

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	COLLABORATIVE PRACTICE TRENDS IN U.S. PHYSICIAN OFFICE VISITS: AN ANALYSIS OF THE NATIONAL AMBULATORY MEDICAL CARE SURVEY (NAMCS), 2007–2016
AUTHORS	Najmabadi, Shahpar; Honda, Trenton; Hooker, Roderick

VERSION 1 - REVIEW

REVIEWER	Bo Kyum Yang Towson University
REVIEW RETURNED	20-Nov-2019

GENERAL COMMENTS	<p>This is an important area to study since NPs and PAs are healthcare professionals who have a potential to alleviate a growing shortage of primary care providers in US. Nonetheless, this paper has a critical flaw related to the data that they used.</p> <p>1) They used NAMCS 2007 to 2016 to compare the trends between two defined time periods. Based on my search and my previous experience using this data, identifying visits to NPs and PAs can be problematic because the data only samples the physician office visits typically during regular physician office hours (which means that NPs and PAs who substitute for absence of physicians outside of the regular office hours will not be included in the data). This could introduce the bias particularly if they want to capture solo NP or PA practice.</p> <p>2) Another concern is it seems like NCHS changed the NAMCS data collection process around 2013 to capture NP and PA visits more accurately using a new question. This change would lead to the biased results when comparing two time periods (statistics from the first time period in their study was before the change was made by NCHS). The authors should clarify how they handled these issues in the data.</p> <p>3) It could have been better to provide how NP and PA visits, solo-NP, solo-PAs visits were measured in the data more clearly. what question or variable was used to capture this provider type?</p> <p>4) They used the term "collaborative practice" Did authors want to measure the practice independence of NPs or PAs from physicians or they wanted to measure "group practice or team practice" vs "solo practice"? Even if patients see only NPs/PAs in their visits (that is how the visit type is captured in the data), it does not mean NPs/PAs have complete practice independence from physicians. Although</p>
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	<p>"collaborative practice" was defined in the method section, it is still unclear how this can be applicable to a real clinical setting.</p> <p>4) Table 1a: authors could provide adjusted statistics using multivariate models after controlling for other factors in the table.</p> <p>5) I was looking for the interpretation in the discussion section on why all types of providers provide fewer prevent services compared to the previous time period. Could authors provide some insight on this?</p>
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REVIEWER	Ai Oishi University of Edinburgh, UK
REVIEW RETURNED	22-Nov-2019

GENERAL COMMENTS	<p>Thank you very much for submitting this manuscript and for giving me an opportunity to learn about this topic. Please note I am not a statistician neither using English as my first language. I am sorry if my comments were not clear enough because of the language and when statistic issues are involved.</p> <p>Major points</p> <ol style="list-style-type: none"> 1. Why did you pick up 2007-2016? It may be related to the time when the Patient Protection and Affordable Care Act was enacted, but not clear in the article. This may be self-explanatory for those who are familiar with the US health care system, but it is better to explain and justify for international readers. Also, why did you divide the 10-year time span into two? Wasn't enough to look at the trend in the 10-year period? 2. From the data you show in this manuscript, it is difficult to conclude that the collaborative practice is increased. I think you need to show absolute changes (of the proportion) of the dyad visits among all visits comparing two timespans. I am not knowledgeable enough in statistics, but there must be better ways to show the difference. <p>Minor points</p> <ol style="list-style-type: none"> 3. P5 L53: I think It's very valuable and that you demonstrated what we could do with publicly available data. 4. P6 L18-24: I'm sorry but I cannot fully comprehend why you particularly concentrated on visits to solo and group practices. It also looks like you also picked up private practices (which I understood from the supplemental figure 1). Could you provide why in particular picked up this type of visits? 5. P6 L36-41: If they were so small in number, why not exclude CNMs? 6. P6 L36: What does 'DO' stand for? 7. P6 L 42-46: It's better to have a reference for the definition of 'collaborative practice'. To my knowledge, the famous one is a WHO's definition, which is slightly different from your(?) definition here. Also, is it appropriate to say there is collaborative practice if
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two professionals are involved? I assume there are cases which multiple professionals are involved at different visits, but only one professional sees the patient at each visit.

8. I am not sure if a mosaic plot (Figure 1) is a best way to present the data from your study. Why did you choose a mosaic plot?

9. Supplemental Table 1: Isn't this table misleading? Why don't you show the percentages of each provider types in each time span (For example the percentage of Solo-PA in 2007-2011, as you did in Overall column)? I'm not sure if showing the percentage of visits in each timespan per each provider type would make any meaningful comparison. At least, it wouldn't be helpful to capture a trend within 10 years. (I'm probably not clear enough in English, but I hope you can understand!) Also, it's not clear what other collaborations mean. Could you provide an explanation?

10. P7L55 to P8L4: 'Of note, the proportion of..., across the timeframe.' I think showing the relative reduction of these visits (POV attended by a solo-PA or solo-NP) is misleading. The number of these visits are proportionally small, so looking at relative reduction leads to overestimation of its changed and values.

11. Figure 2: It is true that in 2015, but could you find any trends in change? Is it increasing or decreasing as a trend? Also, it's a bit weird to see annual changes only for percentage of POV with PA and/or NP.

12. Figure 4: I think you need better explanation of this figure. What is (Ref.) in the title? Also are you comparing 'solo' practice as 'solo-PA/NP' or 'solo-physician'? It's not clear to me.

13. P8 'Patient Characteristics', P9 'Major reason for visit' and 'Visit specialty': I would put 'patient characteristics' first before showing the results of number of visits per provider types. I understand that this section is to demonstrate the changes you observed in provider types are not due to the patient characteristics. Considering that, you probably can make these sections (particularly patient characteristics) simple and concise – to make your arguments clearer and delivered.

14. Table 1A & 1B: It might be better to add a 'Total' row at the bottom.

15. P13 L7-9: I don't think the data you presented in the result section supports the fact the collaborative practice has increased.

16. P13 L10-13: The raise in 2015 seems to be temporal, so it seems inappropriate to refer this raise here.

17. P13 L51-55: You didn't show the growth of the numbers of PAs and NPs in the observation period. So it doesn't explain the observed increase.

18. P14 L16-18: The reimbursement rate can have a significant impact. Did they change the rate during the observation period?

