

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association between early working life patterns, in publicly and privately owned companies, and the course of future sickness absence due to mental disorders: a cohort study in Catalonia (Spain)
AUTHORS	Ayala-Garcia, Amaya; Serra, Laura; Ubalde López, M

VERSION 1 – REVIEW

REVIEWER	Jurgita Narusyte Karolinska Institutet, Sweden
REVIEW RETURNED	23-Jun-2020

GENERAL COMMENTS	<p>The study contributes with an important knowledge on the role of early working life on future sickness absence. The manuscript clearly describes the analysis part, making it easy to follow and understand. My concern is though whether the sample size is large enough for the chosen study design. Also, Introduction and Discussion would gain from a more thorough review of the previous research. Please find my comments and suggestions below.</p> <p>Title The authors use term “relationship” in the title. In epidemiology, a more common term is “association”, which is also used by authors later in the manuscript. My suggestion is therefore to change “relationship” into “association”, both in the title and in Abstract.</p> <p>Abstract The results presented in Abstract focus on LMPs that gain more favorable SA trajectories. However, in Conclusions, authors focus on LMPs possibly associated with less favorable SA trajectories. It comes a little bit of a surprise and confuses a reader, I think. Most importantly, the associations in Conclusions were not significant but are described as they were significant. Please revise.</p> <p>Introduction A detailed review of the (historical) development of employment situation in Spain is provided in Introduction. However, the background behind the association between LMPs and SA is very scarce and there is hardly any background on why SA due to mental diagnoses was studied in this study as well as why LMPs and SA could be different private and public companies. Please add a review of previous literature on that.</p>
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	<p>Last sentence in paragraph 4, page 4. The authors probably mean most frequent diagnoses behind SA. Please modify accordingly.</p> <p>Methods SA due to mental diagnoses: please add what ICD-10 diagnoses specifically were included in the study. Have all mental diagnoses been included or have some of them been excluded (e.g., most severe ones)? SA trajectories were estimated for a period of three years. This seems to be a short time to identify trajectories for SA due to mental diagnoses. Mental disorders usually require a long-term and several years' treatment, and therefore three years of follow-up does not seem to be sufficient here to capture the change (the very stable SA trajectories among males give a hint about that). Also, in general, at least four time points are usually recommended for growth models. Thus, the authors may want to consider starting the SA follow-up one year earlier, i.e., 2011. Or, change the outcome variable into simple yes/no SA or categorical SA with different number of SA days (please see my next comment). The other issue related to SA trajectories is the number of observations in each of these. It's reflected by logistic regression results where only a few associations are significant and those that are significant have wide confidence intervals. What was a rationale behind studying SA trajectories? Perhaps having a dichotomous SA (i.e., at least one SA day during 2012-2014) or SA with categories for different number of SA days would be more suitable here? Please consider that.</p> <p>Results Description of the whole sample (i.e., Table 1) is missing. Besides, there seems to be some confusion with table numberings as there are two tables No 1 right now. Paragraph 5, page 6. The authors give a very detailed description of LMP "Delayed employment" here. It seems unnecessary as the number of observations in this pattern is very small and the results do not say a lot. At the very least, a notion on a small number of individuals needs to be added here. Page 8. SA pattern "Decreasing" among women and "High stable" among men also have very few individuals and this should be noted.</p> <p>Discussion Paragraph 4, page 14. The authors discuss in detail the findings on LMP Delayed Employment. Again, this pattern includes very few observations, which is also reflected by non-significant associations or wide confidence intervals. In my view, less attention could be paid to the discussion of this finding. At the very least, a notion on few observations regarding this LMP should be added here. Paragraph 5, page 14. Very few men (n=58) belonged to the pattern of Delayed employment and very few men belonged to High stable SA trajectory (n=69). Even though the association is significant, the wide confidence intervals warrants about instability. Therefore, the authors should be more cautious with the language when discussing it and also notify the reader about the small number of observations.</p>
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	<p>Paragraph 6, page 14. The authors give a short review of previous studies here which would fit perfectly in Introduction instead.</p> <p>Paragraph 2, page 15. Again, due to the scarce material the findings on public sector should take less space and attention here.</p> <p>Paragraphs 4 and 5, page 15. Authors give a review of previous studies here which seems to be more suitable in Introduction. Please modify.</p> <p>Limitations One of the main limitations is the number of observations in some of LMPs and SA trajectories. Please add this.</p> <p>Last sentence This sentence sounds as the aim and could be moved to Introduction instead.</p> <p>Conclusion First sentence. It is unclear what the authors mean with “this situation” here. Second sentence. This finding was not significant but is described as the main (significant) finding of the study. Please revise.</p>
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REVIEWER	Ben Amick University of Arkansas for Medical Sciences USA
REVIEW RETURNED	25-Sep-2020

