

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Cross-sectional Analysis of United States Scope of Practice Laws and Employed Physician Assistants
AUTHORS	Valentin, Virginia; Najmabadi, Shahpar; Everett, C

VERSION 1 – REVIEW

REVIEWER	Edward J Timmons Saint Francis University, USA
REVIEW RETURNED	19-Oct-2020

GENERAL COMMENTS	<p>The authors look at an important policy questions. I encourage the authors to do more with the regression-- I think the authors are missing an opportunity to do more with the existing data. My main comments address the regression and my questions and suggestions. I also follow up with some additional minor comments.</p> <p>Major comments</p> <p>1) The authors note that least squares regression was utilized on p.2 and p.6. It appears that the authors perform this analysis in Table 2, but I can't follow the methodology. Are the authors performing the following regression: $PA \text{ employment ration} = a + b1(PA \text{ "ideal"practice environment}) + b2(PA \text{ "average" practice environment}) ?$</p> <p>This needs to be clarified further as I can't tell from the information provided. Basic regression output like sample size and R2 should also be reported.</p> <p>I'd also recommend that the authors run a regression with the 3 years pooled that includes time dummies and perhaps some additional regional controls. This would allow the authors to provide more evidence in support of their hypothesis that broader PA SOP increases PA employment.</p> <p>2.) Although the demographic and age data are constrained to 3 years, the BLS data on employment is not. I'd recommend that the authors obtain employment data for all of the years of the OES-- or at the very least, all of the years of the OES where the survey was conducted the same month (beginning 2003 you can get snapshots from May). This would allow the authors to perform a fixed effects estimation (including time and state fixed effects) that would allow for a more thorough understanding of how changes in PA SOP impact PA employment.</p> <p>Minor comments</p>
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	<p>P.2: What is meant by "ideal" SOP? You explain this later on p.7, but this terminology should not be used here.</p> <p>P.3 It is misleading to characterize the data as 1998-2017-- this suggests an annual time series. You should consistently indicate that you have data for 3 years throughout the manuscript to avoid confusion: 1998, 2008, and 2017</p> <p>P.5 I'm not sure what you mean by "clinically active?" How is this determined? My understanding is the BLS simply provides estimates of employed professionals.</p> <p>P. 6: I don't think it makes sense to use the "ideal" terminology throughout the manuscript. Maybe AAPA model SOP? The term "ideal" makes a value judgement that undermines the author's basis for scientific argument.</p>
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REVIEWER	Benjamin McMichael University of Alabama United States of America
REVIEW RETURNED	09-Nov-2020

GENERAL COMMENTS	<p>I appreciate the opportunity to review this paper. It was interesting and addresses a chronically understudied topic. The paper offers new evidence on the relationship between physician assistant (PA) scope-of-practice (SOP) laws. Using data from three national datasets, the paper reports evidence that states with more permissive SOP laws have higher densities of PAs. The evidence offered in the paper is important, but I have a few concerns about the presentation and analysis that the authors may consider addressing. My comments follow in no particular order.</p> <p>At various times throughout the paper (e.g., p. 4, l. 21; p. 9, l. 47), the authors discuss prior work on the supply or capacity of the healthcare workforce. The authors may consider citing an additional two studies that are directly relevant: Stange, Kevin. 2014. "How does provider supply and regulation influence health care markets? Evidence from nurse practitioners and physician assistants." <i>Journal of Health Economics</i>, vol. 33, 1-17. McMichael, Benjamin. 2018. "Beyond Physicians: The Effect of Licensing and Liability Laws on the Supply of Nurse Practitioners and Physician Assistants." <i>Journal of Empirical Legal Studies</i>, vol. 15(4), 732-771.</p> <p>It would be helpful if the authors offered more details on the datasets they examine. Examining only three years of data is not necessarily a problem. However, it would be helpful to better understand this limitation. Are the BLS data not available during all years? Is the analysis limited to only years in which the AAPA demographic data were available?</p> <p>The way the paper reports its primary results with respect to changes in PAs per capita is somewhat unusual. The authors may consider reporting raw numbers or otherwise eliminating the use of these categories. Terms like "super expanders" and "contractors" made it harder to comprehend the results.</p> <p>The authors should consider changing the way they paper categorize PA SOP laws. The authors note that they follow existing literature in counting the number of six key elements a state has adopted and categorizing each state as falling into three categories of laws based on this number. This may lead to</p>
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inaccurate coding of legal schemes. This method essentially treats each of the six key elements as interchangeable and equal, but there is no particular reason to believe this is the case. For example, the use of licensure as a regulatory term may have essentially no impact on patient care. On the other hand, granting PAs full prescriptive authority and having scope of practice determined at the practice level are likely to meaningfully expand the ability of PAs to provide care to patients (and thus drive up demand for PAs). Treating either of these legal changes as interchangeable with licensure as a regulatory term may not be justified. Similarly, co-signature requirements may be relatively important, but removing legal caps on the number of PAs that a physician may supervise is more likely to contribute to the expansion of PAs per capita. In addition to these methodological concerns, the current legal categorization scheme offers little advice to policymakers on which elements matter most. Since they are all treated as interchangeable, policymakers will not know which ones make the most difference. This, in turn, may limit the usefulness of the paper. The authors may consider re-engineering how they categorize laws. They could either examine each constituent element by itself or decide which elements are most important and separate states into those that have adopted the most important elements and those that have not adopted all of those elements.

