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eLetter exchange between Lee C. Rogers, DPM, director of the American Board of Podiatric Medicine; D. Scot Malay, DPM et al.; and Ettore Vulcano, MD et al., authors of a *JBJS* study titled “Surgeon Type and Outcomes After Inpatient Ankle Arthrodesis and Total Ankle Arthroplasty” (<http://dx.doi.org/10.2106/JBJS.17.01555>).

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**Lee C. Rogers, DPM**

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**Marc F. Swiontkowski, MD, Editor**

The Journal of Bone and Joint Surgery  
editorial@jbjs.org

February 15, 2019

Re: Chan, et al., JBJS 2019;101:127-135

Dear Editor,

I want to bring your attention to the above referenced article that the title and abstract misrepresent the results, there is selective reporting of the data, and failure to disclose authors' conflicts of interest.

**Misrepresentation of Results**

1. The title "Surgeon Type and Outcomes After Inpatient Ankle Arthrodesis and Total Ankle Arthroplasty" misrepresents the study because there are no "outcomes" reported in the article. Outcomes research in medicine typically focuses on the quality of care. For a study evaluating ankle arthrodesis (AA) or total ankle arthroplasty (TAA), patient-centered outcomes would be post-operative pain, infection, non-union, range of motion, activities of daily living, and lifespan of the implant.
2. The abstract is misleading. The following statement in the abstract conclusion is false. "An increasing trend in the proportion of procedures performed by podiatrists was coupled with apparent increases in length of stay and cost compared with procedures performed by orthopedic foot and ankle surgeons." The statement refers to "procedures," which implies to both procedures in the title (TAA and AA) performed by foot and ankle orthopedic surgeons versus a podiatrist, when the study results found there to only be a cost difference with AA.
3. The discussion states "Hospitalization costs increased over time, particularly for ankle arthrodesis performed by podiatrists, suggesting an increasing cost burden on the population level." This statement misrepresents the data because when the trends of

the costs of total ankle care (TAA + AA) are considered, the trend line of increasing cost over time are similar for both foot and ankle orthopaedic surgeons and podiatrists (Appendix A).

### Selective Reporting of Data

1. Central to the theme of this manuscript is that there is an increasing trend of podiatrists performing more TAAs and AAs compared to foot and ankle orthopedic surgeons from 2011 to 2016 and that podiatrist-performed procedures are more costly. However, the data presented in Tables I and II show that this trend is clearly fictitious. For TAA in 2011, the number of unknown surgeon types was as many as the number of podiatrists, creating a fictitiously low proportion of podiatrists performing TAA as a percent of the whole and an erroneous trend as reported in the abstract and the study results (Appendix B). For AA in 2011, there were more unknown surgeon types than podiatrists resulting in the same fictitious trend (Appendix C). Furthermore, there is an obvious trend of orthopaedic foot and ankle surgeons performing fewer, less-expensive AAs and increasing their utilization of more-expensive TAAs, thus actually increasing patient-level costs (Appendix D). Since foot and ankle orthopaedic surgeons performed significantly fewer AAs over time, it also inflated the percent of podiatrist-performed AAs. As patient-centered health outcomes were not reported in this study, there is no way to determine if that trend of foot and ankle orthopaedic surgeons to do more expensive TAAs in lieu of AAs is more cost-effective.
2. In the analysis, it appears as if the data has been manipulated to create "matched cohorts". Table I and Table II reveal that more than half of orthopedic cases were excluded from the study but only 10% of podiatry cases were excluded (Appendix E). Additionally, the exclusion of all outpatient procedures subjects the cost data to bias because the costs associated with outpatient procedures are generally less than the total costs of inpatient procedures.

### Conflicts of Interest Disclosure

1. I have concerns that not all the authors have disclosed their true conflicts of interest. The paper's senior author, Ettore Vulcano, MD, is a foot and ankle orthopedic surgeon and a member of the American Orthopaedic Foot and Ankle Society (AOFAS), which actively pursues policies that limit the practice of podiatric surgery and opposes parity within the healthcare system.<sup>1</sup> The authors' bias is evident in the study's stated hypothesis, "We hypothesized that orthopaedic foot and ankle surgeons, in light of longer and more encompassing training in orthopaedics and medicine, would have better outcomes and less resource utilization than podiatrists."

The JBJS Conflict of Interest policy states that "Authors are required to reveal any conflicts of interest when they submit the manuscript, using the form published by the International Council of Medical Journal Editors (ICMJE)." The ICMJE form asks for authors to report "financial relationships with entities in the biomedical arena that could

be perceived to influence, or that give the appearance of potentially influencing, what you wrote in the submitted work. You should disclose interactions with ANY entity that could be considered broadly relevant to the work.”

We cannot be naive to the fact that a perceived conflict of interest exists when a foot and ankle orthopedic surgeon and member of the AOFAS conducts a study criticizing podiatric education and competency to the benefit of their own practice and potential financial gain. Dr. Vulcano reported “nothing to disclose” on the ICMJE form.<sup>2</sup> The manuscript suggests making policy changes by considering surgeon type associated with resource utilization that would financially benefit foot and ankle orthopedic surgeons.

And, in fact, this same data set has been used by some of the same authors and in those manuscripts the authors’ disclosed conflicts of interest including their membership in relevant medical societies and associations.<sup>3,4</sup>

While professional turf wars are common, they tend to be limited to the professional associations’ lobbying efforts or marketing campaigns and do not usually grace the pages of respected scientific journals like JBJS.

I am sure you believe, as I do, that journals have a duty to protect readers from misleading work. The Committee on Publication Ethics (COPE) states that “journal editors should consider retracting a publication if they have clear evidence that the findings are unreliable, either as a result of misconduct (e.g. data fabrication) or honest error (e.g. miscalculation or experimental error).”<sup>5</sup>

It appears as if the authors selectively reported the data to prove their hypothesis, which may have financial benefits for at least one of the authors. I am hopeful *The Journal* and Editorial Board will consider these points and take appropriate action to ensure the accuracy of the scientific record.