GENERAL COMMENTS	<p>My comments are a series of minor revisions except one overall comment. Overall, this paper needs a review by an English editor. It is not poorly written or well written it needs a heavy/hard edit and reviewers should not be asked to do this edit.</p> <p>In the Abstract, the Results needs to be expanded to describe the four employment trajectories and it needs to be clear what the reference group is.</p> <p>In the Methods, it is unclear what decision rules were used in either optimal matching or latent growth modeling to choose groups. Also choices were made about how to describe states (e.g., type of contract), but this reviewer wonders whether a more effective approach might not be to choose time-varying covariates vs assigning the most time spent in a category. Choices are always made in statistical modeling.</p> <p>In the results, there were a lot of tables with a lot of information. On the one hand this I every exciting, but as I looked at Figure one I noticed the patterns were very similar for men and women. Arguable for the primary exposure there is no need to disaggregate the already complex exposure data. I looked up front to see if the authors justified the gender stratification and there was no justification. I would recommend a justification for stratification, a priori, rather than post hoc explanations of findings.</p> <p>In the Discussion, I was a little dissatisfied with the discussion of the limitations and strengths. First, the administrative data is a great strength since it allows obtaining detailed diagnostic</p>
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	<p>groups. But it is unclear why all mental health problems were aggregated. There is no discussion of the variability on clinical outcomes. Is this a depression result? That would link to other literature. Second, from a life course perspective there is a growing literature on the role of mental health problems in adolescence on working life. While discussed there is a robust literature to reflect up here.</p> <p>References are not recent. Search, at least, for Bultmann research in the Netherlands. There is a large literature describing employment trajectories that is not discussed. Perhaps the authors have a reason. But, it would be good to declare why this literature is not referenced.</p> <p>As I said above each of these is a small revision but there is more than two. A worthy contribution to the literature.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Jurgita Narusyte

Institution and Country: Karolinska Institutet, Sweden

Please state any competing interests or state 'None declared': None declared

The study contributes with an important knowledge on the role of early working life on future sickness absence. The manuscript clearly describes the analysis part, making it easy to follow and understand. My concern is though whether the sample size is large enough for the chosen study design. Also, Introduction and Discussion would gain from a more thorough review of the previous research. Please find my comments and suggestions below.

We appreciate the reviewer's comments and recommendations. We have answered them below.

Comment 1: Title. The authors use term "relationship" in the title. In epidemiology, a more common term is "association", which is also used by authors later in the manuscript. My suggestion is therefore to change "relationship" into "association", both in the title and in Abstract.

We thank the reviewer the suggestion. It is true that in epidemiology "association" term is used when analytic studies are conducted. We considered relationship for the title as term that showed the potential nature of the association (cause-effect). Following the reviewer's suggestion, the title has been modified from "relationship" to "association". Also following the editorial's requirement, the new title is as follows:

Manuscript title: "Association between early working life patterns, in publicly and privately owned companies, and the course of future sickness absence due to mental disorders: a cohort study in Catalonia (Spain)"

Comment 2: Abstract. The results presented in Abstract focus on LMPs that gain more favourable SA trajectories. However, in Conclusions, authors focus on LMPs possibly associated with less favourable SA trajectories. It comes a little bit of a surprise and confuses a reader, I think. Most importantly, the associations in Conclusions were not significant but are described as they were significant. Please revise.

We appreciate the reviewer's comment and in accordance we have changed the conclusions of the abstract. There was a confusion in the wording of these conclusions. Consequently, they have been rewritten as follows:

“Early labour market participation patterns characterized by an increasing stability - decreased number of transitions between temporary contracts and lack of social security coverage towards permanent contracts- were related to a better future SA course due to mental diagnosis”. (page 1, abstract, conclusions section)

By this change, statistical significance is considered in the conclusions and coherence between results and conclusions is maintained. As recommended by the other reviewer we have added the following to results:

“Among both men and women, four LMP patterns were identified: Stable Permanent Employment (reference group), Increasing Permanent Employment, Fluctuating Employment, and Delayed Employment.” (page 2, abstract, results section)

Comment 3: Introduction. A detailed review of the (historical) development of employment situation in Spain is provided in Introduction. However, the background behind the association between LMPs and SA is very scarce and there is hardly any background on why SA due to mental diagnoses was studied in this study as well as why LMPs and SA could be different private and public companies. Please add a review of previous literature on that. Last sentence in paragraph 4, page 4. The authors probably mean most frequent diagnoses behind SA. Please modify accordingly.

We thank the reviewer for the comments and the opportunity of improving the background of our study. We have extended it and covered all aspects suggested by the reviewer: association between LMP patterns and SA, background behind SA due to mental diagnosis and labour market participation and SA differences in private and public companies.

First of all, the last sentence in the introduction section (paragraph 3) has been modified to: “The most frequent diagnoses behind SA are musculoskeletal disorders and mental disorders(13).”