One explanation for the lack of an association between the number of PAs and SOP laws in 1998 may be the fact that, until the Balanced Budget Act of 1997, PAs were not able to bill Medicare independently. Many insurance companies had similar rules. Thus, even if a state had less restrictive SOP laws, PAs would still be restricted under federal law, which would explain the lack of an association in 1998. This changed after the Balanced Budget Act of 1997, which is consistent with the significant associations found in later years.

The paper states in the discussion section: “However, as of 2017 half of US states are defined as contractors or never adopters, with PA/population ratios below the national median” (p. 9, ll. 41-42). Why is this noteworthy? This seems like it should be true by construction. If this is not true by construction, I think more explanation would be helpful.

The paper also states in the discussion section: “This also highlights research that indicates that perhaps PA demand and daily work is not impacted as much by state SOP laws as organizational policies.” I do not believe the results of this paper support this statement. This paper analyzes three years of data, and the authors note that they cannot fully address all of the ways that SOP laws and the supply of PAs may interact. Such an analysis is not enough to conclude that organizational policies may be more important—this type of conclusion could only be drawn from a more thoroughgoing analysis of SOP laws or from a comparative analysis of SOP laws and organizational policies. The authors have not done either of these here. I do not mean to minimize the contribution this paper makes. I only mean to emphasize that this contribution is limited to the realm of SOP laws. The authors should consider eliminating the above language and other instances where they suggest that their results are sufficient to draw any conclusions about the relative importance of SOP laws and organizational policies.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Comments to the Author

The authors look at an important policy questions. I encourage the authors to do more with the regression-- I think the authors are missing an opportunity to do more with the existing data. My main comments address the regression and my questions and suggestions. I also follow up with some additional minor comments.

Response: We appreciate your concern about doing more with the existing data. This cross-sectional study along with our previous publication "State Scope of Practice Laws: An Analysis of Physician Assistant Programs and Graduates"¹ are to set the background for our further time-series analysis. We made sure to clarify the applied linear predictive models both in this letter and in the revised manuscript.

Major comments

1) The authors note that least squares regression was utilized on p.2 and p.6. It appears that the authors perform this analysis in Table 2, but I can't follow the methodology. Are the authors performing the following regression:

PA employment ration = a + b1(PA "ideal"practice environment) + b2 (PA "average" practice environment) ?

This needs to be clarified further as I can't tell from the information provided. Basic regression output like sample size and R2 should also be reported.

I'd also recommend that the authors run a regression with the 3 years pooled that includes time dummies and perhaps some additional regional controls. This would allow the authors to provide more evidence in support of their hypothesis that broader PA SOP increases PA employment.

Response: Thank you for this suggestion. Our study is a retrospective cross-sectional analysis at three time points. Assessing the time effect in PA employment is beyond the scope of this study. Per your suggestion we strengthened the statistical analysis by adding in demographic Table 1, the survey simple linear regression model, the generated total and by SOP policies least square means for the available covariates (age and percent female PA) which were weighted by the states' population in the respective year. In Table 2, linear mixed models were used to generate unadjusted and adjusted least square means ($y = X\beta + \varepsilon$).

