

An Initial Critical Response to Swartout et al.'s (2015) paper in *JAMA Pediatrics*,
“A Trajectory Analysis of the Campus Serial Rapist Assumption”

By Jim Hopper, David Lisak, & Allison Tracy

April 27, 2016

REVISED 1/28/16 to clarify definitions of “serial rapist” vs. “repeat rapist”
and streamline charts on simple frequency analyses;

REVISED 4/27/16 to fix y-axis labels on two slides of Dr. Hopper’s presentation

This document is available as part of a [post on PubPeer.com](#), where it may be
accompanied by comments from the *JAMA Pediatrics* paper’s authors,
other researchers and other interested parties.

Introduction

Jim Hopper & David Lisak

On July 13, 2015 *JAMA Pediatrics* published, "[A Trajectory Analysis of the Campus Serial Rapist Assumption](#)," an article by Drs. Kevin Swartout, Mary Koss and four of their colleagues. It was accompanied by an [uncritically praising editorial commentary](#).

In the paper's title and introduction, Swartout and colleagues refer to "an assumption" that is actually two propositions: (1) Most men who rape are perpetrators of multiple rapes, i.e. repeat or serial rapists; (2) the vast majority of rapes are committed by repeat or serial rapists. Swartout and colleagues then state, "Although the serial rapist assumption is widely taken as fact by politicians and the popular press, it appears to be premised on a single source," and cite a 2002 paper co-authored by one of us, Dr. David Lisak, "[Repeat Rape and Multiple Offending Among Undetected Rapists](#)." In fact, as cited by Lisak and Miller (2002), decades of research document that repeat or serial offending is the norm for all kinds of crimes, not only rape.

In the rest of the paper, Swartout and colleagues report on research that they claim refutes the "serial rapist assumption" and thus, by implication, refutes or at least supercedes Dr. Lisak's research and what he has been teaching policy makers and others whose work addresses sexual assault. For example, in the "Conclusions and Relevance" section of the abstract, Swartout and colleagues write:

Although a small group of men perpetrated rape across multiple college years, they constituted a significant minority of those who committed college rape and did not compose the group at highest risk of perpetrating rape when entering college.

As described below, we hired a statistical and methodological consultant to evaluate independently Swartout and colleagues' data and analyses, and we have received peer review of our consultant's critique of those data and analyses.

We have shared our concerns about the scientific validity of this paper with its authors and the editors of *JAMA Pediatrics*.

Reasons for Public Critique

We are sharing this critique publicly for two reasons: First, the paper's publication has been followed by extensive dissemination of its findings and their alleged implications. This has occurred via the media and professional outlets, including an August 2015 "[research brief](#)" published by the National Sexual Violence Resource Center and a September 15, 2015 [web conference](#). Dr. Swartout has said that other dissemination efforts, including with the Centers for Disease Control, are in the works.

Second, we seek to promote an informed and open discussion among researchers, policy makers and those working to prevent, investigate, prosecute and adjudicate crimes of sexual assault, as well as interested members of the media and the public.

Initial Concerns: Lack of Transparency

Within a week of the article’s publication, one of us, Dr. Jim Hopper, opened communication with Dr. Swartout, which included a brief phone call and several email exchanges. Dr. Swartout quickly provided a bare-minimum dataset, consisting only of 5 “R” (rape) variables, each of which had values of 1 (yes), 0 (no), or 9999999 (missing) for whether each man had reported committing completed rape on surveys covering 5 assessment periods (pre-college and at the ends of the spring semesters of freshman, sophomore, junior and senior years). He also provided the statistical code he had used to conduct his “latent class growth analysis” using those simple dichotomous variables for the *JAMA Pediatrics* paper.

Then, despite immediate and repeated requests over more than 2 months, Dr. Swartout never shared a version of the publicly available “derivation dataset” (used to derive the latent class model) that included both the R variables created for the paper’s analyses and subject ID numbers. Indeed, despite repeated requests Dr. Swartout never provided any version of that dataset that allows matching, for each subject, the R variables in the bare-minimum dataset with other key rape variables in the public version of the complete “derivation dataset.” Critically, the complete public dataset has variables indicating how many times each man reported raping (and attempting to rape) during each period assessed, which is essential information when it comes to evaluating claims about repeat or “serial” rape by those men.