Sincerely,



Lee C. Rogers, DPM

*\*Academic and professional affiliations noted for identification and disclosure purposes only*

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# Appendix to the Letter to the Editor of JBJS authored by Lee C. Rogers, DPM on February 15, 2019

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## Surgeon Type and Outcomes After Inpatient Ankle Arthrodesis and Total Ankle Arthroplasty

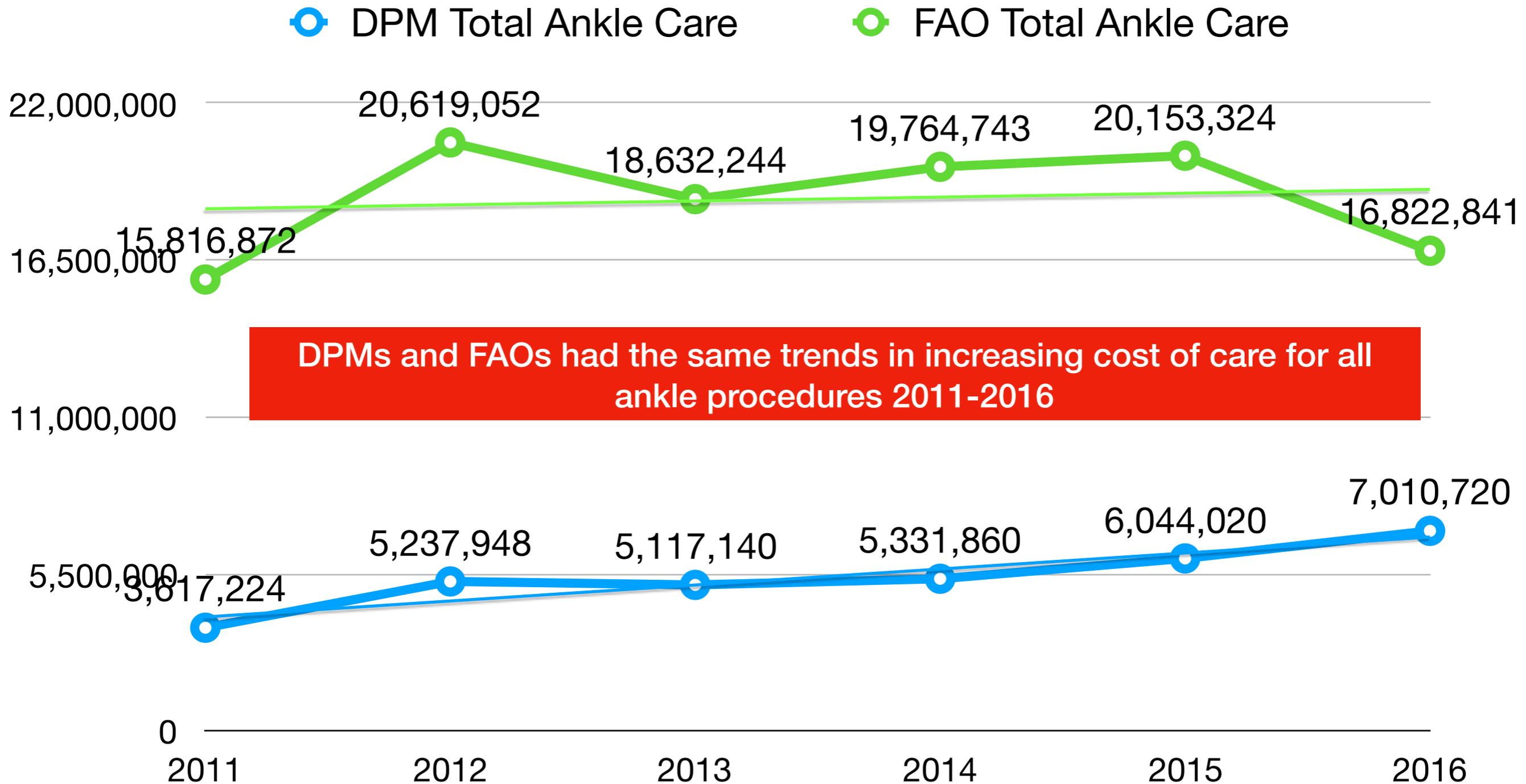
A Retrospective Cohort Study Using the Nationwide Premier Healthcare  
Claims Database

Jimmy J. Chan, MD, Jesse C. Chan, Jashvant Poeran, MD, PhD, Nicole Zubizarreta, MPH, Madhu Mazumdar, PhD, and  
Ettore Vulcano, MD

*Investigation performed at the Icahn School of Medicine at Mount Sinai, New York, NY*

