Von:	"Journal of Political Economy" <em@editorialmanager.com>
An:	"Rudolf Winter-Ebmer" <rudolf.winterebmer@jku.at>
Datum:	06.09.2023 22:33
Betreff:	Your JPE Submission

CC: melissadell@fas.harvard.edu

Ref.: MS 20230334
Banking on Snow: Bank Capital, Risk, and Employment
Journal of Political Economy
Sep 06, 2023

Dear Professor Rudolf Winter-Ebmer,

Thank you for submitting your paper, "Banking on Snow: Bank Capital, Risk, and Employment" (MS 20230334), to the Journal of Political Economy. First, I apologize for the time elapsed. I have been promised a report for some time by a referee, but at this point have given up on receiving it. Fortunately, I do have a very detailed report from a knowledgeable referee, and have also carefully read the study myself.

Both the referee and I found a lot to like about the study and its fascinating setting, but also have some doubts about the interpretation. Overall, my reading aligns with that of the referee. First, for this sort of question, one would ideally have firm balance sheet or credit registry data, to more directly test the hypothesis. Without such data, the interpretation relies upon what is essentially a reduced form relationship with a highly aggregate bank shock. The referee discusses in detail potential confounders like impacts on capital investment. Another potential concern is that the employment days outcome is a potentially noisy measure of the wage bill. The referee also raises concerns about external validity.

Overall, it is a really fascinating setting – and the study may well have done the best it possibly can within the data constraints – but the setting also lacks the rich data that would be needed to fully nail the question. The JPE is able to accept fewer than 5% of submissions, a very high bar, and regrettably with the data constraints I don't think this study can clear this bar. I am sorry that we will not be able to publish the paper, and hope that this will not dissuade you from submitting your work to the JPE in the future.

Sincerely,
Melissa Dell
Editor
_____

Reviewer #1: ##Summary of paper:
This paper argues that firms rely on banks in part to provide employment "insurance" to their workers by offering more fixed employment contracts, and that bank capitalization positively impacts this channel. The setting for identification in the paper is ski resorts in Austria. The idea is that when ski resorts face more weather-related demand shocks, the labor productivity of their workers goes down, but firms could insure their workers through these spells by providing more guaranteed employment via bank financing. Then the friction that banks face is the extent to which they can lend due to their capital constraints. The paper uses an instrument for bank capital to provide a causal estimate of the relationship between financial constraints and employment. The setting is clever, and even though the paper does not appear to have firm-level balance sheets or credit registry data that would provide more direct evidence of this mechanism, the mechanism itself is overall
plausible and interesting to analyze.

##Major comments:
1. Interpretation: The paper argues that bank equity impacts ski resorts through firms' ability to raise bank financing that would hedge labor productivity (i.e. demand shocks) and to maintain higher employment even when the risk of demand shocks is higher. Since there is no credit registry data, it is not possible to see directly whether there are actually loans being contracted. The paper needs to infer whether there is this relationship based on the reduced form relationship between employment (a real outcome of the firm) and bank equity shocks. However, there is an additional, very

important role for bank financing, which is in capital investment. Ski resorts in particular are capital-intensive, and I would imagine that there is a significant amount of funding that goes into maintaining that capital each year. It would seem that there would be a pretty standard production function between capital and labor for a resort, and it would be hard for resorts to deviate too

much from it (for example, a chairlift needs to be manned by X number of people; if the chairlift is undergoing repairs, then X fewer people will be needed, regardless of demand). Since the relationship between bank financing and physical capital is so well-established in the literature as having a first-order effect, it would be important to show that the relationship between bank equity and employment goes *beyond* just what would be expected from adjusting on the capital side. From an identification perspective, it may be the case that the firm-year fixed effects are residualizing the capital effect, but conceptually, I think the paper would be stronger if it could speak a bit more to exactly how readers should think about the capital-labor optimization and the role of bank equity in providing capital.

   - Firms can provide employment "insurance" either by offering more employment days or by offering higher wages. For example, a resort that very flexibly adjusts its count of employee hours would look like one that is not providing much insurance, but perhaps it is precisely *only* able to adjust on the hours of employment because it pays higher wages per hour. This equivalence is what would make workers indifferent ex-ante. Given that much of the action is coming from tight labor markets, it seems that resorts would need to compete for labor. Is this a possibility that can be addressed directly with the social security data?