Dr. Martie Thompson, who is a co-author of the paper and controls the other, “validation dataset” used in its analyses, which is not publicly available, declined (through Dr. Swartout and by not answering a directly emailed request) to provide any other variables (than the simple R variables) from her dataset.

Independent Peer Review

Given our concerns about the paper and its scientific foundation, as well as the campaign to promote claims about its alleged implications, we hired Dr. Allison Tracy, a methodologist and senior researcher who regularly employs the statistical method used by Dr. Swartout, to evaluate independently the science reported in the paper.

Dr. Tracy (like Dr. Hopper) immediately encountered several significant issues with data integrity and validity, both in the bare-minimum dataset provided by Dr. Swartout (i.e., in its derivation dataset portion) and in the publicly available version of the derivation sample data. Attempting to address those problems and understand the data has been very time consuming, and accounts for the fact that it took more than 10 weeks to create a document like this one, on problems with the paper’s data and analyses.

With time continuing to pass and Dr. Swartout and colleagues pressing forward with their plans to “educate” policy makers and others about their paper and its alleged implications, we felt a responsibility to inform David Lee, PreventConnect and CALCASA – and attendees of the September 15, 2015 webinar they hosted – about our discoveries of significant problems with the statistical analyses and the data on which the paper was based. In the final days before the webinar we rushed to create the first version of the document provided here, that is, a relatively high-level and summary critique of the paper, the science behind it, and the claims its authors and others have made about its alleged implications.

Since then we have expanded our analyses and created a more extensive high-level summary and critique document (this one). In addition, Dr. Tracy has written a comprehensive (75-page) technical report that documents all of the analyses she conducted and all of her findings.

Furthermore, Dr. Tracy’s methodological critique and technical report documents have been subjected to peer review (see Appendix to “Methodological Critique: Executive Summary”).

We invite other researchers to evaluate – independently – the data and analyses used by Swartout and colleagues for their *JAMA Pediatrics* paper, as well as the analyses conducted by Drs. Hopper and Tracy. We are happy to share any of the data, SPSS syntax, Mplus syntax, etc. in our possession (with the exception of the full public derivation sample dataset, which can be [downloaded directly](#) by [eligible](#) parties).

As documented here, sufficient information is already available to discover and demonstrate major problems with the data and analyses reported in the *JAMA Pediatrics* paper. However, any analyses of Swartout and colleagues' data and analyses will remain incomplete, including with respect to data integrity and validity, until and unless Dr. Thompson shares enough of her "validation dataset" to enable independent researchers to understand (a) how the simple yes/no rape variables were derived, and (b) what the data tell us about the numbers of rapes (and attempted rapes) that the men in her study reported committing at every timepoint.

Remaining Two Sections of this Document

1. Dr. Hopper's presentation, "What Does the 'Derivation Dataset' Used in Swartout et al. 2015 Tell Us About Repeat Rape? Simple Frequency Analyses." This presentation provides a clear window into the massive contradiction between what Swartout and colleagues claim about their data and analyses, on the one hand, and what their data actually reveal, on the other. For their publicly available derivation dataset, it is very clear that a majority of men who commit rape (and attempted rape) do so repeatedly, and that the vast majority of rapes (and attempted rapes) are committed by repeat rapists (or serial rapists as typically defined, i.e., repeat rapists).
2. Dr. Tracy's "Methodological Critique: Executive Summary." This document clearly lays out major problems with Swartout and colleagues' latent class analyses, and with the validity and integrity of their "derivation dataset" (as noted above, the "validation dataset" has not been shared beyond the simple dichotomous yes/no rape variables).