# APPENDIX A

## FAO and DPM Trends in Costs Total of Ankle Care

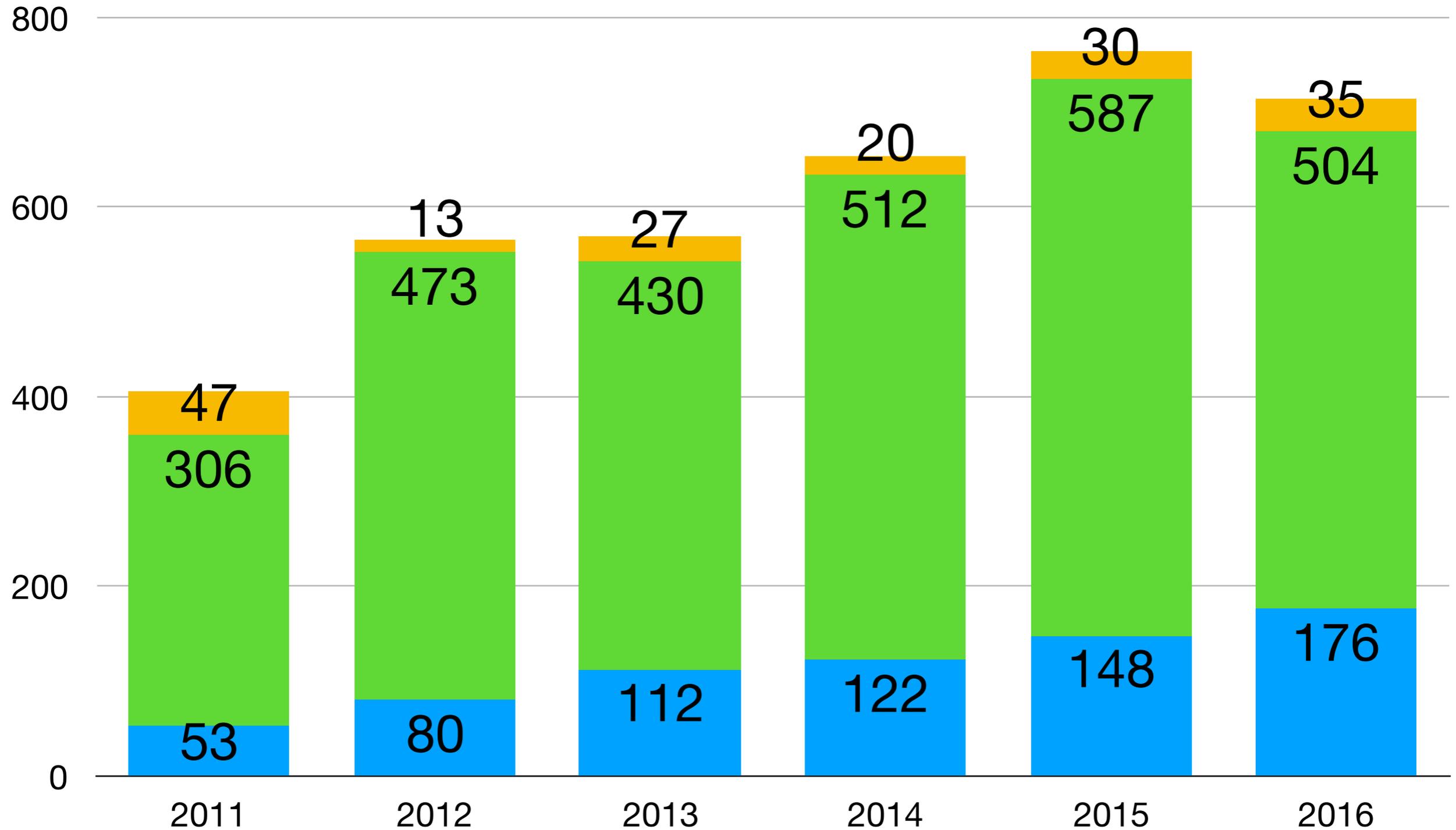


\*This graph represents all ankle procedures added together and separated by DPM and FAO

