Discussion of
Entrepreneurship and Information on Past Failures: A Natural Experiment

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What the paper does?

- Analyzes how public information on past failures affects entrepreneurs ex post (i.e. after failure) "outcomes" (restarting a business, ability to borrow)
- Exploits as a natural experiment a policy shock which eliminated a "bankruptcy flag" for managers that was easily accessible to banks
- Compares the outcomes of managers who were "deflagged" following the reform with those that were not (holding constant the time passed from the last failure) before
What the paper finds?

- Eliminating the flag:
  - make failed (and deflagged) managers more likely to restart a business
  - but these new firms have a higher probability of default
  - improve the credit conditions (quantity and costs) of new and existing (multi-firms) managers
  - notwithstanding the small cost for lenders of "reconstructing" the flag
I really like the paper...

- The topic is important and the paper suggests a relevant and novel policy implication.
- The data are very rich and the deflagging shock provides useful natural experiment.
- Even if preliminar, the paper:
  - carefully exploits a policy shock
  - is rich in results (almost too many!)
  - provides some interesting policy conclusions on the importance of the "framing" of public information and on how small costs of accessing information might prevent unnecessary stigmatization
... but I have to discuss it anyway

- Two main comments:
  - The contribution
  - The empirical framework (what are the empirical challenges? what are your solutions? what is the indentifying assumption?)

- Minor comments
The contribution I

- In the paper there are references to some paper that do similar empirical exercises (e.g. Dobbie et al., 2016, Bos and Nakamura, 2014, Bos et al., 2016) but little explanation of how this paper differs from them.

- Perhaps the most novel aspects is in the policy conclusions (the "policy implications" section at the end of the paper).

- Based on the results of the paper (that deflagging improves the ex-post outcomes of failed entrepreneurs) and on fact that the cost of retrieving the same information destroyed by the policy is small, the paper argues that small economic and cognitive costs to access information on past failures might reduce unnecessary stigmatization.

- However the policy section is also the more speculative one.
The paper is about the *ex post* effects of information about past failures. However the policy conclusions depend also on the *ex ante* effects (i.e. for not yet failed managers).

The natural experiment and the data might be used to address even the *ex ante* ones (maybe exploiting the sectoral heterogeneity in the importance of the entrepreneurial talent already used in the paper).

This would strengthen some of the arguments in the policy section.
The contribution III

• Furthermore the results have potential interesting, and not yet fully explored implications, for understanding bank behaviour. A deeper analysis of these implications, would be interesting *per se*, but also provide stronger support for the policy implications than the simple back of the envelope calculations in the paper so far.

• In this respect the paper would benefit from a more thorough discussion of the costs of acquiring the deflagged information, for bank and for managers. Maybe the bank organizational costs are bigger than the economic ones (e.g. the flag was an input of the internal rating model of the bank, ad the bank has to review the whole model after the policy shock). How easy is to recover the status, after the policy shock for a bank and for the manager?
The empirical framework

- The paper is a bit difficult to read. I would explain a bit more the way the policy experiment is exploited in the empirical analysis. This is not a "simple" DID framework (not sure it is a DID at all).

- The figures (e.g. Figure 5) are intuitive.
Figure 5

The diagram illustrates the probability to start a business over years from the start of the flag. The x-axis represents years from the start of the flag, ranging from -1 to 5. The y-axis shows the probability to start a business, ranging from 0.35 to 0.06.

Key events include:
- **PRE-FAILURE**: A peak in the probability around year 0.
- **DEFLAGGED**: A notable decrease in the probability around year 1.
- **NO DEFLAGGED**: An increase in the probability starting around year 2.

The graph highlights three policy scenarios:
- **Policy: one-year flags** (dotted line).
- **Policy: two-year flags** (dashed line).
- **Three-year flags** (solid line).

The diagram underscores the impact of deflagging and the variation in start-up probabilities under different policy durations.
The empirical framework

• The figure compares the outcome (e.g. starting a new firm after a default) of "old regime" failed managers (those deflagged after 3 years) with that of managers that, due to the exogenous policy shock are deflagged earlier (after either one or two years).

• It clearly shows that the probability of starting a new firm increases after deflagging.

• However there is no role here for entrepreneurs that never failed.
The regression counterparts

• There are two regressions (at the manager-quarter level).

• The first where the probability of new firm creation in quarter $t$ by manager $i$ is explained by a failure dummy ($=1$ if $i$ ever failed) and a deflag dummy ($=1$ if $i$ is deflagged in $t$) which is only after 3 years (old regime)

• This regression is less interesting and not closely related to the policy experiment, but mostly to the differences between failed and non failed managers.

• Here the results are kind of strange, in the sense that failed managers are more likely to start a new firm than never failed ones. I would experiment with the concept of "firm creation", like for example restricting the firm creation of non-failed managers to those that were not managing any firm in the previous year.
The main regression

- In the second regression (which replicate the figure) the probability of new firm creation is explained by:
  - a set of dummies capturing time since failure (one for each quarter) which capture the standard dynamics of pre-reform failed entrepreneurs (who were always flagged during the 11 quarter periods following a failure)
  - the deflag dummy (=1 if \(i\) is deflagged in \(t\)) which now is randomly determined by the time passed from the manager’ last (and only, because of sample selection) failure at the moment the policy shock happen (September 2013)
  - the coefficient of this last dummy captures the differences of being deflagged early with respect to not being deflagged yet
I would expect this regression to be estimated on the sample of failed entrepreneurs only, where the interesting variation is in the exogenous, due to the policy experiment, date of deflagging.

However this second regression seems (looking at sample size) to be estimated using also the sample of managers who never failed.

- How? (i.e. what is the "failure date" for these managers? how do you create a dummy quarter since failure or these managers?)
- Why? What do they add to the results? (one thing they do is to increase the sample size)
- What happens if this model is estimated using failed managers only?
The manager dataset starts in 2005 long before the policy: I would be worried that in 2005 the firm creation dynamics of failed entrepreneurs is rather different from 2010. I would check what happens if we exclude the early years (at least excluding those before the crisis).

The manager dataset ends in 2015q4. How do you treat the fact that for several managers there are not the full 11 quarters post-failure? (should you?)
Other minor comments: summing up

• I would expand the data descriptive section, to give information about: what exactly managers are, how exogenous the shock was, what are the trend of firm creation and bankruptcies over the period, how many multi-firms managers there are and how do they differ from the others ...

• Explore possible heterogeneity in the results, for example according to the type of managers or legal form of the firms, whether the multi-firm managers operate in the same sector or in different ones...
Other minor comments: I

- From Figure 5 it seems that, even before the failure (at time 0), the probability to start a new firm is higher after the policy ("policy" lines) than before (three years flag line). How do you take that into account?

- What is a "manager"? Do you have information on what role the manager had in the failed firms and in the new one? There might be interesting differences in the results depending on what the manager actually do in the firm. Have you tried to look at them?
Other minor comments: II

- In the second part of the paper (the "intensive margin" part) you look at the effects of a manager failure on the outcomes of other firms she is managing. It would be interesting to know more facts about these multi-firm managers: how many there are and whether they are different (both in terms of what they do and how often they fail) with respect to the single firm managers.

- In this case it would be interesting to know whether the effects you find are different if the other firms are in the same sector or in different sectors of the failed one.
Other minor comments: III

- The section on whether the probability of failure depends on past failure and on whether the manager is flagged, is potentially interesting but not very clear. In particular I would focus on just one very specific event (e.g. firm does not survive past the first year).