Additional Preview of Our Findings

Swartout and colleagues' claim that only a small proportion of rapists in their samples are repeat or serial offenders rests on the following definitions and assumptions, which we submit are either shaky or incorrect:

1. They define "serial rape" in an arbitrarily restrictive way: men who report committing rapes across more than one assessment period. Thus a man who reports that he committed multiple rapes, for example more than 5 rapes between September and May of freshman year of college, is, according to them, not a "serial rapist". (On whether he is a "repeat" rapist, they are silent.) The same is true if a man reported committing more than 5 rapes freshman year and then dropped out of the study.
2. They arbitrarily exclude attempted rapes entirely from their analyses. Attempted rape is a serious violent behavior, which can have significant traumatic impacts on its victims, and clearly very related to "completed" rape. Indeed, in many if not most cases it's just a matter of luck that the man is unable to "succeed" at committing penetration. There is no justification for excluding attempted rape.
3. They assume that any subject who reports committing multiple forms of rape during a single assessment period (e.g., both rape by force and rape by intoxication) was actually referring to the

same incident(s). For example, if a man reported that he committed 3 acts of rape involving penetration of a woman's vagina with his penis, as well as three other acts of rape in which he gave a woman alcohol or drugs and then had intercourse with her even though she didn't want to, they treat this man as having committed only three rapes, because they assume that both types of rape were committed in each of the three rapes reported on those separate survey items. They argue that this is necessary because the survey they used (the Sexual Experiences Survey or SES) does not preclude this possibility. While it is true that the SES does not preclude this possibility, they neglect to acknowledge that their assumption – that all forms of reported rape refer to the same incident(s) – is just as “radical” as its opposite, that all forms of rape refer to separate incidents. They have not improved the validity of the SES questions or their analyses, but merely replaced one extreme assumption with its opposite, and thereby minimized the number of rapes committed by men in their studies. A more rational solution (which Dr. Hopper has used) is to run both analyses – assume they are all one incident, assume they are all separate incidents – and let readers see the results and judge for themselves.

After selecting and processing their data based on the biased definitions and assumptions above, then the authors ran latent trajectory analyses that, as Dr. Tracy shows, are rife with problems that undermine the authors' analyses and conclusions.

In addition, the authors fail to note another possible, even likely source of artifact that could dramatically affect the validity of their data and trajectory analyses. The SES and all such surveys function on implicit deception. That is, researchers ask people questions about behaviors that meet the legal definition of rape, and hope that they will not recognize what is being asked. In longitudinal studies such as those used for the authors' analyses, this implicit deception is stretched to its limits. Do some subjects – at the second, third or fourth assessment – “catch on” to the meaning of the questions and become more guarded in their responses, failing to disclose crimes that they have actually committed? This is a distinct possibility.

Final Introductory Comments

We believe it is important to articulate some key points and opinions of ours that speak to the larger contexts of sexual assault research and its implications for policy and practice.

First, any conflicts between law enforcement and prevention approaches to sexual assault are not inherent, necessary, or helpful. Indeed, just as successfully investigating, prosecuting and incarcerating a repeat rapist prevents him from raping more people in the community, primary prevention efforts (e.g., bystander training) increase the odds that a rapist's peers will stop him from committing rapes, will report him if they discover he has raped, and/or will testify truthfully about any sexual assaults that they have (partially) witnessed him commit. And prevention programs are essential for reducing the likelihood that men will rape even one time – however much alcohol has been consumed, whatever “opportunities” may arise.

Second, the strong evidence that most rapists are repeat rapists, and the overwhelming evidence that a large majority of rapes are committed by repeat rapists (which is mostly a function of simple math), do not in any way contradict the following realities: (a) like each victim of sexual assault, each person who rapes or who is accused of committing rape is a unique human being with inherent dignity, human rights and legal rights, including the right to due process, that must be promoted and protected; (b) rape-promoting beliefs and attitudes, and the behaviors of those who rape (whether once, twice, or many times), are shaped by cultural and social factors that can be altered (at least partly) by primary prevention efforts; (c) not only

public-safety promotion and punitive justice approaches (both of which include the options of incarceration and expulsion from college), but [restorative justice](#) approaches as well, have valuable roles to play in how communities and institutions deal with those who commit rape and other sexual assaults.

[Jim Hopper, Ph.D.](#) is a clinical psychologist, independent consultant and part-time Instructor in Psychology at Harvard Medical School. For over 20 years his research, clinical and consulting work has focused on the psychological and biological effects of child abuse, sexual assault and other traumatic experiences. He has conducted research on various aspects of child abuse and sexual assault, including male perpetrators of sexual and physical violence, the neurobiology of trauma, unique effects of sexual abuse on males, and the nature of traumatic memories. In Dr. Hopper's forensic work he testifies on short- and long-term impacts of child abuse and sexual assault. He has served on the Peace Corps Sexual Assault Advisory Council and consults and teaches nationally and internationally to military and civilian investigators, prosecutors, judges, victim advocates, commanders and higher education administrators.