# APPENDIX B

## Numbers of Procedures - Unmatched TAA

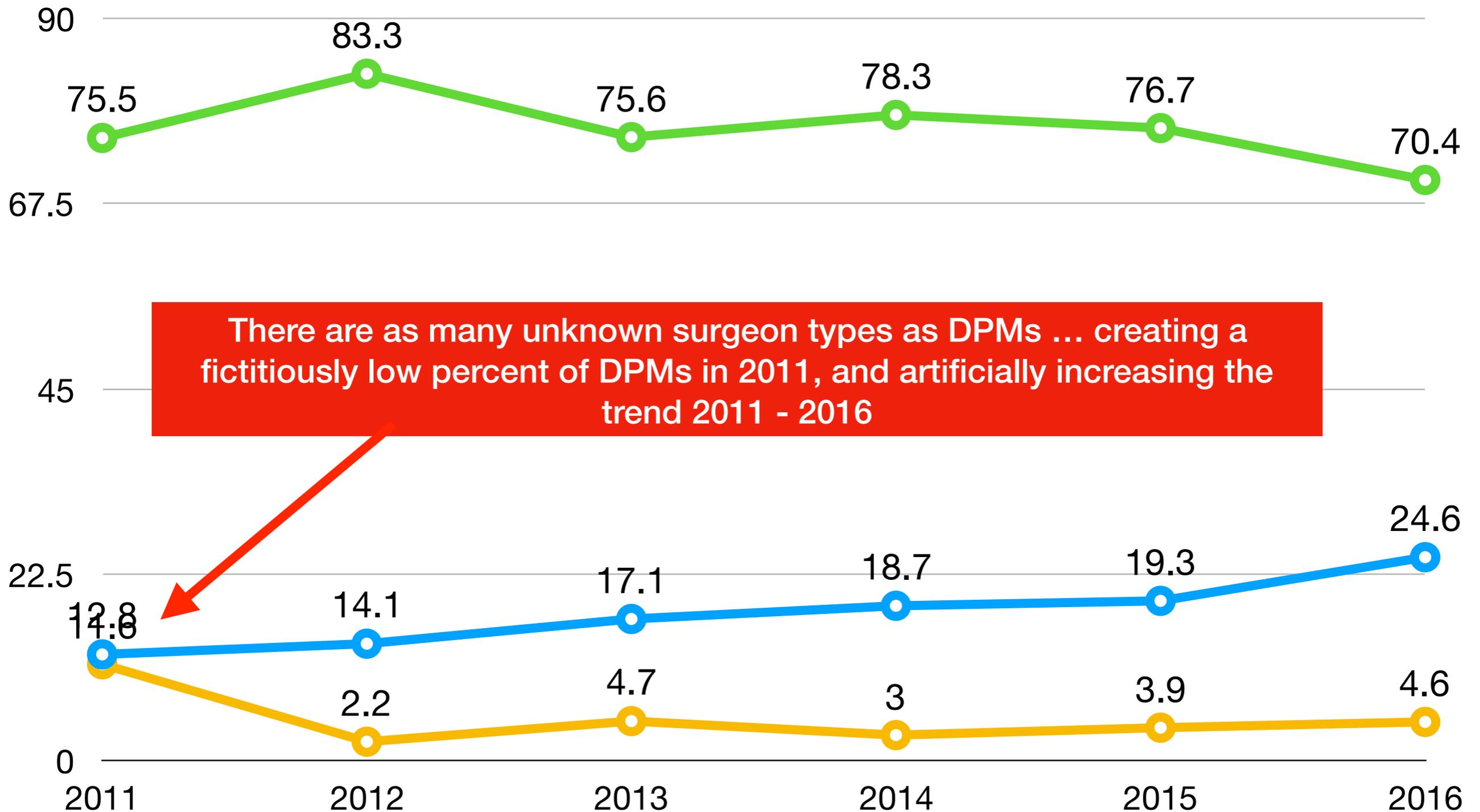
■ DPM      ■ FAO      ■ Unk



# APPENDIX B

## Percent of Procedures - Unmatched TAA

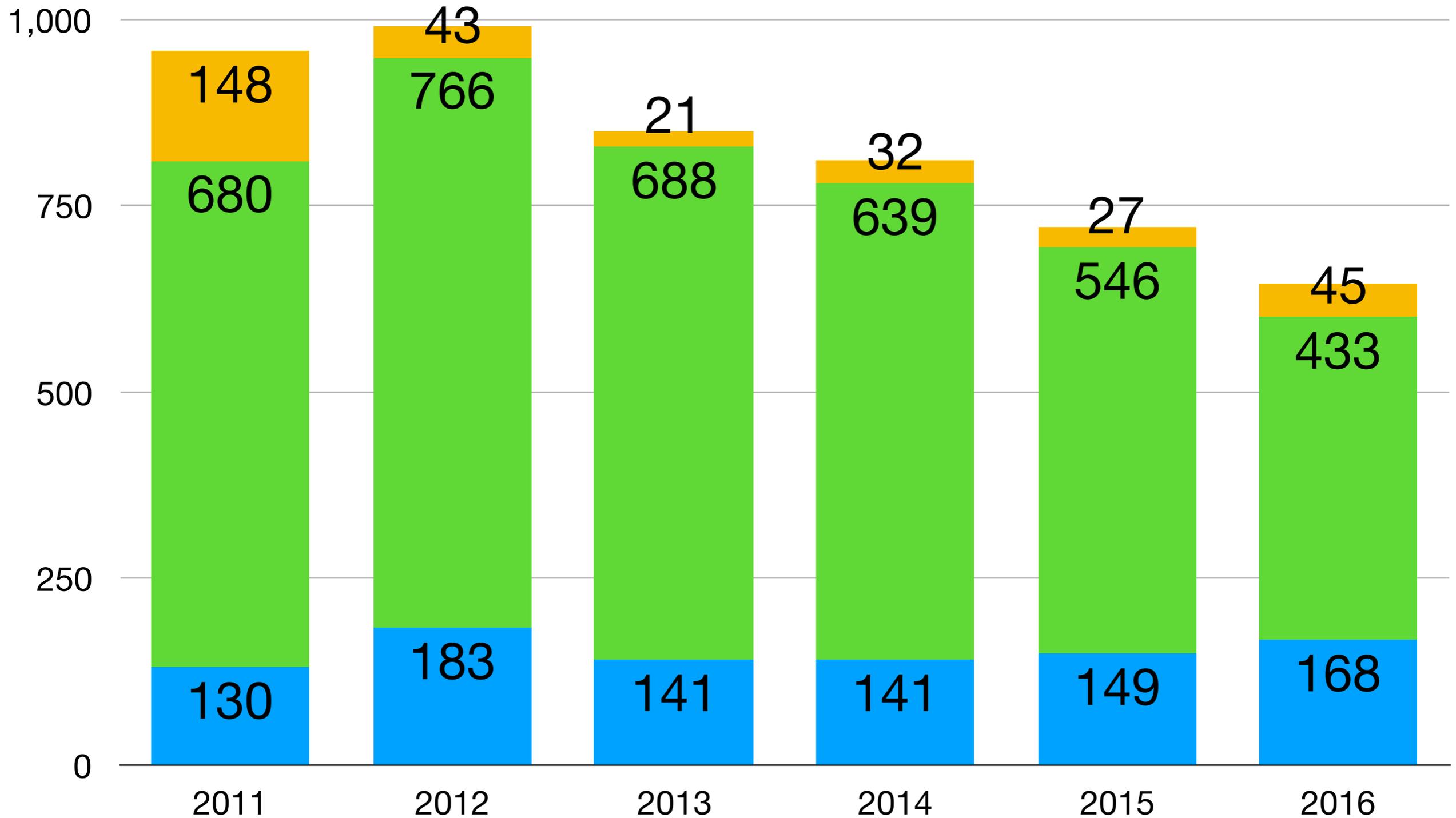
○ DPM      ○ FAO      ○ Unk



# APPENDIX C

## Numbers of Procedures - Unmatched AA

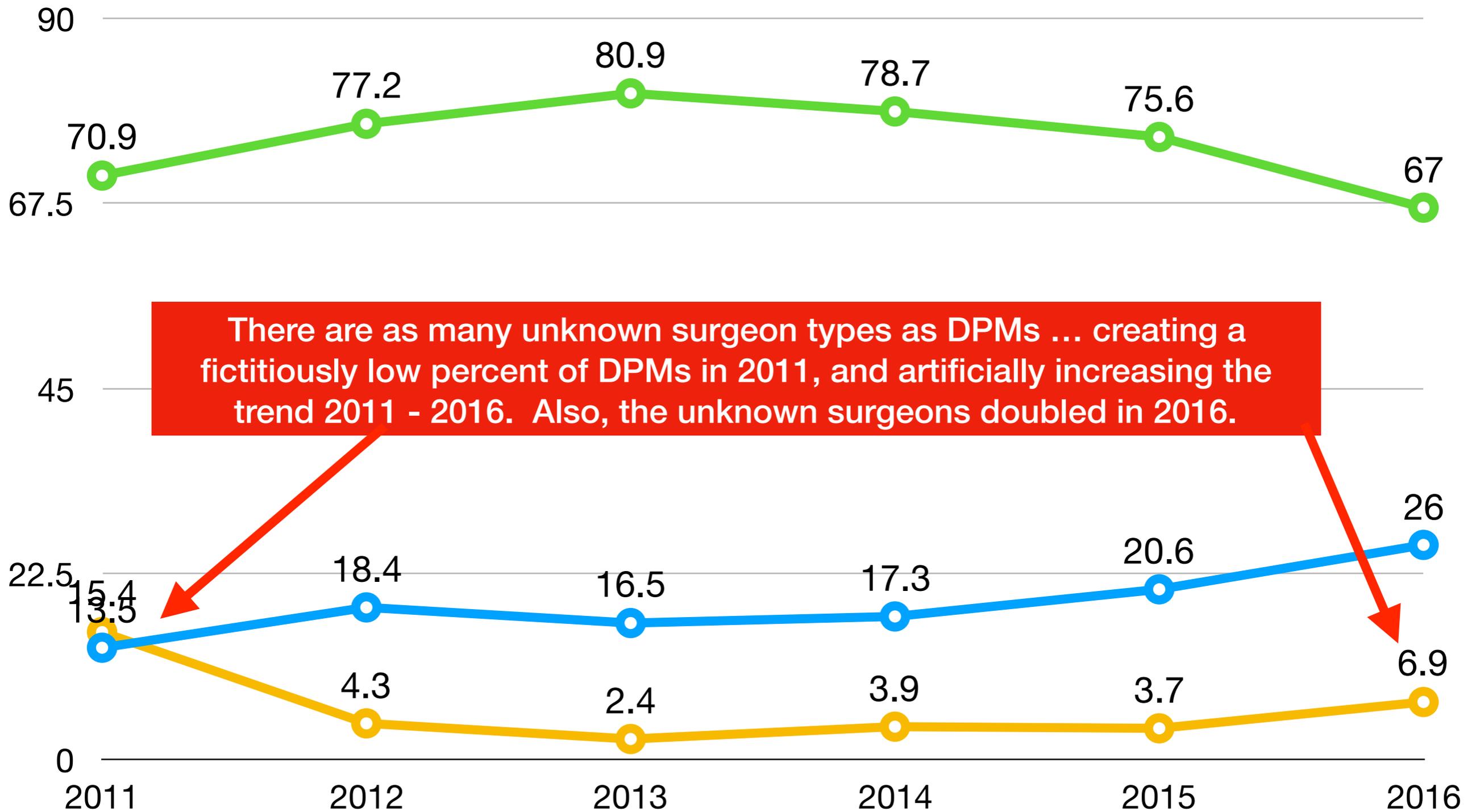
■ DPM      ■ FAO      ■ Unk



# APPENDIX C

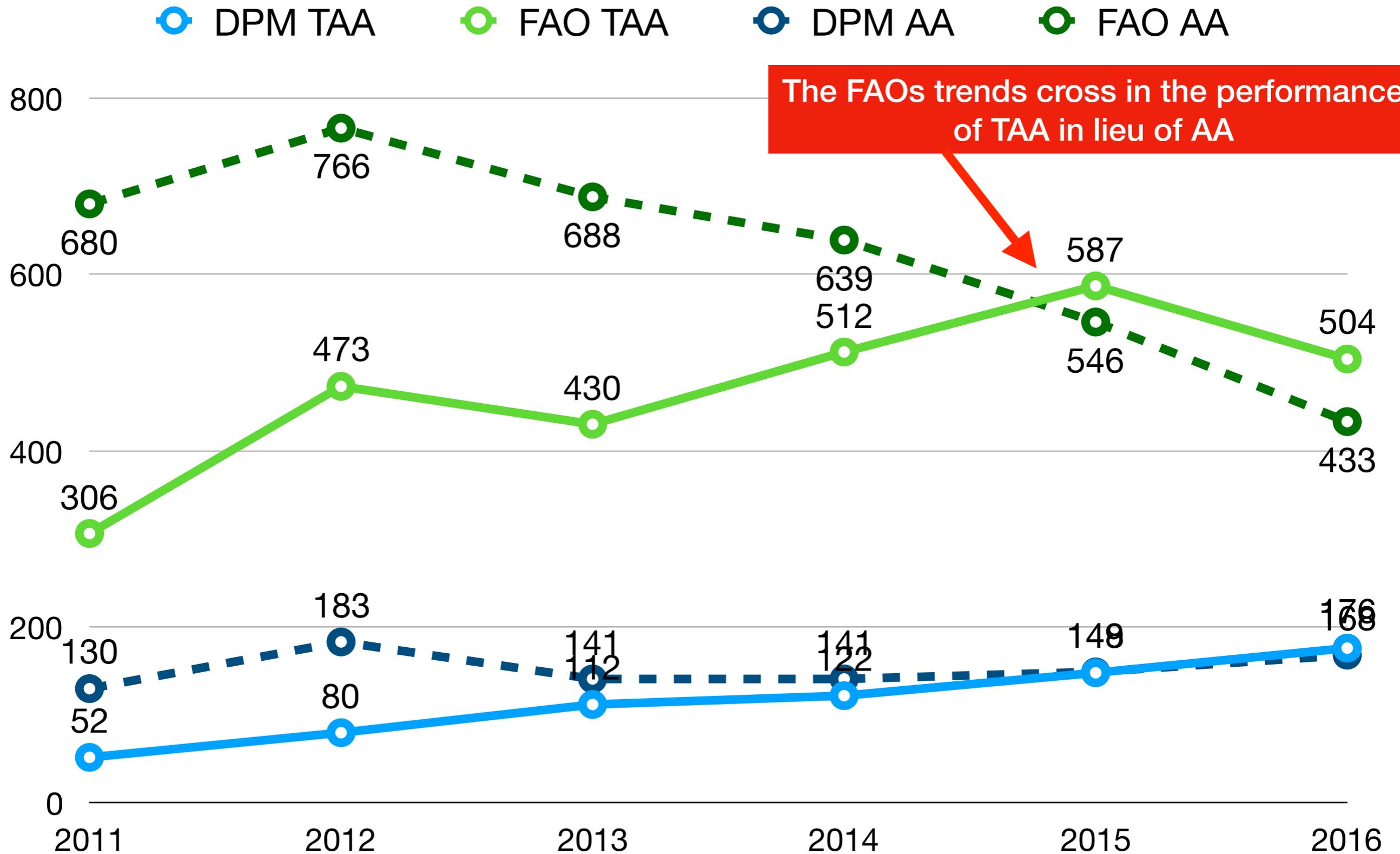
## Percent of Procedures - Unmatched AA

○ DPM      ○ FAO      ○ Unk



# APPENDIX D

## FAO and DPM Trends in TAA and AA



# APPENDIX D

## FAO and DPM Trends in TAA and AA

The previous slide clearly shows that the trend is that FAOs are replacing less expensive procedures, Arthrodesis (\$13,433) with a more expensive one Arthroplasty (\$21,838), whereas DPMs have the same rate of increase in both Arthrodesis (\$19,236) and Arthroplasty (\$21,472)