Then, in order to better justify our study objective, we've added the following text in the Introduction section (at the beginning of 4th paragraph):

“Several studies have reported that unemployment, temporary employment, and job insecurity are related to mental disorders, stress, and poor self-rated health (14,15). In 2017, a scoping study highlighted one mechanism that drives the effects of precarious employment and unemployment on mental disorders among young people—namely, the life-course perspective, suggesting a cumulative nature of the exposure (16). It has been projected that the frequency of mental disorders will increase, particularly affecting young adults (17).” (...)

Also, we have included the following one in the 5th paragraph of Introduction section:

(...)“Existing literature has focused on whether public companies attract potentially absent workers, due to worse health status and greater self-interest, compared to the presumed more restrictive SA practices in the private sector (24–26). A study in Finland found that the risk for more adverse SA trajectories did not differ between public and private employees (5). However, the relationship between the potential effects of previous working life on SA trajectories in the private and public sectors has generally been poorly studied. ”

And finally, we have added this sentence at the end of 6th paragraph of Introduction:

(...) “Recent research has applied the life-course perspective to labour-market trajectories to assess how working life affects health outcomes, such as SA. Longitudinal analysis provides a more holistic perspective of work participation due to the consideration of transitions, order, and duration of employment statuses (14,29).”

Comment 4: Methods. 4.1. SA due to mental diagnoses: please add what ICD-10 diagnoses specifically were included in the study. Have all mental diagnoses been included or have some of them been excluded (e.g., most severe ones)?

4.1. We thank the reviewer for pointing out the lack of explicitness on the mental diagnoses included in the study. In response to this comment we have added: (...) “All mental and behavioural disorders (ICD-10, chapter V [F00-F99]) were included.” (page 5, methods, 4th paragraph)

4.2. SA trajectories were estimated for a period of three years. This seems to be a short time to identify trajectories for SA due to mental diagnoses. Mental disorders usually require a long-term and several years’ treatment, and therefore three years of follow-up does not seem to be sufficient here to capture the change (the very stable SA trajectories among males give a hint about that). Also, in general, at least four time points are usually recommended for growth models. Thus, the authors may want to consider starting the SA follow-up one year earlier, i.e., 2011. Or, change the outcome variable into simple yes/no SA or categorical SA with different number of SA days (please see my next comment).

4.2. Long-term clinical features of severe or chronic mental disorders may not be captured in a three-year period but, unfortunately, we only have information about SA episodes from 2012 to 2014. However, we had available exact date for the onset and closure of episodes which allows to calculate cumulative days on SA and its trend over time that could be understood as a proxy for the severity of the mental disorder. In conclusion, a three-year period with a growing accumulation of days could potentially capture the poorest mental health status courses in terms of temporary disability (i.e. SA).

4.3. The other issue related to SA trajectories is the number of observations in each of these. It’s reflected by logistic regression results where only a few associations are significant and those that are significant have wide confidence intervals. What was a rationale behind studying SA trajectories? Perhaps having a dichotomous SA (i.e., at least one SA day during 2012-2014) or SA with categories for different number of SA days would be more suitable here? Please consider that.

4.3. We appreciate that the reviewer highlights the small number of observations of some SA trajectories, and our results should be interpreted with caution, but still the associations found can be understood as a first step to identify small groups of workers that might need special attention. Also, when looking at limits of too few numbers of observations in classes, some authors consider meaningful a trajectory group that represents at least 5% of the study population (2).

Regarding the change of the way we measured SA trajectories due to mental disorders, as explained above, only 2012 to 2014 diagnosis are available, so the extension of the follow-up period is not feasible. The possibility of changing the outcome to dichotomous would imply a change of design and hypothesis since the inclusion criteria is having at least one episode of SA due to mental disorders. We are interested in the progression of the SA accumulation of days to identify and compare worse SA trajectories as a proxy of the severity of the mental disorder. The third possibility, changing to a categorical variable of number of days, would also imply losing the evolution in time of the outcome, which enriches the study from a clinical perspective (2,3).

1. Farrants K, Friberg E, Sjölund S, Alexanderson K. Trajectories of future sickness absence and disability pension days among individuals with a new sickness absence spell due to osteoarthritis diagnosis ≥ 21 days: A prospective cohort study with 13-month follow-up. *BMJ Open*. 2019;9(8):1–11.
2. Nagin DS, Nagin DS, Odgers CL. Group-Based Trajectory Modeling in Clinical Research. 2014;(May).
3. Jung T, Wickrama KAS. An Introduction to Latent Class Growth Analysis and Growth Mixture Modeling. *Soc Personal Psychol Compass*. 2008;2(1):302–17.

Comment 5: Results. Description of the whole sample (i.e., Table 1) is missing. 5.1. Besides, there seems to be some confusion with table numberings as there are two tables No 1 right now.