2. Empirical specification:
   - I would like more exposition on the baseline regression (equation 1) that estimates the relationship between snow risk and employment, going through the channel of bank equity. Even though this is not the instrumented regression, the estimates it produces are in magnitudes very similar to the 2SLS so it would be helpful to understand it better.
      - What is in the vector of controls Z? Since the main coefficient of interest is based on the interaction term of the Ski-Resort (SR) risk interacted with Bank-Equity (BE) measured regionally, I'd like to know how the BE term enters into the estimation. Is $BE_{\{j(i),t\}}$ in the Z vector, and if so, is it interacted with τ fixed effects so that it's allowed to have varying effects every week? or is it absorbed in the $\alpha_{\{i,t\}}$ fixed effect? Does it matter empirically? The tables for the baseline effects do not describe the control variables nor adds them in individually.
      - How much variation is there actually in both the SR and BE variables? I would recommend plotting them in a histogram. I'm skeptical that the either variable has much time variation because any change over time would be slow-moving. That suggests that the identifying variation is based on regional variation (which is fine), but it would be helpful to understand the source of the variation. If indeed the variation in SR (after absorbing the weekly variation in overall risk) is regional, a map to visualize it would be helpful so that it is clear whether it might be useful to have region x year fixed effects that absorb, for example, regional labor policies that may impact employment.
   - Instrument: the instrument is based on the idea that individual banks play a small role in the overall equity in a "group" of banks (of which there are 3 types of these groups). This intuition is similar to a shift-share based on shares where each individual "share" is unlikely to generate the overall variation in equity at the group level. The work on Bartik instruments on shares provides strategies for assessing whether that is actually the case, in particular using Rotemberg weights.
      - I am unclear on the number of banking groups used for the instrument. The text says there are 3 different groups —are those 3 groups the ultimate source of variation? If so, that is only 3 internal capital markets. This means that the actual instrument is essentially a different shift-share where there are 3 "shocks" (time-varying BE across bank groups) interacted with municipality-level shares. In this setting shares are going to be very static and also conceptually very unlikely to be random. The ultimate source of variation would be coming from the "shocks". 3 is generally considered to be too small for inference.
      - Is there any additional information about the bank branch network other than the existence of a branch in any given place? If so, it would be helpful to see the BE variable constructed with other measures of shares.
   - Measurement: Is the SR variable sensitive to the 5 year measurement cut-off? How much unregistered employment is there likely to be? The outcome variable is measured using Austrian social security data; is it likely/possible for these resorts (especially later on in the sample) to hire temporary workers from other countries that are not registered in the SS database?

##Minor comments:
- Institutional detail of hotel bookings: I was unclear about whether the footnote (33) about when booking.com was introduced to Austria addressed the concern that resorts can differentially price. Especially if the hotel is operating its own website (or operators), it seems even easier to update prices than in a third-party system. It would be good to have direct evidence of the prices, and to show that there are vacancies because prices did not adjust sufficiently.

- Organization: I found it a little strange that the empirical strategy was discussed before the data. It was harder to follow the former section without understanding the structure of the data.
- I found Figure 1 a bit hard to follow; in particular, arrows generally indicate causality, and that does not appear to be the case in the figure.
- External validity: while I am sympathetic that some empirical settings are useful for identification, and I appreciate that the paper explicitly draws connections to theory that helps to make the mapping between this setting to a broader economic phenomenon, I think the paper should do more to discuss how to extrapolate from Austrian ski tourism. In particular, this setting may be reasonable for thinking about seasonal workers broadly, but it is not clear that it extrapolates to discussing family-owned firms generally speaking. In order to address that, the authors could use the social security database to provide some summary statistics on the % of overall employment that looks like seasonal spells, the % of family firms that employ seasonal workers, the % of the overall employment that family firms actually explain, etc. These statistics would be helpful to believe that this paper is really about small firms making a big difference in the aggregate economy as opposed to
seasonal workers in a specific industry being exposed to more frictions.

_____

_____

Revision of "Banking on Snow"

Simon Baumgartner, Jakob Conradi, Alex Stomper,
Thomas Schober and Rudolf Winter-Ebmer

This document explains how the revision of our paper addresses concerns described in a referee report we obtained from the Journal of Political Economy (JPE). This report was based on the previous version of our paper, written in 2023.