[David Lisak, Ph.D.](#) is a clinical psychologist who has devoted his professional life to studying the causes and consequences of interpersonal violence. His research on nonstranger rapists, and on the long term impact of childhood sexual abuse in adult men has been published in leading scientific journals. For the past 25 years he has served as a forensic consultant, professional trainer, and public speaker across the United States. He has served on the faculty of the National Judicial Education Project and the American Prosecutors Research Institute, and has served as a consultant to judicial, prosecutor and law enforcement education programs across the country. Dr. Lisak has conducted trainings and workshops in all fifty states across the U.S., and consults widely with universities, the four services of the U.S. Military, the Department of Defense, and other institutions regarding sexual assault prevention and policies. He consults frequently on sexual violence and homicide cases across the country, and serves as an expert witness in sexual assault cases on issues of victim behavior and offender characteristics.

What Does the “Derivation Dataset”
Used in Swartout et al. 2015
Tell Us About Repeat Rape?
Straightforward Frequency Analyses

Analyses and Presentation
by Jim Hopper, Ph.D.

April 27, 2016

(two corrected y-axis labels, slides 18 & 21)

Background Information – Datasets

- Swartout et al. 2015* used 2 different data sets:
 - “Derivation dataset” – J. White’s, 1990-1995
 - “Validation dataset” – M. Thompson’s, 2008-2011
- The “derivation dataset” is publicly available
- We have been unable to obtain sufficient data from the “validation dataset” to run simple frequency analyses (we only have 5 yes/no rape variables, one for each assessment)

* Swartout, Koss, White, Thompson, Abbey & Bellis, Trajectory analysis of the campus serial perpetrator assumption. JAMA Pediatrics, 5(3), 314-324).

Swartout et al.'s Definition of Rape

They used 3 items from Koss's Sexual Experiences Survey to define rape as completed rape. For derivation dataset:

1. **Penile-vaginal rape:** "Have you engaged in sexual intercourse with a woman when she didn't want to by threatening or using some degree of physical force (twisting her arm, holding her down, etc.)?"
2. **Drug/alcohol-related rape:** "Have you ever deliberately given a woman alcohol or drugs and then engaged in sexual intercourse when she didn't want to?" [Note: unclear if incapacitated.]
3. **Other rape:** "Have you engaged in sex acts (oral or anal intercourse or penetration by objects other than the penis) with a woman when she didn't want by threatening or using some degree of physical force (twisting her arm, holding her down, etc.)?"

High Attrition Rate

Total numbers of derivation dataset participants providing data on sexual assault survey items:

- **~850:** Fall freshman year (Pre-College, ages 14-18)
- **~640:** Spring freshman year (Sept - May, 8 months)
- **~455:** Spring sophomore year (12 months)
- **~300:** Spring junior year
- **~145:** Spring senior year (too few for valid analyses)

Swartout et al.'s Definition of “Serial” Rape

- Some researchers (e.g., David Lisak) have used the term “repeat rapist” and (the more inflammatory) term “serial rapist” interchangeably.
- For this paper, Swartout et al. created a new and restricted definition of “serial rapist”: a man who admits to committing rape during more than one assessment period / year of college.
- They did not look at the number of rapes men reported committing in each assessment period / year of college.
- In addition, they did not include data from 2 items on attempted rape (i.e., penile-vaginal, drug/alcohol-related).

Swartout et al.'s Definition of “Serial” Rape

- Because Swartout et al.'s definition ignores the number of rapes men reported committing in each assessment period / year of college, **a man could commit more than 5 rapes during a particular assessment period / year of college, but if he did not admit raping during another assessment period, he was not defined as a “serial” rapist.**
- This was true if even the man dropped out of the study after reporting committing “more than 5” rapes, even on just a single survey item, in just the past 8 or 12 months.
- In short, for this paper Swartout et al. made no attempt to determine whether men were repeat rapists, let alone how many rapes they committed. Nor did they acknowledge that men they define as not “serial rapists” have committed more than 2 rapes, even more than 5 rapes – even in 8-12 months.