2.) Although the demographic and age data are constrained to 3 years, the BLS data on employment is not. I'd recommend that the authors obtain employment data for all of the years of the OES-- or at the very least, all of the years of the OES where the survey was conducted the same month (beginning 2003 you can get snapshots from May). This would allow the authors to perform a fixed effects estimation (including time and state fixed effects) that would allow for a more thorough understanding of how changes in PA SOP impact PA employment.

Response: We appreciate this suggestion. As assessing the time effect is beyond the scope of this study, we would consider the information for our further time-series analysis.

Minor comments

P.2: What is meant by "ideal" SOP? You explain this later on p.7, but this terminology should not be used here.

Response: Thank you for your question. The term ideal as defined in the methods is a state with 5-6 SOP elements. This terminology and categorization is based on previous literature by Wing et al.2 To prevent a value judgement terminology "ideal" has been changed to "permissive".

P.3: It is misleading to characterize the data as 1998-2017-- this suggests an annual time series. You should consistently indicate that you have data for 3 years throughout the manuscript to avoid

confusion: 1998, 2008, and 2017

Response: Yes, thank you for pointing this out. Throughout the manuscript this revision has been made.

P.5: I'm not sure what you mean by "clinically active?" How is this determined? My understanding is the BLS simply provides estimates of employed professionals.

Response: Thank you for pointing this out, clinically active has been deleted from the variable description.

P. 6: I don't think it makes sense to use the "ideal" terminology throughout the manuscript. Maybe AAPA model SOP? The term "ideal" makes a value judgement that undermines the author's basis for scientific argument.

Response: Thank you for this perspective. To prevent a value judgement terminology "ideal" has been changed to "permissive".

Reviewer: 2

Comments to the Author

I appreciate the opportunity to review this paper. It was interesting and addresses a chronically understudied topic. The paper offers new evidence on the relationship between physician assistant (PA) scope-of-practice (SOP) laws. Using data from three national datasets, the paper reports evidence that states with more permissive SOP laws have higher densities of PAs. The evidence offered in the paper is important, but I have a few concerns about the presentation and analysis that the authors may consider addressing. My comments follow in no particular order.

At various times throughout the paper (e.g., p. 4, l. 21; p. 9, l. 47), the authors discuss prior work on the supply or capacity of the healthcare workforce. The authors may consider citing an additional two studies that are directly relevant:

Stange, Kevin. 2014. "How does provider supply and regulation influence health care markets? Evidence from nurse practitioners and physician assistants." *Journal of Health Economics*, vol. 33, 1-17.

McMichael, Benjamin. 2018. "Beyond Physicians: The Effect of Licensing and Liability Laws on the Supply of Nurse Practitioners and Physician Assistants." *Journal of Empirical Legal Studies*, vol. 15(4), 732-771.

Response: Thank you for this good suggestion, the McMichael article was added to the references.

It would be helpful if the authors offered more details on the datasets they examine. Examining only three years of data is not necessarily a problem. However, it would be helpful to better understand this limitation. Are the BLS data not available during all years? Is the analysis limited to only years in which the AAPA demographic data were available?

Response: We appreciate the concern with our choice of retrospective cross-sectional analysis. This study along with our previous publication "State Scope of Practice Laws: An Analysis of Physician Assistant Programs and Graduates"¹ are to set the background for our further time-series analysis.

The way the paper reports its primary results with respect to changes in PAs per capita is somewhat unusual. The authors may consider reporting raw numbers or otherwise eliminating the use of these categories. Terms like "super expanders" and "contractors" made it harder to comprehend the results.

Response: Thank you for this comment and suggestion. The categories were added to help understand the change over time. Figure 3 was edited to show the median change by state.

The authors should consider changing the way they paper categorize PA SOP laws. The authors note that they follow existing literature in counting the number of six key elements a state has adopted and

categorizing each state as falling into three categories of laws based on this number. This may lead to inaccurate coding of legal schemes. This method essentially treats each of the six key elements as interchangeable and equal, but there is no particular reason to believe this is the case. For example, the use of licensure as a regulatory term may have essentially no impact on patient care. On the other hand, granting PAs full prescriptive authority and having scope of practice determined at the practice level are likely to meaningfully expand the ability of PAs to provide care to patients (and thus drive up demand for PAs). Treating either of these legal changes as interchangeable with licensure as a regulatory term may not be justified. Similarly, co-signature requirements may be relatively important, but removing legal caps on the number of PAs that a physician may supervise is more likely to contribute to the expansion of PAs per capita. In addition to these methodological concerns, the current legal categorization scheme offers little advice to policymakers on which elements matter most. Since they are all treated as interchangeable, policymakers will not know which ones make the most difference. This, in turn, may limit the usefulness of the paper. The authors may consider re-engineering how they categorize laws. They could either examine each constituent element by itself or decide which elements are most important and separate states into those that have adopted the most important elements and those that have not adopted all of those elements.