# APPENDIX E

The Podiatrist sample size was reduced by 62 (8.9%) and the FAO sample was reduced by 1583 (56.3%) to create the matched cohort.

**TABLE I Covariates and Outcomes by Type of Provider for TAA Unmatched and Propensity-Score-Matched Cohorts\***

	Unmatched				Matched		
	Podiatrist, N = 690	Orthopaedic Foot/Ankle Surgeon, N = 2,812	Unknown, N = 172	STD	Podiatrist, N = 628	Orthopaedic Foot/Ankle Surgeon, N = 1,229	STD
Patient demographics							
Age† (yr)	61 (54-70)	65 (58-72)	62 (55-71)	0.216	63 (55-70)	64 (57-70)	0.090
Sex				0.141			0.028
Female	304 (44.1)	1,439 (51.2)	70 (40.7)		289 (46.0)	583 (47.4)	
Male	386 (55.9)	1,373 (48.8)	102 (59.3)		339 (54.0)	646 (52.6)	
Race/ethnicity				0.188			0.037
White	577 (83.6)	2,442 (86.8)	158 (91.9)		530 (84.4)	1,053 (85.7)	
Black	37 (5.4)	78 (2.8)	3 (1.7)		24 (3.8)	42 (3.4)	
Other	76 (11.0)	292 (10.4)	11 (6.4)		74 (11.8)	134 (10.9)	

The Podiatrist sample size was reduced by 92 (10.1%) and the FAO sample was reduced by 1907 (50.8%) to create the matched cohort.

