Revision of "Banking on Snow"

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

This document describes a revision in response to a referee report we received from the Journal of Political Economy (JPE). The revision included a substantial extension of the paper (described below) and also involved a new coauthor, Jakob Conradi. Below, we refer to the resulting paper as our new paper. It replaces Banking on Snow. We also changed the title to succinctly describe the paper's contribution: evidence for a multiplier relevant for macro-finance and bank regulation.

We did not again submit to the JPE, but we want to honor the valuable input we received. This is the point of the following report.

The revision focused on four major issues:
1. Interpretation of our main result and the need for firm-level balance sheet data or credit registry data.

We managed to obtain suitable data, so as to address a question of the JPE editor (Melissa Dell) and referee: "Whether there are actually loans being contracted" when ski tourism businesses have to cope with weather-induced shocks. We view this as the central unresolved issue leading to "Banking on Snow" being rejected by the JPE.

The new paper answers the question by documenting the effects of weather shocks on the balance sheets of tourism businesses and banks in Austrian ski resort areas. The bank balance sheet data are publicly available data, published by the Austrian Central Bank. To cover the need for balance sheet data on ski tourism firms, we use data from the Austrian Tourism Bank (ATB) (with some aggregation due to banking secrecy requirements).

The latter data come from a selected sample (customers of the ATB), so that we only use them in an exploratory analysis in the first part of the new paper. See Figure 1[1] where the bar chart in the lower left part shows changes in short-term bank debt. This bar chart documents that we actually see 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). The short-term borrowing appears to be driven by banks deferring interest payments, as discussed in Section 2.3 of the new paper and illustrated in Figure 1. We believe that the latter evidence is strong evidence that banks are addressing weather-induced problems of financial distress of ski tourism businesses because interest expenses would (otherwise) be largely pre-determined by past borrowing and cannot be reduced without banks' consent.

A complementary analysis appears in the new paper's Section 6.1. This analysis addresses two concerns: That the analysis in Section 2.3 is based on a selected sample and that we have no data about actual transactions between firms and

[1] The old paper's Figure 1 (- a causal graph mentioned in the referee report) was removed because the referee found it hard to follow.

banks. The analysis in Section 6.1 uses data about the population of regional banks close to Austrian ski resorts in rural areas. These banks all rely heavily on the tourism industry for interest income, often as their main source. We use balance sheet data on these banks to analyze effects of weather shocks on bank interest income growth, with a focus on the shocks behind the evidence (Figure 1) coming from the balance sheet data we obtained from the Austrian Tourism Bank.

Both types of balance sheet data yield consistent evidence that banks in Austrian ski resort share a risk of snow shocks with the resorts' tourism businesses. In both cases, the evidence comes from regions with strongly capitalized banks. Given that we see consistent evidence in balance sheet data of, both, banks and tourism businesses in these regions' ski resorts, it is highly likely that the evidence actually comes from transactions between these banks and firms. This is reassuring, given that we can neither directly observe the transactions nor the actual extent of the banks' interest income from lending to tourism businesses (rather than to other borrowers).

2. Both the editor and the referee mention that bank capitalization could affect employment due to effects on capital investment. 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 documents an effect of bank capitalization on employment which is unrelated to investment. Our main evidence comes from weekly variation in employment, so that firm-year fixed effects can be used to control for effects of investment (which was actually acknowledged in the referee report). In the new paper, this is discussed in the first paragraph of the second page of introduction and in the second paragraph below expression (1).

Figure 2 of the new paper 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. It turns out that, in regions with more strongly capitalized banks, we observe a somewhat *smaller* level of baseline employment (which can be seen in Figure 2 in that the dashed line is below the solid line).

This is surprising. Similar evidence, however, comes from the balance sheet data we obtained from the Austrian Tourism Bank. The data allow us to measure correlations between our measures of bank capitalization and key firm characteristics of ski tourism businesses in Austrian ski resorts. We report these correlations in the final paragraph of Section 2.3 of the new paper. If capital investment in ski tourism were driven by bank capitalization, we should see substantial correlations with firm size or leverage. We, however, do *not* see these correlations in the data. In fact, the correlation between bank capitalization and firm size is somewhat negative (-0.097).