Potential for “Item Overlap”

- Swartout et al. caution that men completing the SES could have been referring to the same rape incident with their responses to more than one item.
- Their solution to this problem? Assume that all rape reports refer to the same incident(s).
- They argue that researchers should not add up the number of rape acts from different SES items to determine how many rapes a man committed during a particular period.
- But Swartout et al.’s solution is no more reasonable than assuming that all rape reports on different SES items refer to separate incidents.

No Overlap for Attempted Rape

- The problem of how to interpret multiple rape reports does not apply to items covering attempted rape.
- By definition, acts in which a man reports attempting but failing to commit penile-vaginal rape (or drug/alcohol-related rape) cannot overlap with acts in which he “succeeded” in completing rapes of the same type.

Dealing with Potential Overlap: Two Approaches

- **Assume 100% OVERLAP** (Swartout et al.) = All forms of rape reported by a subject in an assessment period are counted as the same incident(s) (e.g., 2 penile-vaginal + 2 drug/alcohol = 2; 3 penile-vaginal + 2 drug/alcohol = 3).
- **Assume NO OVERLAP** = All forms of rape reported by a subject in an assessment period are counted as separate incidents (e.g., 2 penile-vaginal + 2 drug/alcohol = 4; 2 penile-vaginal + 3 drug/alcohol = 5).

Our 1st Simple Question

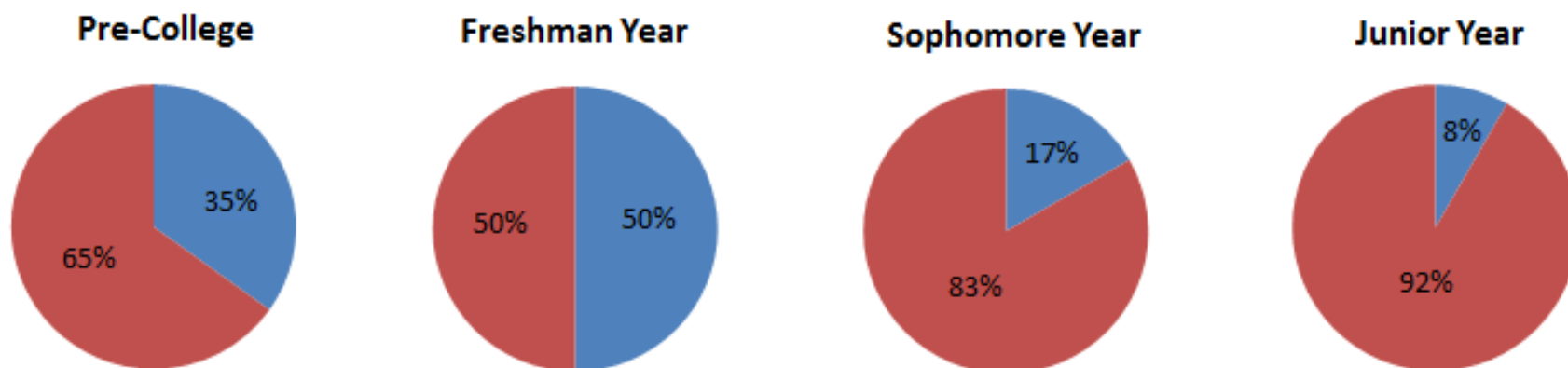
What percentage of men
who raped during a particular
assessment period committed
more than one rape during
that period?