The revision focused on four major issues:

1. <u>Interpretation</u> of our main result and the need for <u>firm-level balance sheet data</u> or credit registry data. We managed to obtain suitable data, so as to address a question of the JPE editor (Melissa Dell) and the referee: "whether there are actually loans being contracted" when firms have to cope with weather-induced shocks.

   The revised version documents this, using (slightly aggregated) firm-level balance sheet data we managed to obtain from the Austrian Tourism Bank. See the bar charts in Figure 1 and, in particular, the bar chart in the lower left part of the picture, showing changes in short-term bank debt. This bar chart documents that we see a pattern of firms borrowing from banks when they have to cope with unfavorable weather shocks (Negative Unexpected Snow, UES), but we only see this pattern in municipalities with above-median levels of bank equity capitalization (High BE).

   Similar evidence comes from an analysis of firms' interest expenses (depicted in the bar chart in the upper right part of Figure 1).

   We also use balance sheet data of banks. These data cover the member banks of three Austrian banking groups. To avoid within-group competition, the banking groups' members focus their operations on different areas in Austria, with branch networks limited to these regions. For the banks in Austrian ski resorts, the local tourism industry will typically be their main source of business and, in particular, interest income. Our data for the banks will, however, also include transactions with customers outside this industry.

   This caveat notwithstanding, our analysis of bank interest income confirms the pattern we see regarding firms' interest expenses. This addresses a concern that the firm-level data are only available for a selected sample (- the customers of the Austrian Tourism Bank, our data source). Given that we see similar patterns in both types of balance sheet data, we can conclude that the results regarding banks' interest income growth are indeed driven by their lending to ski tourism businesses.

2. Both the editor and the referee mention that bank capitalization could affect employment due to effects on <u>capital investment</u>. For example, the referee writes that "it would be important to show that the relationship between bank equity and employment goes *beyond* just what would be expected from adjusting on the capital side".

We believe that our paper indeed documents an effect of bank capitalization on employment which is unrelated to investment. This is possible because our main evidence comes from weekly variation in employment, so that firm-year fixed effects can be used to control for effects of investment. In the revised version of the paper, this is discussed in the second paragraph below expression (1).

Figure 3 shows the size of the effect we document and allows for comparing it to baseline differences in the employment of firms operating in areas with more or less bank equity capital. These baseline differences include variation in employment caused by effects of bank capitalization on investment.

3. <u>External validity</u>: The revised version addresses the referee's suggestion that "the paper should do more to discuss how to extrapolate from Austrian ski tourism".

    The first paragraph of the section on related literature points out that, with its main result being about quasi-fixed employment, our paper provides evidence regarding the micro-foundations of the model of Arellano et al. (2018). One problem in bringing models of risk-shocks to data is that it is usually hard to tell at which points in time firms respond to changes in labor productivity risk. We solve this problem by focusing on firms with a seasonal business model. These firms respond to changes in labor productivity risk (induced by changes in snow risk) at a known point in time: when they hire workers prior to the start of the season.

    Our results should generalize to family firms offering quasi-fixed employment, irrespective of whether this employment is seasonal. This is discussed in the final part of the concluding section. Another point regarding external validity appears in the opening paragraph of the paper. There, we point out that it is common for employees to receive insurance from employers against the risk of transitory shocks to labor productivity. We make sure that we analyze this sort of risk by focusing on snow risk during the starting weeks of the ski season, when a lack of snow in ski resorts is almost certain to be a transitory problem. See the third paragraph of the introduction.

4. The editor raises the concern that our measure of employment is a potentially noisy measure of the <u>wage bill</u>. A related point appears in the referee report which points out that we analyze an employment which may appear to be quasi-fixed, but we may be missing risk-sharing between employers and employees in terms of hours worked. The referee asks whether "this is a possibility that can be addressed directly with the social security data?'