19. P14 L42: NHAMCS appears here for the first time. What does this stand for?

	<p>20. P14 L 40-46: This characteristic of the NAMCS (sampling physicians not clinics) should be explained in a method section.</p> <p>21. I think the discussion can be better organised for a convincing argument.</p>
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REVIEWER	John Mafi UCLA
REVIEW RETURNED	23-Nov-2019

GENERAL COMMENTS	<p>The authors describe a national analysis of trends in U.S. office-based visits by nurse practitioners (NPs), physician assistants (PA), and physicians using nationally representative data from the National Ambulatory Medical Care Survey. The authors find that the proportion of collaborative visits (NP plus physician or PA plus physician seeing the patient) has increased in recent years, while solo-NP or solo-PA visits has declined in recent years. This is an interesting and well-designed study, with important policy implications. The authors would do well to reassure the reader against the possibility of secular trends and other factors confounding their results. The writing clarity does need some improvement, particularly in the abstract. Overall I recommend accepting the paper pending revisions for this important work.</p> <p>Abstract</p> <ol style="list-style-type: none"> 1) It's not clear in the abstract why we split the trends into two separate analyses. This needs better framing. 2) Are these trends age-sex adjusted? What about changes in comorbidities over time? Increasing age and comorbidity of the US population could account for at least a part of these trends as older, more complex patients may require both an NP or PA and a physician in a team-based visit. 3) This sentence in the results: "When stratifying by provider type, we observed a trend away from preventive care visits among all providers." Does this mean that preventive visits declined among all providers? If so, I would rephrase it more simply and clearly, e.g., "Preventive visits declined among all provider types." 4) Second sentence of the results is confusing, and I believe it is referring to NP or PA solo visits rather than overall NP or PA visits– this should be stated very clearly. 5) First sentence of conclusion is not exactly supported by the data provided in the abstract and I would consider removing. We cannot accept that PA and NP collaboration has become an integral part of office-based care delivery without knowing the absolute visit rates in the abstract. We only see differences rather than actual rates. I do think the main results support a real increase in NP and PA collaboration with physicians during office visits and a real decline in NP or PA solo visits. 6) And are the numbers reported absolute differences or relative differences? The authors should clarify this as well. 7) Second sentence of conclusion is redundant and should be clarified again to say that NP and PA collaborative visits with physicians are increasing. Moreover, the abstract does not seem to address the overall trend of visits with a PA and NP with or without an MD – currently we do not know the answer to this question in the results section of the abstract (in Figure 1 it doesn't look like it
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changed significantly and I don't see a statistical test on this).

8) The third sentence in the conclusion is also unsupported by the results and is introducing new findings in the conclusion section. This should be moved to the results section. We don't see any results that stratify NPs and PAs by visit type or specialty in the results.

9) Overall, I suggest the authors ensure that there is a corresponding and clear explanation in the methods for each component of the results section in a 1:1 fashion and be very clear on the different categories and use the consistent terms for each provider category throughout the abstract and paper.

Main paper:

1) Methods: the authors report that NAMCS has improved its data collection of NPs and PAs in recent years – could this confound the trends we see? Can you reassure us that the nature of data collection remains fundamentally consistent throughout the study period?

2) Methods: did the analyses also account for the complex survey design, e.g., the strata and clustering variables? This is a very important methodological requirement when using NAMCS data.

3) Not sure I follow this limitation in the main findings section: "Restricting data to nonfederal visits by PAs or NPs are subject to underestimation until the 'incident to' clause for Medicare and Medicaid reimbursement is removed." This is more clearly explained in the discussion, however, I am not sure this applies to NAMCS data because they don't need to follow these Medicare billing rules when they collect the survey data from chart abstracting physician progress notes (and thus would be agnostic to billing requirements I would think). Instead, the main limitation of NAMCS is that it surveys physician office practices, not NP or PA practices, therefore, it underrepresents autonomous NPs and PAs nationally. Even still, it's one of the best national surveys we have on this topic. See for example: <https://www.ncbi.nlm.nih.gov/pubmed/17850531>.

4) Methods: The central objective of the paper seems to be to determine whether collaborative practice patterns among NPs, PAs, and physicians from 2012-2016 are significantly different from collaborative practice patterns from 2007-2011 in the context of changes in insurance reform. This is not at all clear in the abstract, as I don't see this objective framed, posed or answered in the abstract. Moreover, I don't understand which policy intervention they are referring to or why the authors chose these two timeframes as the ACA was passed in 2009. So wouldn't that make the 2007-2011 timeframe the main time period of interest? Why would the authors expect significant changes from 2012-2016 when compared with previous timeframes? Perhaps the authors are referring to the Medicaid expansion component of the ACA, which was rolled out in 2014? Or some ACA policy related to NPs/PAs rolled out from 2012-2016? In any case, the objective of the study needs far more clarity and details, particularly in the abstract.

5) Main findings: collaborative practice has increased in recent years while solo-PA or solo NP practice has decreased. This should be front and center in abstract results and conclusions.

6) The slight decrease in solo practice physicians seems to be a non-significant finding.

Discussion.

7) Discussion: How exactly was the ACA supportive of PAs and NPs? This would be very informative in terms of study framing.

	<p>Figures and Tables</p> <p>Table 1 is quite dense and hard to follow. I would try to simplify and focus (perhaps by bolding) on proportions and how they differ across the subgroups.</p> <p>Figures are quite clear and helpful. Figure 1 tells the entire story of the paper in one image. Would consider adding the proportion of NP/PA visits among all office-visits overall to Figure 1, though this is addressed in Figure 2. You could consider combining Figures 1 and 2 by breaking down the overall trend line into four components but this is up to the authors.</p> <p>Figure 3 shows a clear decline in preventive care visits. I wonder how much of this was due to the Choosing Wisely recommendation against these kinds of visits.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1
Bo Kyum Yang
Towson University

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

Please leave your comments for the authors below

This is an important area to study since NPs and PAs are healthcare professionals who have a potential to alleviate a growing shortage of primary care providers in US. Nonetheless, this paper has a critical flaw related to the data that they used.

1) They used NAMCS 2007 to 2016 to compare the trends between two defined time periods. Based on my search and my previous experience using this data, identifying visits to NPs and PAs can be problematic because the data only samples the physician office visits typically during regular physician office hours (which means that NPs and PAs who substitute for absence of physicians outside of the regular office hours will not be included in the data). This could introduce the bias particularly if they want to capture solo NP or PA practice.

Authors Reply: We are fully aware that NAMCS never sampled non-physician providers directly as part of the office-based component of NAMCS, and clarified it in the previous manuscript under METHODS (page 5, lines 35–42) and DISCUSSION (page 14, lines 36–40). In the revised manuscript, we clarified that results exclude 'non-physicians with independent patient daily rosters and those with independent practices'. Also we clarified that due to office-based physicians who do not employ non-physician providers, findings are subject to underestimation. However, we are confident this data report is reliable for the objective of this study, i.e. "characterizing ONLY TRENDS in evolving collaborative practices in physician offices". Expanding the NAMCS sampling methods to non-physician providers can enrich the reliability of the utilization of PAs and NPs.

We used a question on PROVIDERS captured in the NAMCS Survey Instrument, called "PATIENT RECORD FORM" 2007–2016. Below we inserted the images of this question from the 2007 and 2016 forms, (available at: https://www.cdc.gov/nchs/ahcd/ahcd_survey_instruments.htm). Since 2012, the option (NONE) has been added as the 7th possible answers. Since answers to the "PROVIDERS" question in 2006 and earlier are different than current versions (2007–2016), we limited our data to 2007–2016. More recent data were not available at the time of this submission.

11. PROVIDERS

Mark (X) all providers seen at this visit.