**TABLE II Covariates and Outcomes by Type of Provider for Ankle Arthrodesis Unmatched and Propensity-Score-Matched Cohorts\***

	Unmatched				Matched		
	Podiatrist, N = 912	Orthopaedic Foot/Ankle Surgeon, N = 3,752	Unknown, N = 316	STD	Podiatrist, N = 820	Orthopaedic Foot/Ankle Surgeon, N = 1,845	STD
Patient demographics							
Age† (yr)	55 (46-64)	60 (50-68)	58 (49-66)	0.163	56 (46-65)	57 (47-66)	0.057
Sex				0.074			0.043
Female	478 (52.4)	1,774 (47.3)	148 (46.8)		428 (52.2)	923 (50.0)	
Male	434 (47.6)	1,978 (52.7)	168 (53.2)		392 (47.8)	922 (50.0)	
Race/ethnicity				0.100			0.052
White	674 (73.9)	2,987 (79.6)	243 (76.9)		612 (74.6)	1,418 (76.9)	
Black	102 (11.2)	300 (8.0)	27 (8.5)		88 (10.7)	179 (9.7)	
Other	136 (14.9)	465 (12.4)	46 (14.6)		120 (14.6)	248 (13.4)	

Dear Editor:

Critical appraisal of the report by Chan et al.(1) led us to question the conclusions, which, in our view, seemed biased, overstated, and influenced by unmeasured exposures. Despite our agreement with your published comments regarding the report (2), we wanted to point out additional concerns that we had.

Importantly, the report did not include, and the authors did not aim to measure, patient-oriented quality of life or cost-effectiveness, so no meaningful conclusions can be made about patient-level outcomes or whether or not surgery by one type of surgeon is more or less cost-effective depending on specialty.

Unfortunately, the raw data are not in a public repository where they could be analyzed by interested readers, even though the guidelines the authors said they followed recommended doing so (3). We appreciate the value of maintaining equipoise during the design and execution of a clinical investigation, and it seems to us that the report was conducted as a means to an end and not necessarily with scientific skepticism. Below, we note 10 critical concerns that we think threaten the validity of the conclusions made by the authors:

1. Citation of literature (4) published >20 years ago to make a point about differences in training conveys a lack of understanding of the changes in foot and ankle surgical training since that time. Inspection of CPME 320 (5), which describes the elements of training in the podiatric realm, makes clear the attention that hindfoot and ankle surgery receive.
2. Small differences in costs and readmissions were presented as meaningful, whereas they were just as likely to be statistical artifacts related to large samples (statistically overpowered and trivial in practice).
3. The median 1-day increased length of stay (LOS) could actually be a matter of just a few hours if a patient was not ready for discharge before noon, and could reasonably be confounded by, for example, fusing an ankle in a neuropathic diabetic patient (which is probably more prevalent in the podiatric cohort), or by systematic variables associated with other hospital services, such as physical therapy and/or social services, or other exposures not under the surgeon's direct control. It is also possible that orthopaedists in the cohort got their patients out of the hospital sooner due to greater use of postsurgical admission to rehabilitation facilities, the costs for which were not considered.
4. Hospital costs for surgical implants and devices are subject to contracts between hospitals and vendors, who typically "carve out" items used by podiatric surgeons, which get billed at higher costs since they are not subject to the discounts afforded to the larger orthopedic service. This common financial arrangement could reasonably explain the observed cost differences.
5. In the propensity score (PS) model, the decision to match 1:3 DPM:MD/DO likely increased bias; whereas a 1:1 or 1:2 or variable ratio would have minimized bias (6), and this could explain why some of the observed differences increased after matching. Furthermore, the PS matching comparisons between podiatric versus orthopedic surgeons are invalid since there were significant differences in measured baseline covariates and known confounders between the two groups post-PS matching, which violates PS matching assumptions.
6. We believe that numerous biasing confounding variables (such as those mentioned above and others) probably compromised the instrumental variable analysis, a method known to be sensitive to confounding, such that meaningful conclusions could not be made.
7. Point estimates for the multiple variable analyses differed substantially (>45% for some comparisons), making conclusions based on these findings dubious. Moreover, reporting p-values for each of the sensitivity analyses is misleading to readers and exacerbates the multiplicity limitations intrinsic to multiple regression.
8. The omission of a sensitivity analysis that designated the "unknown providers" as alternating DPM and MD/DO within the main effect analysis was curious, since the summary statistics suggest that such inclusion would bias toward the null hypothesis and likely eliminate all significant differences.
9. Without seeing the data set, we wonder how much was actually missing and precisely how the authors accounted for the variability introduced by selecting a value for each missing data point in the multiple imputation model, a shortcoming that could impart bias for or against either group of surgeons depending on how the imputation was defined.
10. Coding errors commonly permeate large data sets, and could have influenced both groups, as did exclusion of outpatient procedures performed in the hospital. For instance, it would be interesting to know how many outpatient (lower hospital cost) arthroscopic AAs with percutaneous fixation, an approach often used by podiatric surgeons, were included in the 3,407 unanalyzed procedures.

In sum, the results described by Chan et al. were likely due to unmeasured systematic variables that were unrelated to patient-level outcomes and that biased the findings against podiatric surgeons. Our key concern with the report is that the LOS and cost differences not be construed as having anything to do with differences in outcomes at the

patient level; and, as presented, the small differences that were observed could be explained by systematic differences that were not elucidated and that would bias toward the null.

Podiatric and orthopedic surgeons should: 1) work with hospitals and vendors to assure that the items used in surgery cost the same regardless of the surgeon's service; 2) refine perioperative interventions and planning that expedite discharge from the hospital such that delays are avoided and associated nonhospital services are appropriately used; and, 3) investigate the additional outpatient costs incurred by patients that undergo TAA or AA.