5.1. We thank the reviewer for pointing out the error of the name of table 1, we have corrected it. Also, a description of the whole sample is available in the supplementary file:

Age in 2002 (mean (SD))	Women (N=879)			Men (N=500)		
		23.2 (3.0)			23.1 (3.0)	
2002-2011	N (%)	Episodes* (%)	MD (P25; P75)	N (%)	Episodes* (%)	MD (P25. P75)
Company size						
Small-medium (\leq 100 workers)	576 (65.5)	694 (65.2)	26 (9;72.5)	367 (73.4)	442 (70.4)	22 (9;55)
Big (>100 workers)	303 (34.5)	370 (34.8)	18 (8;58)	133 (26.6)	186 (29.6)	21 (6;58)
Company ownership						
Private	738 (84.0)	892 (83.8)	22 (8; 64)	449 (89.8)	449 (71.5)	22 (8;54)
Public	100 (11.4)	125 (11.7)	29 (10.5; 76)	32 (6.4)	39 (6.2)	35.5 (6;75.5)
2012-2014						
Working time (%weekly hours)						
Full-time (>87.5%)	748 (85.1)	902 (84.8)	24 (9; 66.5)	476 (95.2)	595 (94.7)	22 (8;57.5)
Part-time (50–87.5%)	116 (13.2)	145 (13.6)	18 (8;45)	21 (4.2)	29 (4.6)	16 (6;50)
Short part-time (37.5–49%)	5 (0.6)	7 (0.7)	75 (24;103)	1 (0.2)	2 (0.3)	11 (11;11)
Marginal part-time (\leq 37.5%)	10 (1.1)	10 (0.9)	17.5 (10;43)	2 (0.4)	2 (0.3)	120 (46;194)
Occupational category						
Non-manual skilled	160 (18.2)	189 (17.8)	23 (10; 66.5)	50 (10)	61 (9.7)	23 (11;67)
Non-manual non-skilled	503 (57.2)	616 (57.9)	24 (9; 67)	150 (30)	178 (28.3)	16 (6;50)
Manual skilled	137 (15.6)	161 (15.1)	22 (7; 58)	217 (43.4)	277 (44.1)	21 (9;61)
Manual non-skilled	79 (9.0)	98 (9.2)	24 (8; 56)	83 (16.6)	112 (17.8)	28 (7;61)
Income in tertiles						
High	268 (30.5)	327 (30.7)	23 (8; 63)	191 (38.2)	244 (38.9)	19 (8;60)
Medium	282 (32.1)	328 (30.8)	27 (10; 78)	177 (35.4)	215 (34.2)	15 (6;46)
Low	328 (37.4)	407 (38.3)	21.5 (8;59)	132 (26.4)	169 (26.9)	37 (13.5;82)
Type of contract						
Permanent contract	701 (79.8)	837 (78.7)	23 (9; 67)	418 (83.6)	523 (83.3)	21 (8;58)
Temporary contract	178 (20.3)	227 (21.3)	26 (8.5; 61)	82 (16.4)	105 (16.7)	28 (7;55)
Total	879	1.064		500	628	

5.2. Paragraph 5, page 6. The authors give a very detailed description of LMP “Delayed employment” here. It seems unnecessary as the number of observations in this pattern is very small and the results do not say a lot. At the very least, a notion on a small number of individuals needs to be added here.

5.2. Regarding to the “Delayed employment” description, certainly this pattern has the fewest observations of all sample even though we find this pattern important and in line with literature descriptions of young working population transition from education to working life. Similarly, “Decreasing” and “High Stable” trajectories depict interesting tendencies, but we agree that its interpretation should be in accordance to its representativeness. We thank the reviewer the concern and we have highlighted the few number of observations that it represents and reduced the description to the most relevant features. The text in the results have been adapted as follows:

(...) “Women in this pattern showed ~~the lowest proportion working on big companies (30.8%)~~ the greatest proportions of marginal part-time jobs (2.9%) and manual non-skilled jobs (15.4%), and men in this pattern had the highest proportion of part-time jobs (8.6%); however, these patterns were based on very small numbers of individuals. ~~and the lowest proportion of men working on publicly owned companies (3.5%).~~” (page 6, results, 4th paragraph)

5.3. Page 8. SA pattern “Decreasing” among women and “High stable” among men also have very few individuals and this should be noted.

In line with above adaptations of the text, the following comment has been added to Results:

(...) “The Decreasing trajectory showed the lowest number of episodes (16.4%) and represented the smallest proportion of workers (14.7%), but accumulated a high number of days annually (median duration: 38.5–97 days).” (...) (page 8, results, 6th paragraph)

(...) “In contrast, the High Stable trajectory included 13.8% of workers and represented the highest proportion of low-income levels (44.9%).” (...) (page 10, results, 7th paragraph)

Comment 6: Discussion. 6.1. Paragraph 4, page 14. The authors discuss in detail the findings on LMP Delayed Employment. Again, this pattern includes very few observations, which is also reflected by non-significant associations or wide confidence intervals. In my view, less attention could be paid to the discussion of this finding. At the very least, a notion on few observations regarding this LMP should be added here.