Notes on Data and Calculations

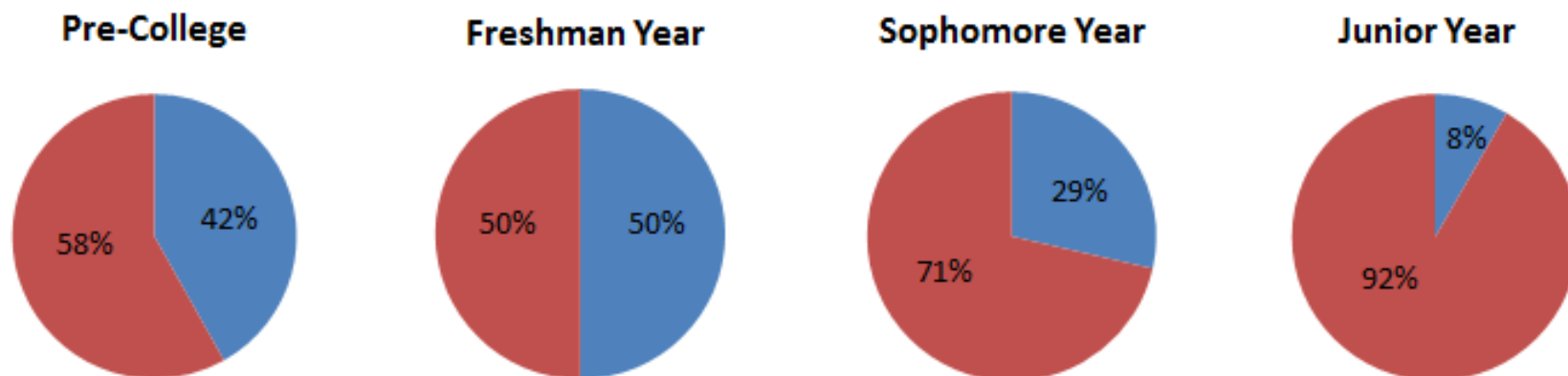
- On the following slides, we define “repeat rapist” (not “serial rapist”) as someone who rapes more than once (in this case, in a particular time period).
- All percentages are based on the data found in the public version of the “derivation dataset” they used.
- Because those data have problems with integrity and validity, the percentages on the following slides may not be accurate (i.e., are likely underestimates).
- However, the percentages are based on the same dataset that the study’s authors relied upon for their analyses, and the main errors we have found (i.e. coding “no response” and “missing” responses as “never” raped), would make these underestimates.