1 Physician

2 Physician assistant

3 Nurse practitioner/ Midwife

4 RN/LPN

5 Mental health provider

6 Other

PROVIDERS

Mark (X) all providers seen at this visit.

1 Physician

2 Physician assistant

3 Nurse practitioner/ Midwife

4 RN/LPN

5 Mental health provider

6 Other

7 None

2) Another concern is it seems like NCHS changed the NAMCS data collection process around 2013 to capture NP and PA visits more accurately using a new question. This change would lead to the biased results when comparing two time periods (statistics from the first time period in their study was before the change was made by NCHS). The authors should clarify how they handled these issues in the data.

Authors Reply: According to Lau, et al. (2016) "As supplies of NPs and PAs increase, there is a growing need to examine the clinical tasks they perform in ambulatory care settings. Starting in 2013, workforce items were added to the NAMCS induction interview on the clinical tasks that all healthcare staff performs in the physician's office". We did not use this question and it is beyond the scope of our study. Also one of the major changes in the NAMCS data collection process over time is related to the community health centers (CHCs). This component of NAMCS was added in 2006, and it samples up to 3 providers, whether NP, PA, nurse midwife or physician (confirmed from correspondences with the NCHS staff). Although CHCs data is a valid source for non-physician clinicians, our data is restricted to physician offices, because at the time of our study

- for the years 2006–2011, publicly available data did not include data on non-physician clinicians from CHCs (restricted data);
- since 2012, NCHS initiated a separate CHC sample of physicians and non-physician clinicians;
- and CHC data for 2014 and the further years were not released.

Also, CHCs are local, non-profit, community-owned healthcare organizations that serve low-income and medically underserved areas. Generally, CHCs patients are among the nation's most vulnerable populations. As such there is the possibility of

- overestimating patient visits at CHCs handled by non-physician clinicians;
- and CHCs patient's health status and demographic characteristics, as an important confounding factor, may influence the type of provider.
- See Hing E, Hooker RS, Ashman J. Primary health care in community health centers and comparisons with office-based practice. *Journal of Community Health*. 2011; 36(3): 406-413.

To avoid bias, we restricted our data to publicly available NAMCS data

- limited our data to physician offices;
- applied patient visits weight, not the physicians weight;
- added that the NAMCS (2007–2016) did not change our question of interest, i.e., ALL PROVIDERS per visit
- According to the NCHS, survey years with the same patient record form (survey instrument), where the same question of interest is asked can be combined.

(available at: https://www.cdc.gov/nchs/ahcd/ahcd_faq.htm;
https://www.cdc.gov/nchs/ahcd/documentation_updates.htm;
https://www.cdc.gov/nchs/ahcd/namcs_participant.htm).

We mentioned that as well in the revised paper.

3) It could have been better to provide how NP and PA visits, solo-NP, solo-PAs visits were measured in the data more clearly. what question or variable was used to capture this provider type?

Authors Reply: As discussed in question 1, the survey instrument (PATIENT RECORD FORM) has always had a section for any provider seen at a visit. Based on providers seen per visit, we considered below definitions:

Solo physician: only physician, not a PA or NP, irrespective of other providers

Solo PA: only PA, not a physician or NP, irrespective of other providers

Solo NP: only NP (including CNMW), not a physician or PA, irrespective of other providers

Dyad of Physician-PA: both physician and PA listed, not an NP, irrespective of other providers

Dyad of Physician-NP: both physician and NP listed, not a PA, irrespective of other providers

Other collaborations: (PA and NP) or (physician and PA and NP), irrespective of other providers

We added the same to the manuscript.

4) They used the term "collaborative practice" Did authors want to measure the practice independence of NPs or PAs from physicians or they wanted to measure "group practice or team practice" vs "solo practice"? Even if patients see only NPs/PAs in their visits (that is how the visit type is captured in the data), it does not mean NPs/PAs have complete practice independence from physicians. Although "collaborative practice" was defined in the method section, it is still unclear how this can be applicable to a real clinical setting.

Authors Reply: We did not measure the practice independence of NPs or PAs from physicians. This is a matter of state's scope of practice laws and the practice setting's term of employment where NPs and PAs practice (and probably only reliably done by personal observation of the providers). We only described the visit in terms of providers, including solo and team-based visits. Labor economists would like to capture large data to determine what may be causative and what each member of a team contributes to an outcome – but that is seldom possible with secondary data. Because we are using 8.5 billion weighted visits to conclude "collaboration" we are laying down a framework where time-motion experts can then decide what contribution each player makes. Your point is well taken, and we hope scholars will use this as a springboard for more granular investigations. Ours was an overview approach.

5) Table 1a: authors could provide adjusted statistics using multivariate models after controlling for other factors in the table.

Authors Reply: This is a descriptive study, and we did not look for any association between provider type (as an outcome) and the independent variables. However, as suggested, in the revised manuscript we reported the results of some adjusted statistics. For example, when adjusted for physician office visits' (POVs) patient age and number of chronic conditions along with their interaction, still the probability of team work (at least 2 providers per visit, including Physician-PA or Physician-NP and other collaborations) in years 2012-2016 compared to years 2007–2011 was significantly higher, OR 1.35, 95% CI 1.01, 1.79.

6) I was looking for the interpretation in the discussion section on why all types of providers provide fewer prevent services compared to the previous time period. Could authors provide some insight on this?

Authors Reply: We have given this observation a great deal of thought and have looked in the literature. Unfortunately, in this era of American medicine, such visits, for annual physical examinations, immunizations, smoking cessation, seat belt advice, and other such types of preventive care, seem to be less reimbursable and less central to a visit. We added a reference to support our conjecture.

Reviewer 2
Ai Oishi
University of Edinburgh, UK

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

Please leave your comments for the authors below

Thank you very much for submitting this manuscript and for giving me an opportunity to learn about this topic. Please note I am not a statistician neither using English as my first language. I am sorry if my comments were not clear enough because of the language and when statistic issues are involved.

Major points

1) Why did you pick up 2007-2016? It may be related to the time when the Patient Protection and Affordable Care Act was enacted, but not clear in the article. This may be self-explanatory for those who are familiar with the US health care system, but it is better to explain and justify for international readers. Also, why did you divide the 10-year time span into two? Wasn't enough to look at the trend in the 10-year period?

Authors Reply: We thank the reviewer for this comment. As the ACA was adopted over time and in different ways across states within the US, we divided the timeframe into two timespans to intelligibly and simply be able to present potential changes in collaborative practice that resulted from this legislation. Additionally, as the NAMCS is not a longitudinal dataset, but a sample of different providers each year, we felt that year to year changes in the sampling frame might introduce an inordinate amount of variability, whereas a longer-term average would be the more robust way to report this. We clarified this point in the manuscript.

2) From the data you show in this manuscript, it is difficult to conclude that the collaborative practice is increased. I think you need to show absolute changes (of the proportion) of the dyad visits among all

visits comparing two timespans. I am not knowledgeable enough in statistics, but there must be better ways to show the difference.