Response: Thank you for this comment and suggestion. The methodology of categorization of SOP elements in three categories builds upon the work of Perry Morgan³, Paul Wing² and Virginia Valentin¹ (references: page 5). We do agree that this methodology has limitations as pointed out by treating each element as equal. This limitation has been added to the limitations section in discussion and recommendation for future analysis to look at each element individually.

One explanation for the lack of an association between the number of PAs and SOP laws in 1998 may be the fact that, until the Balanced Budget Act of 1997, PAs were not able to bill Medicare independently. Many insurance companies had similar rules. Thus, even if a state had less restrictive SOP laws, PAs would still be restricted under federal law, which would explain the lack of an association in 1998. This changed after the Balanced Budget Act of 1997, which is consistent with the significant associations found in later years.

Response: Thank you for the comment, we definitely agree. There are so many factors that likely impact these results.

The paper states in the discussion section: “However, as of 2017 half of US states are defined as contractors or never adopters, with PA/population ratios below the national median” (p. 9, ll. 41-42). Why is this noteworthy? This seems like it should be true by construction. If this is not true by construction, I think more explanation would be helpful.

Response: Thank you for your question. This sentence was amended to be more clear to: However, as of 2017 half of US states are defined as contractors or never adopters which demonstrates that PA policy makers still have a long road ahead to assure that all PAs are working within the full scope of their license.

The paper also states in the discussion section: “This also highlights research that indicates that perhaps PA demand and daily work is not impacted as much by state SOP laws as organizational policies.” I do not believe the results of this paper support this statement. This paper analyzes three years of data, and the authors note that they cannot fully address all of the ways that SOP laws and the supply of PAs may interact. Such an analysis is not enough to conclude that organizational policies may be more important—this type of conclusion could only be drawn from a more throughgoing analysis of SOP laws or from a comparative analysis of SOP laws and organizational policies. The authors have not done either of these here. I do not mean to minimize the contribution this paper makes. I only mean to emphasize that this contribution is limited to the realm of SOP laws. The authors should consider eliminating the above language and other instances where they suggest that their results are sufficient to draw any conclusions about the relative importance of SOP laws and organizational policies.

Response: Thank you for point this out: this sentence was removed to eliminate a connection and inference of the findings from this paper.

VERSION 2 – REVIEW

REVIEWER	Timmons, Edward Joseph Saint Francis University
REVIEW RETURNED	23-Jan-2021

GENERAL COMMENTS	<p>I appreciate the attention paid to addressing my comments and concerns and I believe the manuscript has improved. My main concern with the revised manuscript is properly motivating this study. The authors are reluctant to perform a time series analysis and cite previous published work. Yet, this publication is not cited in the manuscript.</p> <p>To more fully explain their motivation for utilizing a cross sectional design and methods, I would suggest the authors cite their previous work and state more clearly within the manuscript how a cross sectional analysis contributes to our understanding of the effects of PA SOP. The author's previous work will also allow readers to better understand the possible mechanism for how SOP affects PA numbers-- aspiring PAs may be less interested in completing a degree if they are forced to work in a restricted practice environment.</p>
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REVIEWER	McMichael, Benjamin J The University of Alabama System
REVIEW RETURNED	03-Feb-2021

GENERAL COMMENTS	I enjoyed reading the revised version of this paper. It makes an important contribution to the existing literature. I appreciate the authors taking the concerns of the reviewers so seriously. I believe the authors have addressed all of my comments to the greatest extent possible. I hope to see the paper in print soon.
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VERSION 2 – AUTHOR RESPONSE

Reviewer 1:

Thank you for your comments and suggestion as it was an oversight to not cite our previous work. Please see in the first paragraph of methods section additional/revised sentences addressing cross-sectional work and citation of our previous work on this subject.

Reviewer 2:

Thank you.