Rigorous cost-effectiveness analyses pertaining to these common procedures would also be welcome additions to the surgical literature. We also propose that further scientific investigations be jointly undertaken by surgeons from both podiatric and orthopedic specialties, since, as Chan et al. said, we are "complementary specialties," and our common aim is to improve outcomes for our mutual patients.

Respectfully,

D. Scot Malay, DPM, MSCE, FACFAS  
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Jeremy Cook DPM, MPH, FACFAS  
Michael S. Downey, DPM, FACFAS  
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Christopher F. Hyer, DPM, MS, FACFAS  
Warren S. Joseph, DPM  
Andrew J. Meyr, DPM, FACFAS  
Barry Rosenblum, DPM, FACFAS  
Amol Saxena, DPM, FACFAS

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**In Response:**

Jashvant Poeran, MD PhD<sup>1,2</sup>; Jimmy Chan, MD<sup>1</sup>; Jesse Chan<sup>1</sup>; Nicole Zubizarreta, MPH<sup>2</sup>;

Madhu Mazumdar, PhD<sup>2</sup>; Ettore Vulcano, MD<sup>1</sup>

<sup>1</sup>Leni and Peter W. May Department of Orthopaedic Surgery, Icahn School of Medicine at Mount Sinai, New York, NY

<sup>2</sup>Institute for Healthcare Delivery Science, Department of Population Health Science and Policy / Department of Orthopaedic Surgery / Department of Medicine, Icahn School of Medicine at Mount Sinai, New York, NY

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We welcome the thoughtful comments by Dr. Rogers and Dr. Malay et al. in reply to our study<sup>1</sup> and appreciate any opportunity for constructive academic discourse. However, we feel that there may have been some misunderstandings regarding some of the wording in our manuscript and as a result a misunderstanding of the academic intent of our study.

In response to Dr. Rogers, first, we respectfully disagree that “*outcomes*” should always equal “*patient-centered outcomes*.” There are many other outcomes that may not be directly relevant to patients but will be relevant to providers, policymakers, and other stakeholders, such as cost.<sup>2</sup> Of note, we do provide univariable comparisons for other outcomes such as readmission; however, we were not able to include this in multivariable models given their low prevalence.

Second, “*procedures*” in the abstract conclusion referred to the multitude of procedures included in our study, the unit of observation in our dataset, not to the two different procedures, which we would have indicated as “*both types of procedures*.”

Third, our trend analysis was specifically stratified by procedure type as prespecified in our methods. The fact that combining both procedure types shows no difference in cost trends between surgeon type does 1) still indicate an “*increasing cost burden on the population level*” and 2) dismisses the value of stratified studies. In line with this, knowledge about increasing US healthcare costs is informative; however, to prioritize policies it is imperative to identify the main drivers behind this increase.

Fourth, Dr. Rogers is correct in his evaluation of the importance of the missing information on surgeon type, particularly in 2011. However, several statements are reflective of unproven assumptions, as we do not know to which surgeon group these missing cases belong. Therefore, without this knowledge it is incorrect to state that this creates a “*fictitiously low proportion of podiatrists.*” This word of caution also applies to the trends demonstrated by Dr. Rogers in his Appendix D, as they are dependent on the missing information on surgeon type. Moreover, these trends are attenuated (or reversed) by using proportions instead of absolute numbers, a more appropriate approach to analyzing trends. Either way, our multivariable results are irrespective of any of these trends, since year of procedure was adjusted for in our models. Thus, cost differences remain, and important follow-up studies should indeed focus on cost-effectiveness as mentioned by Dr. Rogers.

Fifth, while we encourage academic discourse, we take offense in being characterized as “*manipulating*” data. Dr. Rogers correctly points out that the propensity score matched cohorts are reduced in size. This is a function of the propensity score matching algorithm. The fact that the podiatrist cohort is proportionally reduced less than the orthopaedic foot/ankle surgeon cohort is based on the potential distribution of propensity scores, which would theoretically be higher in the orthopaedic foot/ankle cohort. In other words, there will be more variation in propensity scores in a larger cohort, compared to a smaller one. Therefore, when matching the two cohorts, it is expected that far fewer cases in the larger cohort will be matched. To reiterate, this is by no means a deliberate action to “*manipulate*” data, but rather inherent in propensity score matching.<sup>3</sup> Exclusion of outpatient procedures will only affect our results if outpatient procedures are performed differentially (in terms of numbers) by surgeon type. Importantly, the

fact that outpatient procedures are inherently lower cost (compared to inpatient procedures) does not indicate any direction of effect on relative cost estimates as presented in our paper.

Finally, indeed, Dr. Vulcano, the senior author, is an orthopaedic foot and ankle surgeon and a member of the American Orthopaedic Foot & Ankle Society (AOFAS), as are almost all orthopaedic foot and ankle surgeons practicing the US. This information was not intended to be hidden and was not mentioned in the International Council of Medical Journal Editors (ICMJE) conflict of interest forms, as Dr. Vulcano is merely affiliated with (as would be reasonably expected given his subspecialization) but not actively involved in AOFAS decision-making and does not hold any board position within the organization. This is in contrast to the examples mentioned by Dr. Rogers, where co-authors did hold administrative positions within professional organizations.<sup>4,5</sup> However, Dr. Vulcano has no reservations in disclosing this information on his (professionally expected) affiliation with AOFAS, and especially given Dr. Rogers' reservations, we fully support an addendum to the current publication if deemed necessary by the Editor. (Please see Editor's Note on p. 27.)

We do regret some of Dr. Rogers' language regarding implications on potential retraction, as we feel that this in turn may represent a more explicit conflict of interest given Dr. Rogers' administrative position. We question whether a response letter would have been sent to *The Journal* with an implied request for retraction of the manuscript if the results of our paper had been neutral or in favor of podiatrists.

In conclusion, regarding Dr. Rogers' comments, we do regret some of the unfortunate misinterpretations and hope this will lead to further constructive discourse. As we state in our study, this will not be the last word on potential outcome differences after ankle surgery by surgeon type and "*validation in future studies using alternative data sources*" is needed to come to more conclusive results.

\* \* \*

Regarding the thoughtful and constructive responses by Malay et al., we wholeheartedly agree that a thorough cost-effectiveness analysis is needed for more conclusive results; however, this was beyond the scope of our study and not possible given the lack of granular patient-centered outcomes in our data source. While the Premier Healthcare dataset is easy to purchase by other study groups, we do agree that the acquisition cost may impair transparency where data, ideally, should be public and freely accessible to all. However, we respectfully disagree that our study was conducted "*as a means to an end and not necessarily with scientific skepticism.*" Indeed, we have added various sensitivity analyses and made some substantial changes to analyses (including a substantial reduction of our study cohort by only including 2011-2016 data instead of 2006-2016) during a lengthy and thorough peer-review process.