Authors Reply: We applied patient visit weights to all analyses. As suggested, in the revised manuscript, we reported the absolute changes. Also, when adjusted for physician office visits (POVs) patient age and number of chronic conditions and their interaction, the probability of teamwork (at least 2 providers per visit, including Physician-PA or Physician-NP and other collaborations) in years 2012-2016 compared to years 2007–2011 was significantly higher, OR 1.35, 95% CI 1.01, 1.79.

Minor points

3) P5 L53: I think It's very valuable and that you demonstrated what we could do with publicly available data.

Authors Reply: We thank the reviewer for this comment.

4) P6 L18-24: I'm sorry but I cannot fully comprehend why you particularly concentrated on visits to solo and group practices. It also looks like you also picked up private practices (which I understood from the supplemental figure 1). Could you provide why in particular picked up this type of visits?

Authors Reply: This is a nomenclature issue. We only looked at private practices. What our health services research language referred to when we said non-federal offices excludes the Veterans Administration, Indian Health Service, Military, federally supported clinics including rural clinics, etc. The Americans do not use "surgery" to mean private offices. Without universal healthcare, American healthcare delivery is a patchwork quilt of different types of clinics and offices, some federal, some state, some cities and counties, and many are private or part of large medical centers. Also as discussed in response to Question 2 of Reviewer 1, differences in patient characteristics of federally supported clinics with those of private practices might impact the results. Also sampling methods of our study setting is different with those of federally funded centers.

5) P6 L36-41: If they were so small in number, why not exclude CNMs?

Authors Reply: We explained this in the text. Per NCHS protocol the CNMs were collapsed into the NP pool. NCHS requires each cell have at least 35 observations. The percentage of CNM visits in POVs does not meet this requirement. CNMs are found in greater concentrations in multispecialty clinics and less and less in family medicine or general medicine practices.

6) P6 L36: What does 'DO' stand for?

Authors Reply: In America physicians are allopathic doctors (MDs) and osteopathic doctors (DOs). In the U.S. DO education and practice is indistinguishable from MDs (as compared to Commonwealth countries where they are not considered the same).

7) P6 L 42-46: It's better to have a reference for the definition of 'collaborative practice'. To my knowledge, the famous one is a WHO's definition, which is slightly different from your(?) definition here. Also, is it appropriate to say there is collaborative practice if two professionals are involved? I assume there are cases which multiple professionals are involved at different visits, but only one professional sees the patient at each visit.

Authors Reply: We thank the reviewer for this suggestion. The suggested WHO reference is now supplied in the text. We did not find that it significantly differed from our definition.

8) I am not sure if a mosaic plot (Figure 1) is a best way to present the data from your study. Why did you choose a mosaic plot?

Authors Reply: The mosaic plot was selected because it allowed us to visually depict the changes in proportions of preceptor visits from 2007-2011 to 2012-2016.

9) Supplemental Table 1: Isn't this table misleading? Why don't you show the percentages of each provider types in each time span (For example the percentage of Solo-PA in 2007-2011, as you did in Overall column)? I'm not sure if showing the percentage of visits in each timespan per each provider type would make any meaningful comparison. At least, it wouldn't be helpful to capture a trend within 10 years. (I'm probably not clear enough in English, but I hope you can understand!) Also, it's not clear what other collaborations mean. Could you provide an explanation?

Authors Reply: Other collaborations, as shown in response to Question 3 of Reviewer 1, is a triad of a physician, NP and PA, or a dyad of NP and PA. We added the same to the text. In supplemental table 1 for the total 10-year timespan and for the two 5-year timespans, both number and percentages, including 95% confidence interval have been reported. More importantly, the mosaic plot shows percentages for each timespan independent of the other time.

10) P7L55 to P8L4: 'Of note, the proportion of..., across the timeframe.' I think showing the relative reduction of these visits (POV attended by a solo-PA or solo-NP) is misleading. The number of these visits are proportionally small, so looking at relative reduction leads to overestimation of its changed and values.

Authors Reply: Your comment is appreciated, and we made changes in the text, reported the absolute values, including the adjusted odds ratios.

11) Figure 2: It is true that in 2015, but could you find any trends in change? Is it increasing or decreasing as a trend? Also, it's a bit weird to see annual changes only for percentage of POV with PA and/or NP.

Authors Reply: As this analysis is about care that occurs in physician offices, overall solo physician visits for the 10-year timespan is 93.1% (Supplemental Table 1). We intentionally, for better visibility, did not include the solo physician group. Figure 2 shows the temporal trend of percent of visits with a PA and NP with or without an MD. As added into the abstract and text, unadjusted 2007–2016 temporal percent of PAs and/or NPs present at a physician office visit indicates slight increasing trend ($P=0.0499$), with the highest annual percentage of POV with PA or NP solo or collaborative work was seen in 2015 (10.5%, 95% CI 6.2, 14.7), and the lowest in 2007 (5.5%, 95% CI 3.7, 7.3) and 2016 (5.6%, 95% CI 3.1, 8.1). When we adjust for the POV patient age and number of chronic conditions, the probability of a visit with presence of PAs or NPs indicate an OR of 1.027 (0.991, 1.064).

12) Figure 4: I think you need better explanation of this figure. What is (Ref.) in the title? Also are you comparing 'solo' practice as 'solo-PA/NP' or 'solo-physician'? It's not clear to me.

Authors Reply: Ref. = Reference. This forest chart shows the risk ratios of team work versus solo work (the reference group) per provider in each time span independently, stratified by the visit specialty (primary, medical, and surgical). We clarified this in the manuscript.

13) P8 'Patient Characteristics', P9 'Major reason for visit' and 'Visit specialty': I would put 'patient characteristics' first before showing the results of number of visits per provider types. I understand that this section is to demonstrate the changes you observed in provider types are not due to the patient characteristics. Considering that, you probably can make these sections (particularly patient characteristics) simple and concise – to make your arguments clearer and delivered.

Authors Reply: We simplified the result section as per your suggestion. The current order of topics let us specify these covariates per provider type.

14) Table 1A & 1B: It might be better to add a 'Total' row at the bottom.

Authors Reply: As per your suggestion and Reviewer 3 Question 17 under Figures and Tables, for more clarity, % (95% Confidence Interval) were bolded. Also we added totals per provider at the bottom. The total numbers are valid per provider per characteristic. However, the percentages in Total row is the percent of provider (out of the total visits). For more clarity, we avoided a total row per characteristics.

15) P13 L7-9: I don't think the data you presented in the result section supports the fact the collaborative practice has increased.

Authors Reply: To support unadjusted findings, we added into the text the distribution of POV patient's total number of chronic conditions. When adjusted for POVs' patient age and number of chronic conditions and their interaction, still the probability of team work (at least 2 providers per visit, including Physician-PA or Physician-NP and other collaborations) in years 2012-2016 compared to years 2007–2011 was significantly higher, OR: 1.35, 95% CI 1.01, 1.79. As due to office-based physicians who do not employ non-physician providers, our findings are subject to underestimation, in an improved NAMCS survey methods, sampling non-physician providers, a trend of higher probability of collaborative practice is warranted.

