applied economic papers, though, the quality of the question and contribution are the most important issue.
- The quality of the writing and presentation can be also the difference between several publication tiers.

Selected references and readings

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