In summary, we see little evidence for the capital investment channel mentioned in the referee report. The reason may be that we study a rather mature industry. We, therefore, focus our paper on the effects of bank capitalization we can actually see in the data: that bank capitalization affects the sensitivity of employment to weather-induced labor productivity shocks.

3. External validity: The new paper implements the referee's suggestion that "the paper should do more to discuss how to extrapolate from Austrian ski tourism".

We approach this issue in three ways. The first way is based on strong evidence that, in developed countries, employment is commonly quasi-fixed with respect to transitory shocks to labor productivity. This evidence implies that, by focusing on a risk of shocks which are clearly transitory (weather-induced demand shocks in ski tourism at the start of the ski season), we analyze a *type* of risk that causes losses in many firms. We stress this point in the opening paragraph of the introductory section.

Moreover, we now explain much better that the need for analyzing a risk of transitory labor productivity shocks drives our focus on ski tourism: The third paragraph of the introductory section points out that our research question requires a focus on a setting in which we can be certain that we measure effects of a risk of transitory labor productivity shocks while also being able to measure this risk. The transitory nature of the shocks is discussed in the next-to-last paragraph of Section 2.1.

Our second way of approaching the issue is by pointing out that our paper contributes targeted empirical evidence supporting key assumptions underlying widely influential theoretical findings in macroeconomics. The setting we analyze can be seen as a real-world version of the microfoundations of Arellano et al. (2019). In bringing this model to data, one must be able to tell which information was available to firms when they pick a level of quasi-fixed employment. We solve this problem by focusing on an industry with a seasonal business model. This allows us to distinguish between information firms have before picking employment levels ("ex ante") and news they learn thereafter. (See Table 3.) Why is this setting likely to generate evidence of broader relevance? Our argument is that it closely resembles the stylized setting analyzed by Arellano et al. (2019), whose model helps explain key features of the Great Recession. This is discussed in the final paragraph of the introductory section.

The third way in which we address the issue of external validity is head-on. At the end of Section 2.1, we cite Del Bono and Weber (2008) to point out that seasonal employment actually accounts for a substantial part of many countries' overall employment. Further arguments of this sort appear in the concluding section's last paragraph, where we discuss the importance of small-firm employment in high-income economies and also argue that our paper contributes to a better understanding of banks' role in economic adaptation to weather risk as a type of risk which is also widely relevant beyond the setting we focus on.

In summary, our results should generalize in the sense that we document an effect of bank capitalization on quasi-fixed employment in family firms which should also affect non-seasonal businesses. This type of employment is very common. By studying it, we can therefore understand the model of Arellano et al. (2019) concerning the macro-economic effects of risk and frictions in risk-sharing between employers and external financiers. We document such financial frictions, with a focus on small firms for which bank credit is the main source of external finance. To point out that this focus is relevant, we cite Bernanke (1983) in the concluding paragraph.

The referee asked us to address the concern about external validity using Austrian social security data. We instead opted for a broader approach because we wanted to *also* address potential concerns about the Austrian economy being "special", given that (ski) tourism contributes rather heavily to this economy.

4. The editor raises the concern that our measure of employment is a potentially noisy measure of the wage bill. 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 speculates that we may be seeing risk premia in wages and asks whether "this is a possibility that can be addressed directly with the social security data?

In revising "Banking on Snow", we realized that, for the starting weeks of the ski season, we can actually use Austrian social security data to construct a measure of the average daily wages earned by service personnel in ski tourism during these weeks. (This works for all employees whose employment spells during these weeks are unique in the sense that no other employment spell features the same triple of employee, employer and year. See the discussion surrounding expression (3) of the new paper which presents our wage measure. Below this expression, we also speculate about the bias from focusing on the unique employment spells; this focus should actually bias our analysis towards finding effects of snow shocks on wages. This is, however, *not* what we find in a regression testing for effects of snow shocks and snow risk. The regression yields no evidence that wages respond to snow shocks as a proxy for hours worked/overtime pay earned by employees. See the estimates reported in the last column of Table 3. In discussing these estimates, we also address the possibility that we observe "risk premia" in wages. See the final paragraphs of Section 5.1 of the new paper.