16) P13 L10-13: The raise in 2015 seems to be temporal, so it seems inappropriate to refer this raise here.

Authors Reply: Please see our answer to your Q 11. We revised the manuscript, accordingly.

17) P13 L51-55: You didn't show the growth of the numbers of PAs and NPs in the observation period. So it doesn't explain the observed increase.

Authors Reply: The number of PAs and NPs in 2014 and 2018 per Bureau of Labor Statistics (BLS) was added to the text. The BLS did not count NPs separately until 2013 so we could not use prior data. Association data (e.g., AAPA and AANP) is unreliable as they list those who state they are licensed (but may not be clinically active) while BLS lists those employed only as clinicians and excludes administrators, educators, etc. We were only interested in those who work as clinicians.

AAPA: American Academy of Physician Assistants
AANP: American Association of Nurse Practitioners

18) P14 L16-18: The reimbursement rate can have a significant impact. Did they change the rate during the observation period?

Authors Reply: Reimbursement is an important financial tool for documenting some labor inputs. We did not examine reimbursement in this project. However, we suggest if this is a topic of interest you may find the study by Benitez et al useful: Benitez J, Coplan B, Dehn RW, Hooker RS. Payment source and provider type in the US healthcare system. Journal of the American Academy of Physician Assistants. 2015; 28(3): 46-53.

19) P14 L42: NHAMCS appears here for the first time. What does this stand for?

Authors Reply: NHAMCS stands for National Hospital Ambulatory Medical Care Survey. We added the full term in the manuscript.

20) P14 L 40-46: This characteristic of the NAMCS (sampling physicians not clinics) should be explained in a method section.

Authors Reply: We have revised the wording in the Methods section.

21) I think the discussion can be better organised for a convincing argument.

Authors Reply: We have reorganized the discussion section as per the comment.

Reviewer 3
John Mafi
UCLA

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

Please leave your comments for the authors below

BMJ Open Review

The authors describe a national analysis of trends in U.S. office-based visits by nurse practitioners (NPs), physician assistants (PA), and physicians using nationally representative data from the National Ambulatory Medical Care Survey. The authors find that the proportion of collaborative visits (NP plus physician or PA plus physician seeing the patient) has increased in recent years, while solo-NP or solo-PA visits has declined in recent years. This is an interesting and well-designed study, with important policy implications. The authors would do well to reassure the reader against the possibility of secular trends and other factors confounding their results. The writing clarity does need some improvement, particularly in the abstract.

Overall, I recommend accepting the paper pending revisions for this important work.

Abstract

1) It's not clear in the abstract why we split the trends into two separate analyses. This needs better framing.

Authors Reply: We offer more information in the abstract and text as to why two time-periods were chosen (natural experiment before and after ACA, as a confounding factor). Explanations are also added as reply to Reviewer #2, Question 1: As ACA was adopted over time and in different ways across states within the US, we divided the timeframe into two timespans to intelligibly and simply be able to present potential changes in collaborative practice that resulted from this legislation. Additionally, as the NAMCS is not a longitudinal dataset, but a sample of different providers each year, we felt that year to year changes in the sampling frame might introduce an inordinate amount of variability, whereas a longer-term average would be the more robust way to report this.

2) Are these trends age-sex adjusted? What about changes in comorbidities over time? Increasing age and comorbidity of the US population could account for at least a part of these trends as older, more complex patients may require both an NP or PA and a physician in a team-based visit.

Authors Reply: Age and sex were not adjusted. Comorbidities have changed over time in the US with some aging. However, recent analysis has shown that life expectancy has decreased over the last three years (due to various causes including suicide, drug use, and violence). No data or study has emerged that more complex patients may require joint PA/NP-MD visits (See Morgan PA, Smith VA, Berkowitz TSZ, et al. Impact of physicians, nurse practitioners, and physician assistants on utilization and costs for complex patients. *Health Affairs*. 2019; 38(6): 1028-1036). However, as per your suggestion, we added into the text the distribution of POV patient's total number of chronic conditions. When adjusted for POVs' patient age and number of chronic conditions and their interaction, still the probability of team work (at least 2 providers per visit, including Physician-PA or Physician-NP and other collaborations) in years 2012-2016 compared to years 2007-2011 was significantly higher, OR: 1.35, 95% CI 1.01, 1.79.

3) This sentence in the results: "When stratifying by provider type, we observed a trend away from preventive care visits among all providers." Does this mean that preventive visits declined among all providers? If so, I would rephrase it more simply and clearly, e.g., "Preventive visits declined among all provider types."

Authors Reply: We made changes in the abstract and text per your suggestion. Thank you.

4) Second sentence of the results is confusing, and I believe it is referring to NP or PA solo visits rather than overall NP or PA visits— this should be stated very clearly.

Authors Reply: we added clarification to this sentence.

5) First sentence of conclusion is not exactly supported by the data provided in the abstract and I would consider removing. We cannot accept that PA and NP collaboration has become an integral part of office-based care delivery without knowing the absolute visit rates in the abstract. We only see differences rather than actual rates. I do think the main results support a real increase in NP and PA collaboration with physicians during office visits and a real decline in NP or PA solo visits.

Authors Reply: Absolute visit rates per solo and collaborative work of PAs and NPs were added to the abstract and manuscript.

6) And are the numbers reported absolute differences or relative differences? The authors should clarify this as well.

Authors Reply: We clarified it in the abstract and manuscript, and it is now reported as absolute difference.

7) Second sentence of conclusion is redundant and should be clarified again to say that NP and PA collaborative visits with physicians are increasing. Moreover, the abstract does not seem to address the overall trend of visits with a PA and NP with or without an MD – currently we do not know the answer to this question in the results section of the abstract (in Figure 1 it doesn't look like it changed significantly and I don't see a statistical test on this).

Authors Reply: We revised this section in CONCLUSION. Figure 2 shows the temporal trend of percent of visits with a PA and NP with or without an MD. As added into the abstract and text, unadjusted 2007–2016 temporal percent of all forms of PAs and/or NPs present at a physician office visit indicates slight increasing trend ($P=0.0499$), with the highest annual percentage of POV with PA or NP solo or collaborative work was seen in 2015 (10.5%, 95% CI 6.2, 14.7) and the lowest in 2007 (5.5%, 95% CI 3.7, 7.3), and 2016 (5.6%, 95% CI 3.1, 8.1). When we adjust for the POVs' patient age and number of chronic conditions, the probability of higher visits with a PA or NP, with or without an MD is insignificant (OR: 1.03, 95% CI 0.99, 1.06). Of note, our results exclude 'non-physicians with independent patient daily rosters and those with independent practices'. Also due to office-based physicians who do not employ non-physician providers, findings are subject to underestimation. Adding state's scope of practice laws and the practice setting's term of NPs and PAs employment make any prediction on this temporal trend more complicated, beyond the objective of our study.