    In revising the paper, we realized that, for the starting weeks of the ski season, we can actually use the social security data to measure the average daily wages that many ski tourism employees earn during these weeks. (This works for all employees whose employment spell during these weeks is unique in the sense that there is no other employment spell for the same triple of employee, employer and year.) The revised version of our paper presents wage regressions testing for effects of snow shocks and snow risk. The regressions yield little evidence that wages respond to snow shocks as a proxy for hours worked/overtime pay earned by employees

(Table A.2). Moreover, we find no evidence that wages depend on snow risk (Table A.3). This is relevant because the referee report speculates about workers earning "risk premia" in wages.

We do, however, observe some evidence that, in areas with relatively little bank equity capital, employers engage in some risk-sharing with employees. (Table A.2, final column) The evidence actually supports our argument for why bank capitalization affects the sensitivity of employment to labor productivity risk: We observe that, in areas with relatively little bank equity capital, employment levels respond more strongly to labor productivity risk and the employment is less "fixed" in that employees' wages respond more strongly to snow shocks. *Both* observations are consistent with the argument that bank capitalization affects the risk-sharing between banks and employers, so that less risk-sharing occurs when banks have less equity capital.

Questions regarding the empirical specification:

1. Exposition: The revision contains an extended discussion of the baseline OLS results. See the paragraphs below expression (1) which explain the regression mechanics. The baseline effect of Bank Equity (BE) is absorbed by the firm-year fixed effects.

2. Control variables: The specification of the vector of control variables (Z) varies across the regressions. Table 4 states the various specifications by listing all control variables.

   The referee suggests that we allow the baseline effect of BE to vary across calendar weeks. This results in a specification in which the firm-year fixed effects only absorb the effect of BE for a particular "reference" week. This specification is reported in the final two columns of Table 4. The reference week is week 1.

3. The referee report asks us to document the variation in both our measure of Snow Risk (SR) and Bank Equity (BE) and to discuss the sources of the variation. The revised version contains maps showing changes in SR which can be interpreted as effects of global warming in the Austrian Alps. We show different maps for ski resorts with different levels of BE to address a potential concern that the results come from an unbalanced type of variation.

   The revised version also includes a discussion of a likely source of variation in BE, - a massive process of FDI of Austrian banks in Central and Eastern Europe. See the discussion of Figure A.4 on page 19.

4. Instrument: The referee report points out that our instrument is of the shift-share type and asks about the number of "shifts" we use.

   The referee's questions are clearly relevant for estimating the baseline effect of BE on employment, but this is not our focus. Given that we want to measure the effects of labor productivity risk, our shift-share measures of BE typically appear in interactions with SR as a driver of labor productivity risk in ski tourism. Our estimates

therefore result from variation in SR while we use BE to test for differences in the effects of this variation on firms' employment.

The Online Appendix includes an analysis showing that there indeed were substantial trends in SR, as well as variation in the trends across ski resorts at different levels of altitude. The most relevant estimate for this variation appears in column 7 of Table A.1 in the Online Appendix. This estimate concerns the weeks of the ski season during which employment in ski tourism is quasi-fixed. For these weeks (- the ski seasons starting weeks, for which Start=1) we see an increase in SR by roughly 0.454 per 1000 meters of altitude. This is substantial relative to the mean SR (of roughly 0.1).

Our instrument essentially compares firms in different ski resorts in terms of their response to changes in SR. The referee report asks about the number of banking groups behind our comparison of ski resorts. While there are many examples for studies testing for differences in some variation across few groups (- the canonical example being a DiD with a treatment and control group), the referee's questions led us to rethink our approach. Our instrument now results from aggregated data about bank capitalization for 21 groups of banks. For a discussion of the rationale behind our approach, see page 14.

The referee report also asks about extending our data about bank branch networks with other information about banks' operations in different ski resort municipalities. We, however, have no additional information.

5. Measurement: The Online Appendix reports a robustness check in which we follow the referee's suggestion to measure SR based on alternatives to the 5 year cut-off. See Table A.5.

   The referee report also asks about the reliability of Austrian social security data for measuring employment in ski tourism. The data are reliable because the needs of the Austrian tourism industry shaped Austria's labor laws (due to the importance of the industry for Austria's GDP). This is particularly true with respect to the industry's hiring of foreign workers, which is regulated through a special provision in the law, the so-called Saisonnier-Regelung. We can, therefore, trust that our data give fairly complete information about employment in Austrian ski tourism. Among EU countries, Austria is the country with the smallest business revenue from unregistered employment relative to GDP.