The main take-away of the wage regression is (further) evidence that Austrian ski tourism businesses insure their service personnel against snow shocks during the starting weeks of the ski season: they indeed offer a quasi-fixed employment. Of course, the quasi-fixity of employment relationships is always a matter of definition since the "relationships" are multi-faceted and also involve working conditions, etc. With the wage data, the new paper substantially extends our definition of employment quasi-fixity to include not only days of employment (- the focus of "Banking on Snow"), but also daily wages. We have no data about hours worked or working conditions.

Questions regarding the empirical specification:

1. Exposition: The new paper contains a comprehensive discussion of our empirical strategy and the OLS estimates. Moreover, Section 5 ends with a new discussion contrasting the OLS and IV estimates.

2. Control variables: The last sentence of the paragraph below expression (1) of the new paper specifies the set of control variables we use as a baseline specification. All our tables present a complete set of estimates, including 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 as a robustness check 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 Online Appendix of the new paper contains maps showing changes in SR in Austrian ski resorts. These maps are discussed in Section 3, in the last paragraph before the start of our discussion of "Identification". They illustrate that our data are well-balanced in the sense that we see all 4 combinations of below-/above median bank capitalization and changes in snow risk.

Are the changes in SR sizable? We explore this question in Table A.1 of the Online Appendix, which reports regressions 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 (roughly 0.1).

The variation in SR over time adds important statistical power to our analysis. This can be seen in a new robustness check in which we shut down this variation and measure the coefficient of the interaction of SR and bank capitalization (BE) by only using the latter variable's variation across years. The robustness check appears in the third and fourth column of Table 4. It shows that we do *not* need the variation over time in SR to robustly measure the coefficient of SR x BE (- our main result), but we do see some instability in the baseline coefficient of SR when we remove this variation. The robustness check is discussed in the first paragraph of the second part of Section 3, titled "Identification". This discussion also explains our original motivation for the robustness check: to check whether our results really come from snow risk, rather than from longer-lasting effects of snow shocks during our sample period.

The new paper also includes a discussion of a key source of variation in our instrument for bank capitalization: a massive process of FDI of Austrian banks in Central and Eastern Europe. See the discussion in the last paragraph of Section 4. The Online Appendix shows a plot of the instrument's year-on-year changes (basis points of banks' equity ratios) averaged across municipalities. These changes are sizable because the summary statistics show that many banks must have been rather close to the regulatory minimum capital (800 basis points).

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 bank capitalization (BE) on employment, but this is not our focus. Given that we want to measure effects of labor productivity risk, our shift-share measures of BE appear in

interactions with SR as an exogenous driver of labor productivity risk in ski tourism.[2] Our key estimates result from variation in SR while we use BE to test for differences in the effects of this variation on firms' employment. Our regressions compare 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 instrument. The new paper states the answer in the discussion below expression (9): 21 groups. For a discussion of the rationale behind the way we construct our instrument, see the final paragraphs of Section 3.

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 look-back period we use in our main specification. See Table A.4. The table also reports a robustness check in which we change another key parameter of our main analysis: the 20km radius we draw around the center of a ski resort municipality to assign banks to this municipality (- all banks with branch offices less than 20km away).

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 actually shaped Austria's labor laws (- due to the industry's importance for Austria's GDP). This is particularly true with respect to the industry's hiring of foreign workers, - regulated through a special provision in the law, the so-called "Saisonnier-Regelung". This provision was added to Austrian labor law because access to foreign workers is important for the country's ski tourism industry with respect to service personnel.

Among EU countries, Austria is the country with the smallest business revenue from unregistered employment relative to GDP. This is a second reason to believe that our data are reliable.

[2] See Nizalova and Murtazashvili (2016).