8) The third sentence in the conclusion is also unsupported by the results and is introducing new findings in the conclusion section. This should be moved to the results section. We don't see any results that stratify NPs and PAs by visit type or specialty in the results.

Authors Reply: We clarified this findings in the result.

9) Overall, I suggest the authors ensure that there is a corresponding and clear explanation in the methods for each component of the results section in a 1:1 fashion and be very clear on the different categories and use the consistent terms for each provider category throughout the abstract and paper.

Authors Reply: Thank you. We revised the paper accordingly.

Main paper

10) Methods: the authors report that NAMCS has improved its data collection of NPs and PAs in recent years – could this confound the trends we see? Can you reassure us that the nature of data collection remains fundamentally consistent throughout the study period?

Authors Reply: According to Lau, et al. (2016) “As supplies of NPs and PAs increase, there is a growing need to examine the clinical tasks they perform in ambulatory care settings. Starting in 2013, workforce items were added to the NAMCS induction interview on the clinical tasks that all healthcare staff performs in the physician’s office”. We did not use this question and it is beyond the scope of our study. Also one of the major changes in the NAMCS data collection process over time is related to the community health centers (CHCs) component of NAMCS, which was added to the traditional NAMCS in 2006, and it samples up to 3 providers, whether NP, PA, nurse midwife or physician (from correspondences with the NCHS staff). Our data is restricted to physician office visits because:

- for the years 2006–2011, regular publicly available data did not include data on non-physician clinicians (only available through restricted data application);
- since 2012, NCHS initiated a separate CHC sample of physicians and non-physician clinicians;
- and CHC data for 2014 and beyond were not released.

As mentioned above, CHCs are local, non-profit, community-owned healthcare organizations that serve low-income and medically underserved areas, and CHCs patients are among the nation’s most vulnerable populations, there is the possibility of

- overestimating patients visits handled by non-physician clinicians;
- and CHCs patient’s health status and demographic characteristics, as an important confounding factor, may influence the type of provider.

Thus, to avoid bias, we restricted our data to publicly available NAMCS data.

- restricted our data to physician offices;
- applied patient visits weight, not the physicians weight;
- and the NAMCS (2007–2016) did not change our question of interest, i.e., PROVIDERS seen per visit in PATIENT RECORD FORM
- According to the NCHS, survey years with the same patient record form (survey instrument), where the same question of interest is asked can be combined.

(available at: https://www.cdc.gov/nchs/ahcd/ahcd_faq.htm;
https://www.cdc.gov/nchs/ahcd/documentation_updates.htm;
https://www.cdc.gov/nchs/ahcd/namcs_participant.htm).

We mentioned that as well in the revised paper. By presenting publicly available data we invite scholars to repeat our analyses for validation purposes.

11) Methods: did the analyses also account for the complex survey design, e.g., the strata and clustering variables? This is a very important methodological requirement when using NAMCS data.

Authors Reply: Yes, besides adding the visits’ weight factor (not the weight for physicians), we accounted for the complex survey design, and included the NCHS provided strata and cluster variables. This was clarified in the STATISTICAL ANALYSIS section.

12) Not sure I follow this limitation in the main findings section: “Restricting data to nonfederal visits by PAs or NPs are subject to underestimation until the ‘incident to’ clause for Medicare and Medicaid reimbursement is removed.” This is more clearly explained in the discussion; however, I am not sure this applies to NAMCS data because they don’t need to follow these Medicare billing rules when they collect the survey data from chart abstracting physician progress notes (and thus would be agnostic to billing requirements I would think). Instead, the main limitation of NAMCS is that it surveys physician office practices, not NP or PA practices, therefore, it underrepresents autonomous NPs and PAs

nationally. Even still, it's one of the best national surveys we have on this topic. See for example: <https://www.ncbi.nlm.nih.gov/pubmed/17850531>.

Authors Reply: We appreciate the reviewer's suggestion and have modified the limitations section as recommended.

13) Methods: The central objective of the paper seems to be to determine whether collaborative practice patterns among NPs, PAs, and physicians from 2012-2016 are significantly different from collaborative practice patterns from 2007-2011 in the context of changes in insurance reform. This is not at all clear in the abstract, as I don't see this objective framed, posed or answered in the abstract. Moreover, I don't understand which policy intervention they are referring to or why the authors chose these two timeframes as the ACA was passed in 2009. So wouldn't that make the 2007- 2011 timeframe the main time period of interest? Why would the authors expect significant changes from 2012-2016 when compared with previous timeframes? Perhaps the authors are referring to the Medicaid expansion component of the ACA, which was rolled out in 2014? Or some ACA policy related to NPs/PAs rolled out from 2012-2016? In any case, the objective of the study needs far more clarity and details, particularly in the abstract.

Authors Reply: Overall, availability of data and our question of interest i.e., "provider type per visit" framed the study timeframe, 2007-2016 (10 years). This is a descriptive study, and we did not look for the causal effect of ACA. It's difficult to define a sharp cutoff point for the ACA and apparently, we cannot see ACA impact in the same year it was passed. Also, as you mentioned, ACA policy related issues came to effect in different years. Thus based on our expert consultant suggestion we chose the defined time-frames.

14) Main findings: collaborative practice has increased in recent years while solo-PA or solo NP practice has decreased. This should be front and center in abstract results and conclusions.

Authors Reply: We clarified it in the abstract, results and conclusions.

15) The slight decrease in solo practice physicians seems to be a non-significant finding.

Authors Reply: We concur. The statistical evidence of this not being significant is included (P=.17, Supplemental Table 1).

16) Discussion: How exactly was the ACA supportive of PAs and NPs? This would be very informative in terms of study framing.

Authors Reply: This is a policy point. The ACA funded a Health Resources and Services Administration program titled – "Expansion of PA Training (EPAT) Program". This ACA clause provided \$32 million in funding for Federal fiscal years 2010 through 2014 for PA education. The program had a five-year budget and project period.

Figures and Tables

17) Table 1 is quite dense and hard to follow. I would try to simplify and focus (perhaps by bolding) on proportions and how they differ across the subgroups.

Authors Reply: As per your suggestion (and Reviewer #2 Question 14), for more clarity, % and (95% Confidence Interval) were bolded. In addition, we added totals per provider at the bottom. The total numbers are valid per provider per characteristic. However, the percentages in Total row are percent of provider (out of the total visits). For simplicity, we avoided a total row per characteristics.

18) Figures are quite clear and helpful. Figure 1 tells the entire story of the paper in one image. Would consider adding the proportion of NP/PA visits among all office-visits overall to Figure 1, though this is addressed in Figure 2. You could consider combining Figures 1 and 2 by breaking down the overall trend line into four components but this is up to the authors.

Authors Reply: Thank you for the suggestion. For visual clearness, Figure 1 excludes physicians (above 92%), but Figure 2 is based on the total visit. We will stay with the current figures.

19) Figure 3 shows a clear decline in preventive care visits. I wonder how much of this was due to the Choosing Wisely recommendation against these kinds of visits.