In response to each of the numbered comments by Malay et al.:

1. We appreciate the provision of more recent documentation on foot and ankle surgical training. We had mentioned the study from 1997<sup>6</sup> in the context of emphasizing the current paucity in the literature on "*studies investigating the association between type of provider and costs and outcomes in foot and ankle surgery.*"

2. We respectfully disagree with the characterization of the substantial relative cost difference (+28.5% in ankle arthrodesis) between groups as “*small differences*.” This may have applied to univariable differences in readmission risk, where we applied standardized differences that may guard against “*statistical artifacts*.” However, this difference was not included in our main results given the low prevalence of this outcome.
3. We agree that differences in length of stay may be multifactorial.
4. We expect cost of implants to play a bigger role in total ankle arthroplasty than in ankle arthrodesis. The fact that the main relative cost differences were seen in the arthrodesis cohort would argue against the mechanism mentioned by Malay et al.
5. Indeed, Austin<sup>7</sup> states that “*increasing the number of untreated subjects matched to each treated subject increased the bias of the estimated treatment effect*,” but he also goes on to conclude that “*conversely, it tended to result in increased precision*.” Our propensity score matching algorithm matched “*up to*” three controls (surgery by orthopaedic foot/ankle surgeon) to each case (surgery by podiatrist). As can be seen by the sizes of the matched cohorts, this led to the majority of cases being matched to 2 controls and not 3, thus minimizing the effect of this proposed bias. An additional analysis using a 1:1 matching approach demonstrates effects similar to our main analysis (**Table 1**).  
  
Moreover, we do not believe that our propensity score analysis is invalid, as there appears to be balance between most measured covariates in the matched dataset when evaluating the standardized differences.
6. We adjusted for measured confounders in our instrumental variable analysis, in line with recommendations and previous work published by our group.<sup>8-10</sup>

7. Specifically for the outcomes we emphasized in our conclusions—length of stay in total ankle arthroplasty and cost and length of stay in ankle arthrodesis—we found that while effect estimates did differ somewhat between statistical approaches, all confidence intervals overlapped and directions of effect did not change. Moreover, given that p-values for the effects mentioned above were almost all  $<0.001$ , any adjustment for multiplicity would not have changed our main results.
8. **Table 1** provides results for the proposed analysis where unknown surgeon type is designated as either podiatrist or orthopaedic foot/ankle surgeon; this did not change our main results.
9. We agree that ideally data should be easily (and freely) accessible to other researchers to evaluate analyses such as the multiple imputation approach. However, we transparently reported on the number of missing surgeon type cases and used standard SAS code when applying multiple imputation.
10. Coding errors and exclusion of outpatient procedures would only potentially affect our results (in either direction) if they are not distributed evenly by surgeon type. We do not expect this to be the case.

In conclusion, we believe that Malay et al. have put forward valid concerns, most of which we were able to address. Our main results did not change in various sensitivity analyses and additional suggested analyses in this response, demonstrating their robustness. We therefore respectfully disagree that our results “*were likely due to unmeasured systematic variables that were unrelated to patient-level outcomes and that biased the findings against podiatric surgeons.*” Since our paper definitely is not the last

word on potential outcome differences by surgeon type, we wholeheartedly agree with Malay et al. on their recommendations for more collaborative approaches in future research, as the “*common aim is to improve outcomes for our mutual patients.*”

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**Table 1.** Results from additional multivariable models in response to suggestions by Rogers and Malay et al.

<b>ANKLE ARTHROPLASTY</b> <i>Effect estimates for 'Podiatrist' with Orthopaedic foot/ankle surgeon as the reference</i>			
	<b>Main Analysis; unknown surgeon type categorized as podiatrist</b>	<b>Main Analysis; unknown surgeon type categorized as orthopaedic foot/ankle surgeon</b>	<b>Propensity Score 1:1 Matching</b>
<i>Cost of Hospitalization**</i>	3.3% (-2.5; 9.4%)	1.6% (-4.9; 8.6%)	1.0% (-3.7; 5.9%)
<i>Length of Stay**</i>	16.1% (8.3; 24.5%)*	13.9% (5.2; 23.4%)*	15.3% (8.7; 22.3%)*
<b>ANKLE ARTHRODESIS</b>			
<i>Cost of Hospitalization**</i>	23.7% (18.2; 29.4%)*	25.1% (19.1; 31.5%)*	33.1% (26.1; 40.6%)*
<i>Length of Stay**</i>	12.0% (6.5; 17.8%)*	13.0% (6.9; 19.4%)*	21.3% (14.4; 28.6%)*

\*P < 0.05

\*\*Effect estimates (exponentiated coefficients) from the log model depicting percent change (for podiatrist) compared with reference (orthopaedic foot and ankle surgeon). Models adjusted for age, sex, race/ethnicity, insurance type, hospital location, hospital size (number of beds), hospital teaching status, hospital annual volume of ankle arthroplasty and ankle arthrodesis, year of procedure, peripheral nerve block use, diagnosis of osteoarthritis, Charlson-Deyo Comorbidity Index, smoking, and obesity.

### Editor's Note

Having re-read the Chan et al. article, the eLetters from Dr. Rogers and Dr. Malay, and the response from the original-article authors, I am gratified that this study has engendered vibrant discussions from members of both the podiatric and orthopaedic surgeon communities. I am satisfied that Dr. Vulcano's comments about his membership in the American Orthopaedic Foot & Ankle Society sufficiently address Dr. Rogers' disclosure concerns.

Furthermore, I would like to assure all readers that *The Journal of Bone & Joint Surgery* publishes painstakingly peer-reviewed and copyedited scientific findings that are never intended to masquerade as either "lobbying efforts" or "marketing campaigns." The findings of the Chan et al. study are sound and speak for themselves. The interpretations of the findings by Drs. Rogers and Malay and the response to those interpretations by Chan et al. also speak for themselves.

I again thank all the contributors to this discussion. I think this data should be shared and discussed among both communities, with the goal of raising every surgeon's level of care for the benefit of all patients.

Marc Swiontkowski, MD  
*JBJS* Editor-in-Chief