Paragraph 5, page 14. Very few men (n=58) belonged to the pattern of Delayed employment and very few men belonged to High stable SA trajectory (n=69). Even though the association is significant, the wide confidence intervals warrants about instability. Therefore, the authors should be more cautious with the language when discussing it and also notify the reader about the small number of observations.

We thank the reviewer’s suggestion of highlighting the caution when it comes to interpreting these associations. Nevertheless, these two patterns, although small and not statistically significant are meaningful in terms of our hypothesis and the study population. We have edited the discussion of these patterns to facilitate a correct interpretation:

(...) “The patterns that most depicted a precarious early working life were Fluctuating Employment and Delayed Employment, and thus we expected that persons with these patterns would more commonly show a worse course of future SA for mental disorders. Our results revealed differences between men and women, although these findings should be cautiously

interpreted since some working life patterns were exhibited by a small number of workers, potentially leading to non-significant associations. (...) Nevertheless, the delayed entry to the labour market in men, showed a later protective effect towards a course of a SA that accumulates a high number of days. This finding could potentially be related to avoidance of the risk of a precarious labour market through family support, as reported in the literature, which enables the young population to delay entry into the labour market, attain a higher education level, and potentially access more favourable employment (37). (...). Notably, our results are inconclusive in terms of significance and thus the observed associations should be interpreted as trends.” (page 15, discussion, 4th paragraph)

6.2. Paragraph 6, page 14. The authors give a short review of previous studies here which would fit perfectly in Introduction instead.

6.4. Paragraphs 4 and 5, page 15. Authors give a review of previous studies here which seems to be more suitable in Introduction. Please modify.

6.2 and 6.4. We thank the reviewer for the suggestion, and we have completed the introduction as specified above and moved paragraph 4 and 5 (page 15) to introduction.

6.3. Paragraph 2, page 15. Again, due to the scarce material the findings on public sector should take less space and attention here.

6.3. We completely agreed with the reviewer regarding the lack of findings about the ownership of the company. We have extended the rationale behind it on introduction to give response to this suggestion as specified above in the comment 3.

Comment 7: Limitations. One of the main limitations is the number of observations in some of LMPs and SA trajectories. Please add this.

Last sentence. This sentence sounds as the aim and could be moved to Introduction instead.

We thank the reviewer for the suggestion and have added to limitations the few number of observations:

(...) “Likewise, the methodology applied to the LMP patterns and SA trajectories involved group-based analyses that classified individuals according to similar behaviours. Thus, some of the resulting groups had a very small number of observations, and these results should be interpreted with caution. However, some authors argue that a minimum of 5% should be enough to consider a pattern, and our results are above these recommendations (46).” (page 17, discussion, 9th paragraph)

Following the reviewer’s suggestion, the last sentence has been moved and expanded at the end of the Introduction section (page 4):

“In the present study, we aimed to explore from a life-course perspective the relationship between labour market participation trajectories at the start of working life, at public and private companies, and the course of future SA due to mental disorders.”

Comment 8: Conclusion. First sentence. It is unclear what the authors mean with “this situation” here. Second sentence. This finding was not significant but is described as the main (significant) finding of the study. Please revise.

We have followed the reviewer's suggestion and modified the first sentence of the conclusion section as follows (page 17):

"Overall, our present results provide insights regarding how labour market transitions—characterized by employment flexibility and high unemployment rates—have impacted the course of future mental health among the youngest working population."

As commented by the reviewer, according to the lack of significance of some associations, the conclusion has been change to the following:

"Our analyses revealed that early working lives characterized by transitions between types of contracts, and periods of unemployment and lack of social security coverage, were apparently related to a worse mental health course." (...) (page 17, conclusion, 2nd paragraph)

Reviewer: 2

Reviewer Name: Ben Amick

Institution and Country: University of Arkansas for Medical Sciences, USA

Please state any competing interests or state 'None declared': No competing interest

My comments are a series of minor revisions except one overall comment. Overall, this paper needs a review by an English editor. It is not poorly written or well written it needs a heavy/hard edit and reviewers should not be asked to do this edit.

Comment 1: In the Abstract, the Results needs to be expanded to describe the four employment trajectories and it needs to be clear what the reference group is.

We thank the reviewer for the suggestion of expanding the description of LMP patterns. We have added the following text in the Results section:

"Among both men and women, four LMP patterns were identified: Stable Permanent Employment (reference group), Increasing Permanent Employment, Fluctuating Employment, and Delayed Employment." (page 2, abstract, results section)

Comment 2: In the Methods,2.1. it is unclear what decision rules were used in either optimal matching or latent growth modelling to choose groups.