Authors Reply: According to the ABIM, Choosing Wisely mission (2012) is to promote conversations between “clinicians and patients” by helping patients choose care that is truly necessary, supported by evidence, and not duplicate of other care already received (available at: www.choosingwisely.org/our-mission/). Thus, patient health education and their engagement in healthcare system, as well as patients’ out of pocket spending can lead to declining trends in preventive care. Coverage limits applied by the insurance companies..., can play its restrictive role too. In some vertically integrated organizations such as Kaiser Permanente and The Veterans Health Administration such initiatives are in place as suggestions or guides for provider-patient interaction. This ABIM initiative is an interesting topic worthwhile pursuing but beyond the scope of this study. As Dr. Mafi is an expert on this topic he might consider an editorial accompanying this publication.

VERSION 2 – REVIEW

REVIEWER	Bo Kyum Yang Towson University
REVIEW RETURNED	01-Jan-2020

GENERAL COMMENTS	<p>This is an important study but as I mentioned in the previous review, the NAMCS data itself cannot capture the valid national representative collaborative practice among physicians, NPs, and PAs in the physician offices and the findings that the author presented in the paper were significantly underestimated (it says only 1-2% of NP or PA visits in physician offices). But, if the purpose was only to compare TRENDS between two time periods, it may work. I appreciate that they included this limitation in their paper. Other descriptive statistics that the authors described in the result section could be misleading, though (particularly, the proportion of each solo NP, PA, dyad....etc, possibly removed from the manuscript?).</p> <p>The authors introduced "natural experiment" in this revised paper (the method section in the abstract). Did they test the policy effect (ACA) in this paper? I have not seen any related statistics to test this effect in the method section at all (at least I expected to see the</p>
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	<p>difference in differences analysis if they tested), and I am unsure what they meant by "natural experiment".</p> <p>Another question is about collaborative practice: The authors provided additional information regarding this term, but still one question remains.</p> <p>If the authors defined "collaborative practice" as the visits seen by both NP/PA and physicians at the same visit, they could clarify how their findings can be interpreted in a real world setting. According to the data documents the authors cited, the solo practice visits seen by either NP or PA were captured from the physician offices in which NPs or PAs are employed and work alongside with physicians (a group practice setting). Isn't the solo practice of NPs or PAs employed in a group practice setting (to fill the unavailability of physicians) more beneficial and efficient than the visits seen by two types of providers: both NP/PA and physician at the same time which was defined as collaborative practice in this paper (at least in terms of efficiency and accessibility of healthcare services)? What does collaborative practice vs solo practice in this paper actually mean in a real setting?</p>
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REVIEWER	John Mafi UCLA; United States
REVIEW RETURNED	15-Jan-2020

GENERAL COMMENTS	<p>Overall comment: Manuscript is both responsive to reviewers' comments and are much improved. Congratulations on an excellent paper. I recommend accepting the manuscript, pending some minor revisions.</p> <p>Physician-PA visits: the increase in physician-PA collaborative visits is non-significant, and so the reader not reading the whole paper will reject the conclusion in the abstract because it concludes that PA-physician collaborative visits are increasing yet $p=0.46$. Therefore, I suggest you incorporate the other finding (currently not shown in the abstract) that shows that the collaborative visits in general (either NP + physician or PA + physician) are increasing ($p<0.01$) as the abstract should stand alone as drawing valid conclusions from the results shown in the abstract. For instance, I would add in this sentence: "Overall, this suggests that collaborative practice, in particular Physician-NP, has increased in recent years (2012–2016) ($p <.01$), while visits handled by a solo-PA or solo-NP seem to have decreased ($P <.01$)" into the results of the abstract and to make room I would remove the part about the natural experiment of assessing the impact of the ACA from the methods section of the abstract.</p> <p>Minor typo: I replaced "was increased" with "has increased" in the quoted sentence above.</p> <p>ACA: I think I am still unclear on why ACA would be expected to increase collaborative practice arrangements. It's also not clear when the ACA took effect specific to NPs and PAs. While the ACA increased demand for primary care services overall, that might be expected to boost NP and PAs participation in primary care overall; however, it's still unclear to me why it would differentially impact</p>
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	collaborative visits over solo visits. The only other ACA-related factor the authors note is boosting funding for NP and PA education, which again would not be expected to impact collaborative visits, or at least not right away. If mechanism and ACA timing is still unclear, I would remove mention of a natural experiment of the ACA from the methods of the abstract. To me, it's not clear why the ACA should be a causative factor; instead I would simply stick to your hypothesis in the objective section of the abstract and just say that you hypothesize that collaborative arrangements are increasing over time (and that the causes of this need warrant further investigation). Ultimately as the authors prefer to keep this study more descriptive, assessing the impact of the ACA becomes more experimental and the parameters of the experiment are still unclear. Certainly, you can speculate in the discussion about perhaps the ACA being a driver and that more research is needed.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name

Bo Kyum Yang

Institution and Country

Towson University

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

None to declare

Please leave your comments for the authors below

This is an important study but as I mentioned in the previous review, the NAMCS data itself cannot capture the valid national representative collaborative practice among physicians, NPs, and PAs in the physician offices and the findings that the author presented in the paper were significantly underestimated (it says only 1-2% of NP or PA visits in physician offices). But, if the purpose was only to compare TRENDS between two time periods, it may work. I appreciate that they included this limitation in their paper. Other descriptive statistics that the authors described in the result section could be misleading, though (particularly, the proportion of each solo NP, PA, dyad....etc, possibly removed from the manuscript?).

1) The authors introduced "natural experiment" in this revised paper (the method section in the abstract). Did they test the policy effect (ACA) in this paper? I have not seen any related statistics to test this effect in the method section at all (at least I expected to see the difference in differences analysis if they tested), and I am unsure what they meant by "natural experiment".

Authors Reply: We agree with the reviewer's observation. This is a descriptive study and we did not assess causality. The abstract and manuscript were revised, and the term "natural experiment" or any sentence suggesting a causal link were deleted.

2) Another question is about collaborative practice: The authors provided additional information regarding this term, but still one question remains. If the authors defined "collaborative practice" as the visits seen by both NP/PA and physicians at the same visit, they could clarify how their findings can be interpreted in a real world setting. According to the data documents the authors cited, the solo practice visits seen by either NP or PA were captured from the physician offices in which NPs or PAs are employed and work alongside with physicians (a group practice setting). Isn't the solo practice of NPs or PAs employed in a group practice setting (to fill the unavailability of physicians) more beneficial and efficient than the visits seen by two types of providers: both NP/PA and physician at the same time which was defined as collaborative practice in this paper (at least in terms of efficiency and accessibility of healthcare services)? What does collaborative practice vs solo practice in this paper actually mean in a real setting?