2.1. According to the reviewer's comment, we have introduced more information related to criterion for the election of LMP patterns and SA trajectories. Thus, we have added this information to the article as follows:

(...) "Average silhouette width was used to select the optimal number of clusters." (page 5, methods, statistical analysis section, 1st paragraph)

(...) "The optimal number of trajectories was assessed considering the lower Bayesian information criterion (BIC), and the Lo-Mendell-Rubin adjusted and bootstrap likelihood ratio tests. In cases where the compared fit indexes had similar values, the one with the highest entropy was preferentially chosen (34). The size of each class was dependent on the sample size and how meaningful a small group was for the study aim." (page 5, methods, statistical analysis section, 2nd paragraph)

2.2. Also choices were made about how to describe states (e.g., type of contract), but this reviewer wonders whether a more effective approach might not be to choose time-varying covariates vs assigning the most time spent in a category. Choices are always made in statistical modelling.

2.2. We considered the possibility of time-varying covariates for the adjustment variables but lastly, we decided to assign the category where the individual spent more time. Nevertheless, it would be very interesting to apply this approach for a future study.

Comment 3: In the results, there were a lot of tables with a lot of information. On the one hand this I every exciting, but as I looked at Figure one I noticed the patterns were very similar for men and women. Arguable for the primary exposure there is no need to disaggregate the already complex exposure data. I looked up front to see if the authors justified the gender stratification and there was no justification. I would recommend a justification for stratification, a priori, rather than post hoc explanations of findings.

We thank the reviewer for the comment. We consider very important to distinguish between men and women, mainly because mental health problems present higher prevalence rates among women. Additionally, even if the LMP patterns are very similar, labour market dynamics might affect differently men and women, mostly in relation with work and employment conditions (i.e. precariousness) that both face during working life. In order to clarify the importance of the stratification we have added the following text to the introduction:

“Health outcomes and work participation clearly differ between men and women and among different age groups (22), highlighting the need for separate investigations of predictors of SA course. Regarding differences in work participation, precariousness is believed to be a gendered phenomenon due to processes of family formation, gender segregation, and wage discrimination, which contribute to an already unstable labour market context (23).” (...) (page 3, introduction, 5th paragraph)

Comment 4: In the Discussion, I was a little dissatisfied with the discussion of the limitations and strengths. First, the administrative data is a great strength since it allows obtaining detailed diagnostic groups. But it is unclear why all mental health problems were aggregated. There is no discussion of the variability on clinical outcomes. Is this a depression result? That would link to other literature. Second, from a life course perspective there is a growing literature on the role of mental health problems in adolescence on working life. While discussed there is a robust literature to reflect up here.

We thank the reviewer for the comment. We considered the possibility of including only common mental disorders. However, we decided to keep all mental and behavioural disorders for several reasons. First, severe mental health problems SA trajectory could also be affected by an unstable LMP pattern. Second, the sample is small, and the exclusion of these diagnoses would decrease sample size. Third, we are interested in making a first approach on the relationship between LMP patterns and temporary disability due to mental health disorders before doing further analysis on specific results.

Anyhow, following the reviewer’s concern we have carried out the analyses only including SA diagnosis for common mental disorders, which include: Depression (ICD-10 [F32, F33, F34]), Generalised anxiety disorder (ICD-10 [F41.1, F41.2]), Panic disorder (ICD-10 [F41.0]), Obsessive-compulsive disorder (ICD-10 [F42]), Post-traumatic stress disorder (ICD-10 [F43.1]), Phobia (ICD-10 [F40]) and Adaptative disorders (ICD-10 [F43.2]). Overall, these episodes represent 91% of all SA episodes in our sample. We have added the table with the associations between LMP patterns, and graphs with SA trajectories below as shown in these new figures (figure 1 and 2). The same number of classes for SA trajectories fits perfectly and proportions in each category remain practically the same. Association results don’t vary significantly from considering all mental disorders diagnoses (Annex 1, table 1).

For these reasons, we have decided to keep all mental health diagnosis (ICD-10, chapter V [F00-F99]) in the analysis. We thank very much the reviewer the comment that has given us the opportunity of further exploration of our data.

Comment 5: References are not recent. Search, at least, for Bultmann research in the Netherlands. There is a large literature describing employment trajectories that is not discussed. Perhaps the authors have a reason. But, it would be good to declare why this literature is not referenced.

We thank the reviewer for the suggestion. We have revised and updated literature about employment trajectories both in the introduction and discussion with the following references which numbering matches the one in the main manuscript:

19. Norder G, Roelen CAM, Bültmann U, van der Klink J JL. Shift work and mental health sickness absence: a 10-year observational cohort study among male production workers. *Scand J Work Environ Health*. 2015;41(4):413–6.
20. Norder G, Roelen CAM, van der Klink J JL, Bültmann U, Sluiter JK, Nieuwenhuijsen K. External Validation and Update of a Prediction Rule for the Duration of Sickness Absence Due to Common Mental Disorders. *J Occup Rehabil*. 2017;27(2):202–9.
28. Amick BC, McLeod CB, Bültmann U. Labor markets and health: An integrated life course perspective. *Scand J Work Environ Heal*. 2016;42(4):346–53.
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We have added the following text to the discussion:

(...) “A life-course perspective has been applied to examine how mental health trajectories in young adults might impact work, but such studies have measured work as a single event, assessing the effect of pre-existing mental disorders or considering the longitudinal approach for return to work (42,43). The potential effects of transitions within the labour market on future sickness absence remain unexplored.”(page 16, discussion, 5th paragraph)

As I said above each of these is a small revision but there is more than two. A worthy contribution to the literature.

Annex 1:

Table 1 Association between labour market participation patterns and sickness absence trajectories among salaried workers

	Women			Men	
	Low Decreasing vs Low stable	Decreasing vs Low stable	Increasing vs Low stable	Middle Stable vs Low stable	High Stable vs Low stable
	aOR (95% CI)	aOR (95% CI)	aOR (95% CI)	aOR (95% CI)	aOR (95% CI)
2002-2011					
Labour market participation patterns					
Stable permanent	1	1	1	1	1
Increasing permanent	1.05 (0.67-1.62)	1.53 (0.89-2.62)	0.83 (0.50-1.39)	1.05 (0.59-1.88)	0.85 (0.38-1.87)
Delayed Trajectory	1.62 (0.90-2.91)	2.19 (1.07-4.46) [†]	0.76 (0.37-1.57)	1.23 (0.60-2.54)	0.21 (0.05-1.03)
Fluctuating trajectory	1.13 (0.64-1.99)	2.00 (1.02-3.95) [†]	0.54 (0.28-1.06)	1.02 (0.53-1.96)	0.96 (0.40-2.31)
Company size					
Small-medium (≤ 100 workers)	1	1	1	1	1
Big (>100 workers)	0.61 (0.42-0.90) [†]	0.48 (0.29-0.79) [†]	0.77 (0.50-1.19)	0.95 (0.55-1.62)	1.42 (0.69-2.96)
Company ownership					
Private	1	1	1	1	1
Public	1.81 (0.98-3.33)	1.42 (0.64-3.16)	1.34 (0.66-2.73)	1.88 (0.73-4.84)	1.65 (0.44-6.17)
2012-2014					
Working time (%weekly hours)					
Full-time (>87.5%)	1	1	1	1	1
Part-time (50–87.5%)	1.19 (0.72-1.96)	0.29 (0.12-0.72)	1.11 (0.62-1.99)	0.81 (0.28-2.32)	0.93 (0.23-3.79)
Short part-time (37.5–49%)	.	.	.	-	-
Marginal part-time (≤37.5%)	1.18 (0.25-5.55)	0.52 (0.05-4.99)	1.34 (0.23-7.70)	-	-
Occupational category					
Non-manual skilled	1	1	1	1	1
Non-manual non-skilled	1.12 (0.66-1.90)	1.16 (0.58-2.30)	0.98 (0.53-1.80)	0.77 (0.36-1.68)	0.76 (0.24-2.45)
Manual skilled	0.94 (0.48-1.83)	0.91 (0.39-2.11)	1.01 (0.46-2.14)	0.79 (0.36-1.73)	1.00 (0.31-3.20)
Manual non-skilled	1.15 (0.52-2.54)	0.64 (0.22-1.87)	1.11 (0.45-2.74)	1.04 (0.42-2.59)	0.99 (0.26-3.76)
Income in tertiles					
High	1	1	1	1	1

				1.30	0.82
Medium	1.21 (0.77-1.93)	1.76 (0.97-3.20)	0.96 (0.56-1.65)	(0.76-2.21)	(0.36-1.87)
Low	0.80 (0.48-1.32)	1.41 (0.74-2.69)	0.88 (0.50-1.55)	1.79	3.06
Type of contract				(0.92-3.45)	(1.28-7.29)
Permanent contract	1	1	1	1	1
				0.97	0.82
Temporary contract	0.87 (0.51-1.51)	0.57 (0.29-1.15)	1.93 (1.06-3.52)	(0.49-1.93)	(0.30-2.23)

aOR: adjusted odds ratio for the rest of the variables included in the model.

† Statistically significant (p<0.05).

Figure 1 Sickness absence trajectories among salaried women living in Catalonia (2012–2014).