Authors Reply: We agree with the reviewer that it is unclear how and why an increase in collaborative practice has emerged, and whether this increase efficiency and/or accessibility of healthcare services. Our data analysis reveals an organizational activity that is not well described in the literature – shared or "collaborative" visits. National shortages of physicians suggest that PAs and NPs who can work independently are needed. But what is the nature of collaborative visits – sequential providers for the same diagnosis or different diagnosis? Is there some synergy in such a dual visit or is this inefficiency of provider productivity? Is this an MD assisting a new graduate or enhancing insurance reimbursement? We are intrigued by this national observation and have amended the manuscript to suggest this is as an area for further investigation. Additionally, we have amended the discussion section to outline numerous specific possible explanations, including 1) changes in practice characteristics to emphasize team based care, 2) changes in patient populations served, whereby the age and complexity of the average patient seen by a provider has increased, which may increase the number of people needed on a visit to provide ideal care, 3) disproportionate growth of the PA, NP, and physician workforce over time, and 4) reimbursement requirements unique to the United States which may incentivize increased visits physically seen by multiple provider types. We suggest the stage is set to examine this growing healthcare delivery observation.

Reviewer: 3

Reviewer Name

John Mafi

Institution and Country

UCLA; United States

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

None declared.

Please leave your comments for the authors below

Overall comment: Manuscript is both responsive to reviewers' comments and are much improved. Congratulations on an excellent paper. I recommend accepting the manuscript, pending some minor revisions.

1) Physician-PA visits: the increase in physician-PA collaborative visits is non-significant, and so the reader not reading the whole paper will reject the conclusion in the abstract because it concludes that PA-physician collaborative visits are increasing yet $p=0.46$. Therefore, I suggest you incorporate the other finding (currently not shown in the abstract) that shows that the collaborative visits in general (either NP + physician or PA + physician) are increasing ($p<0.01$) as the abstract should stand alone as drawing valid conclusions from the results shown in the abstract. For instance, I would add in this sentence: "Overall, this suggests that collaborative practice, in particular Physician-NP, has increased in recent years (2012–2016) ($p <.01$), while visits handled by a solo-PA or solo-NP seem to have decreased ($P <.01$)" into the results of the abstract and to make room I would remove the part about the natural experiment of assessing the impact of the ACA from the methods section of the abstract.

Minor typo: I replaced "was increased" with "has increased" in the quoted sentence above.

Authors Reply: We thank the reviewer for this observation. The abstract and manuscript have been revised accordingly.

2) ACA: I think I am still unclear on why ACA would be expected to increase collaborative practice arrangements. It's also not clear when the ACA took effect specific to NPs and PAs. While the ACA increased demand for primary care services overall, that might be expected to boost NP and PAs participation in primary care overall; however, it's still unclear to me why it would differentially impact collaborative visits over solo visits. The only other ACA-related factor the authors note is boosting funding for NP and PA education, which again would not be expected to impact collaborative visits, or at least not right away. If mechanism and ACA timing is still unclear, I would remove mention of a natural experiment of the ACA from the methods of the abstract. To me, it's not clear why the ACA should be a causative factor; instead I would simply stick to your hypothesis in the objective section of the abstract and just say that you hypothesize that collaborative arrangements are increasing over time (and that the causes of this need warrant further investigation). Ultimately as the authors prefer to keep this study more descriptive, assessing the impact of the ACA becomes more experimental and the parameters of the experiment are still unclear. Certainly, you can speculate in the discussion about perhaps the ACA being a driver and that more research is needed.

Authors Reply: We agree and believe our study sets the stage for examining this organizational observation. Because this is a descriptive study and we did not assess any causality we agree that overemphasizing the potential impact of the ACA might not appropriately characterize the likely multifaceted underlying causes for the observations we observe. Accordingly, the abstract and manuscript were revised, and the terms "natural experiment" or a sentence reflecting a causality were

deleted. Furthermore, we limited the discussion of the ACA to the discussion and ensured that we included this as a speculative potential factor only.

VERSION 3 - REVIEW

REVIEWER	John Mafi UCLA USA
REVIEW RETURNED	10-Feb-2020

GENERAL COMMENTS	<p>Paper is much stronger.</p> <p>Minor comments:</p> <p>ABSTRACT</p> <p>Given use of significance vs. non-significance throughout the paper, I would restate sentence on page 3, line 30 in the abstract results in the following way: "Likewise, the rate of POVs with a collaborative physician-PA (1.98% vs 2.34%, P=0.46) increased non-significantly and the rate of POVs with a collaborative physician-NP (0.49% vs. 0.97%, P <.01) increased." Also, there is an extra period and space in abstract results on line 35.</p> <p>INTRODUCTION</p> <p>I would consider replacing seniors with older adults.</p> <p>Same with abstract results, given use of significance and non-significance throughout the paper, I might restate sentence on page 9, line 38 in the results in the following way: "Likewise, the rate of POVs with a collaborative physician-PA (1.98% vs 2.34%, P=0.46) increased non-significantly and the rate of POVs with a collaborative physician-NP (0.49% vs. 0.97%, P <.01) increased."</p> <p>DISCUSSION</p> <p>Page 15, line 9: "Simultaneously, there has been a significant shift in the reason for visits handled by a PA or NP or in a collaborative practice" rather than concluding a change, I would restate more precisely the finding of interest here, e.g., fewer preventive and pre/post-surgical visits, etc.</p>
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VERSION 3 – AUTHOR RESPONSE

Reviewer: 3
Reviewer Name
John Mafi

Institution and Country
UCLA; United States

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

None declared.

Please leave your comments for the authors below
Paper is much stronger.

Minor comments:

ABSTRACT

Given use of significance vs. non-significance throughout the paper, I would restate sentence on page 3, line 30 in the abstract results in the following way: "Likewise, the rate of POVs with a collaborative physician-PA (1.98% vs 2.34%, $P=0.46$) increased non-significantly and the rate of POVs with a collaborative physician-NP (0.49% vs. 0.97%, $P <.01$) increased."

Authors Reply: We thank the reviewer for this suggestion. Considering the word limit of 300, we revised this section accordingly. However, we used the exact same suggested sentence in the result section.

Also, there is an extra period and space in abstract results on line 35.

Authors Reply: Extra period and space were deleted.

INTRODUCTION

I would consider replacing seniors with older adults.

Authors Reply: Seniors was replaced with older adults.

Same with abstract results, given use of significance and non-significance throughout the paper, I might restate sentence on page 9, line 38 in the results in the following way:

"Likewise, the rate of POVs with a collaborative physician-PA (1.98% vs 2.34%, $P=0.46$) increased non-significantly and the rate of POVs with a collaborative physician-NP (0.49% vs. 0.97%, $P <.01$) increased."

Authors Reply: This sentence was revised accordingly.

DISCUSSION

Page 15, line 9: "Simultaneously, there has been a significant shift in the reason for visits handled by a PA or NP or in a collaborative practice" rather than concluding a change, I would restate more precisely the finding of interest here, e.g., fewer preventive and pre/post-surgical visits, etc.

Authors Reply: This sentence was revised accordingly:

"At the same time there have been fewer preventive and pre/post-surgical visits recorded at physician offices."