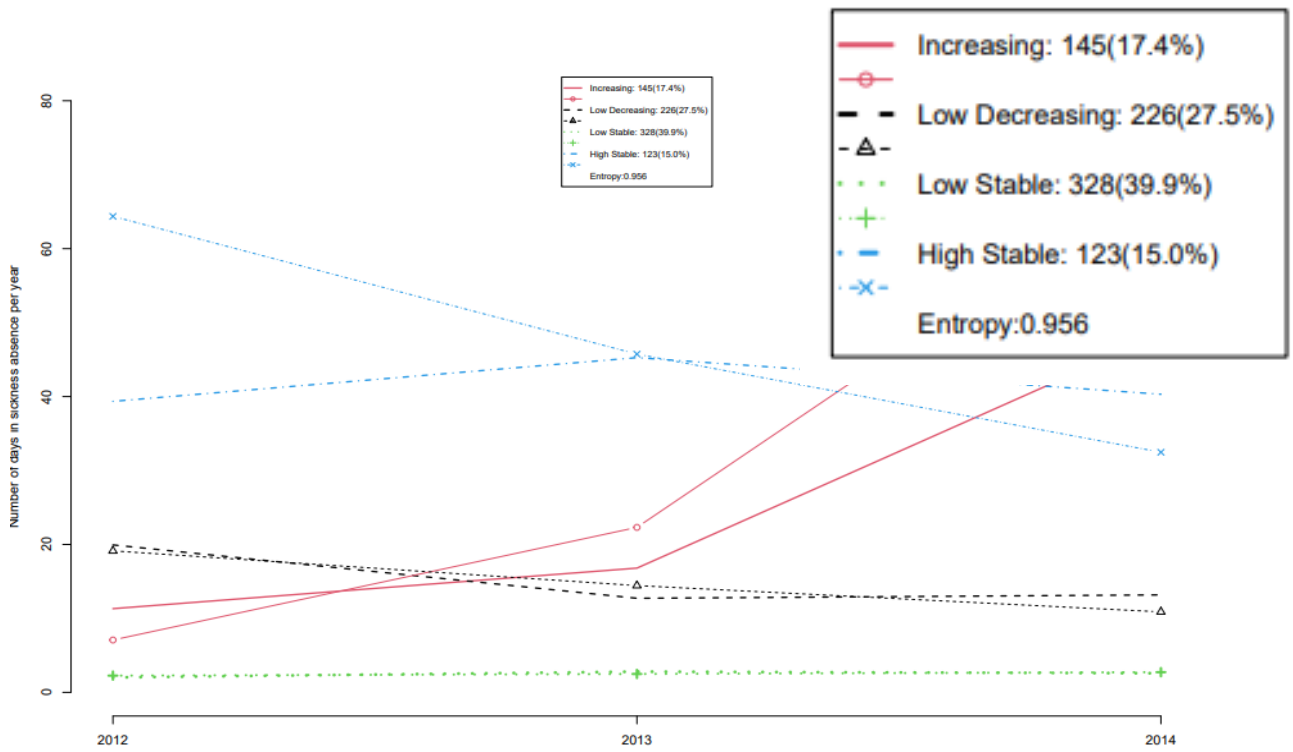
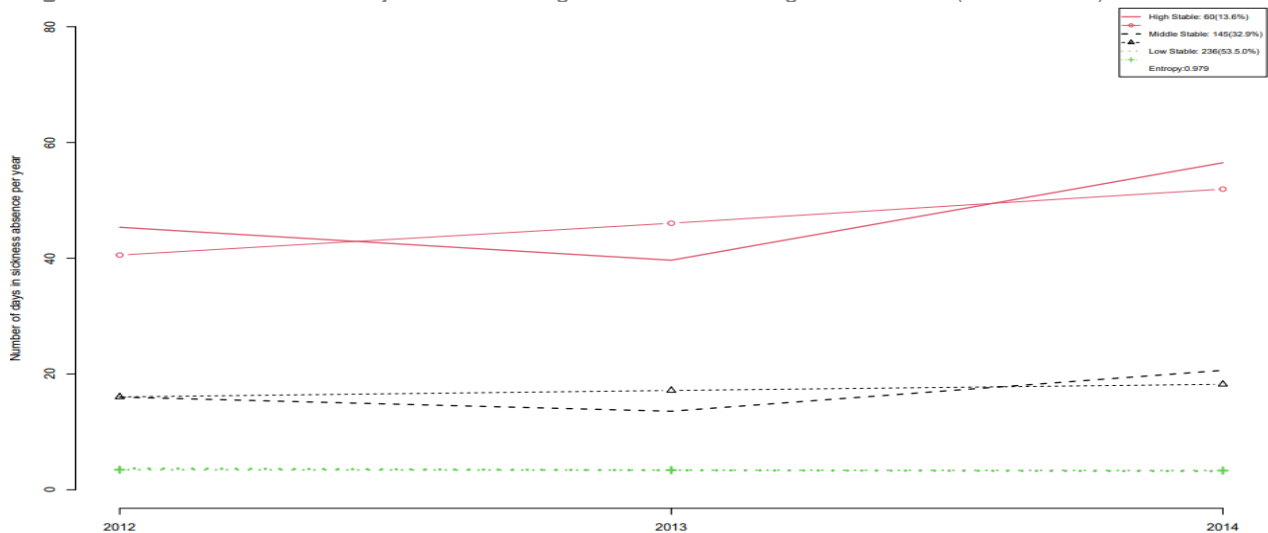


Figure 2 Sickness absence trajectories among salaried men living in Catalonia (2012–2014).



VERSION 2 – REVIEW

REVIEWER	Jurgita Narusyte Karolinska Institutet, Sweden
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REVIEW RETURNED	28-Dec-2020
GENERAL COMMENTS	Thanks to the authors for addressing my comments.