Percentage of Rapists Committing Completed Rapes That Are Repeat Rapists

Assuming No Overlap

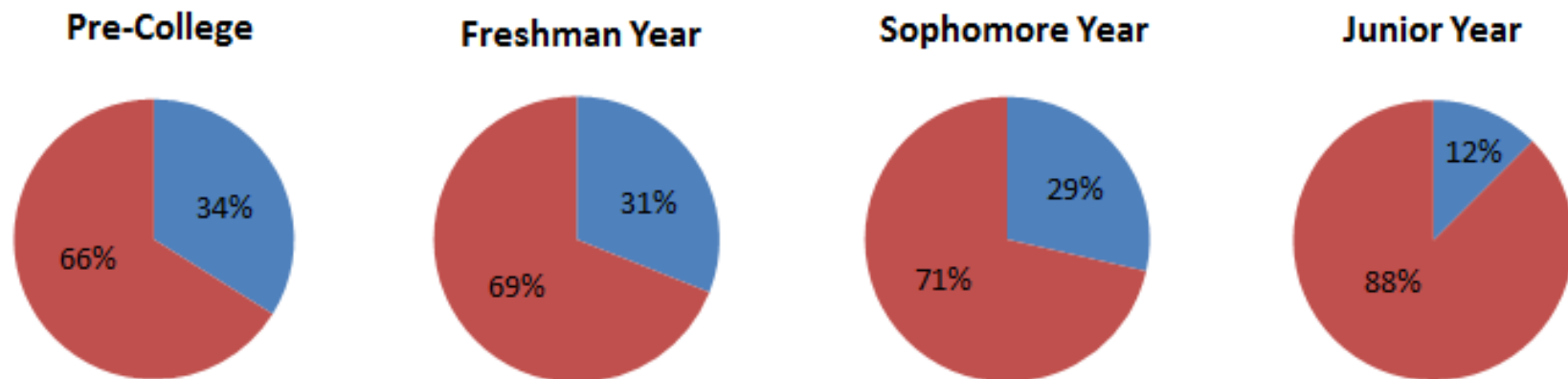


Assuming 100% Overlap

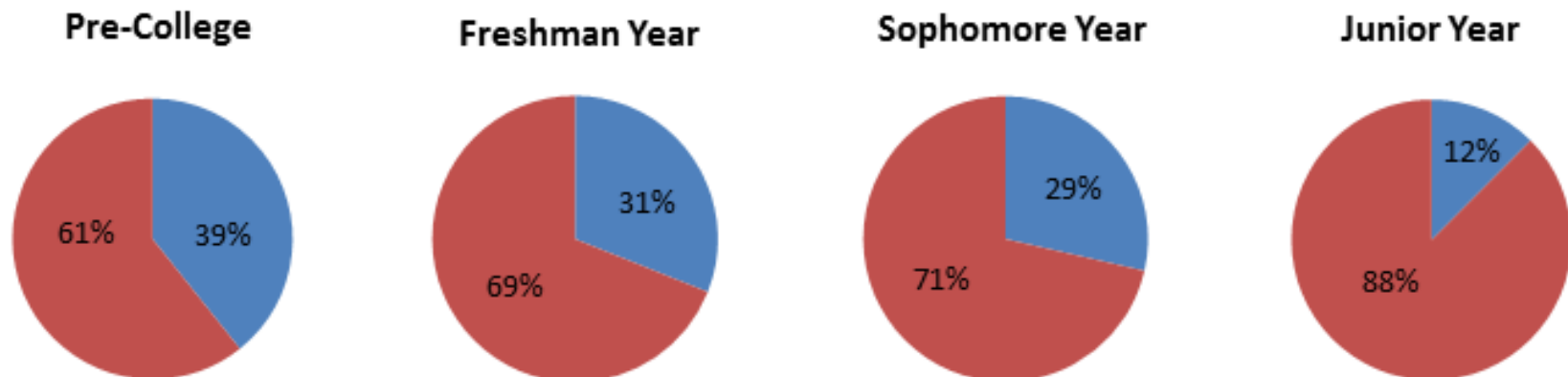


Percentage of Rapists Committing Attempted & Completed Rapes That Are Repeat Rapists

Assuming No Overlap



Assuming 100% Overlap



Our 2nd Simple Question

What percentage of rapes reported
by all men during a particular
assessment period were
perpetrated by repeat rapists?

Notes on (Conservative) Calculations

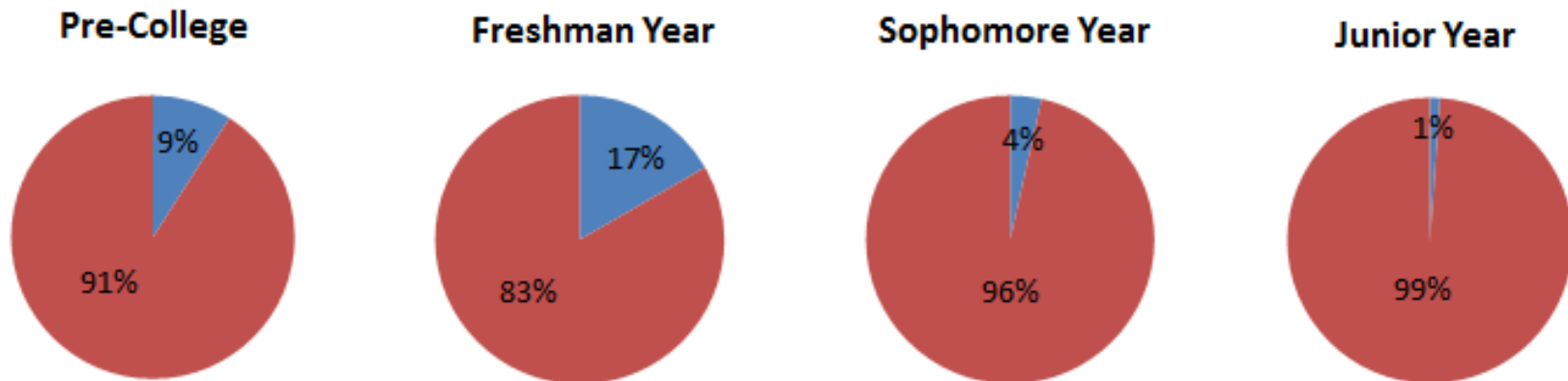
For the following slides, all calculated total numbers of rapes (committed by serial rapists), used to generate the percentages, are based on **conservative** estimates:

Response of “3 to 5” = 3

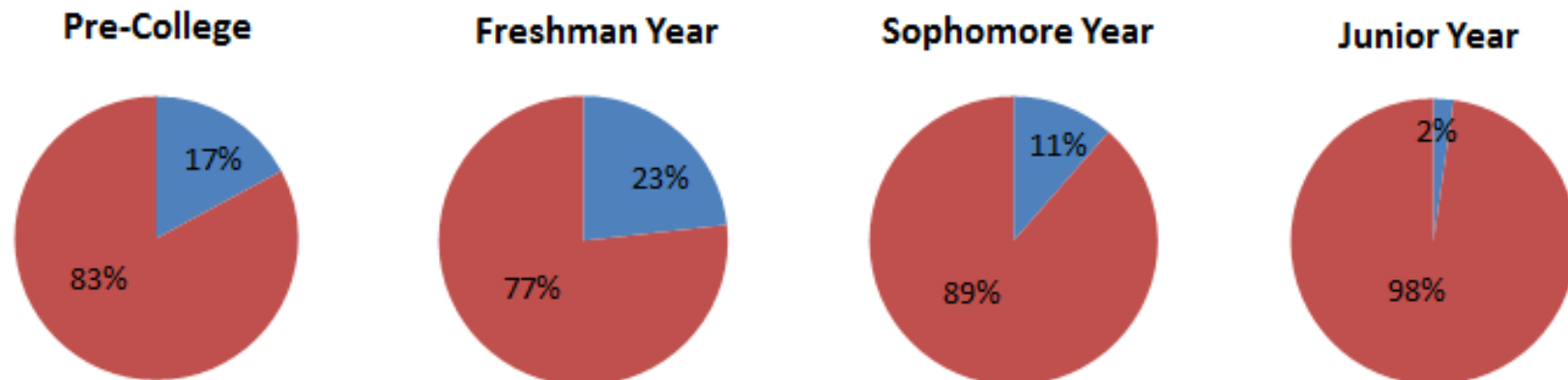
Response of “more than 5” = 6

Percentage of All Completed Rapes Committed by Repeat Rapists

Assuming No Overlap

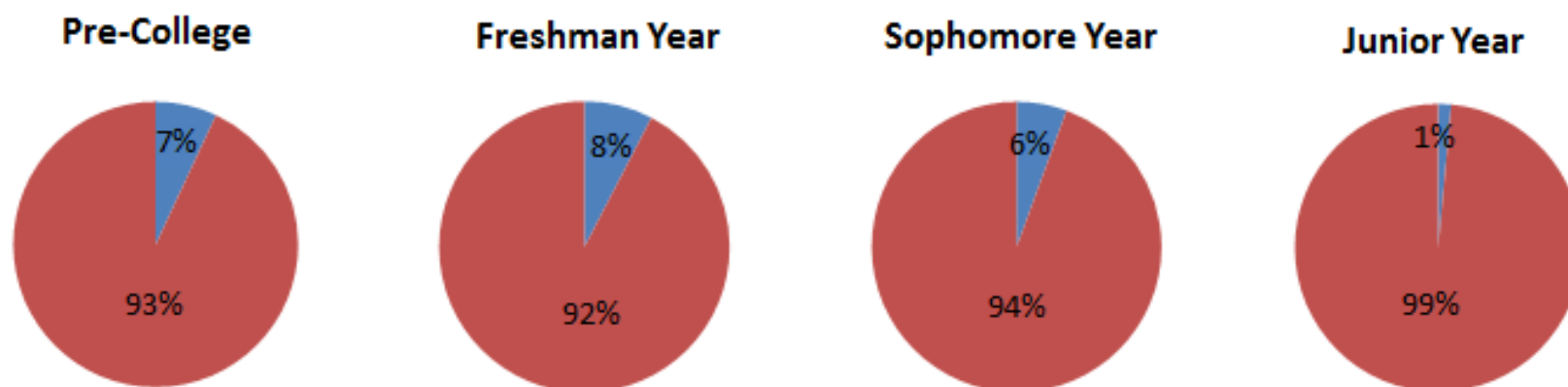


Assuming 100% Overlap

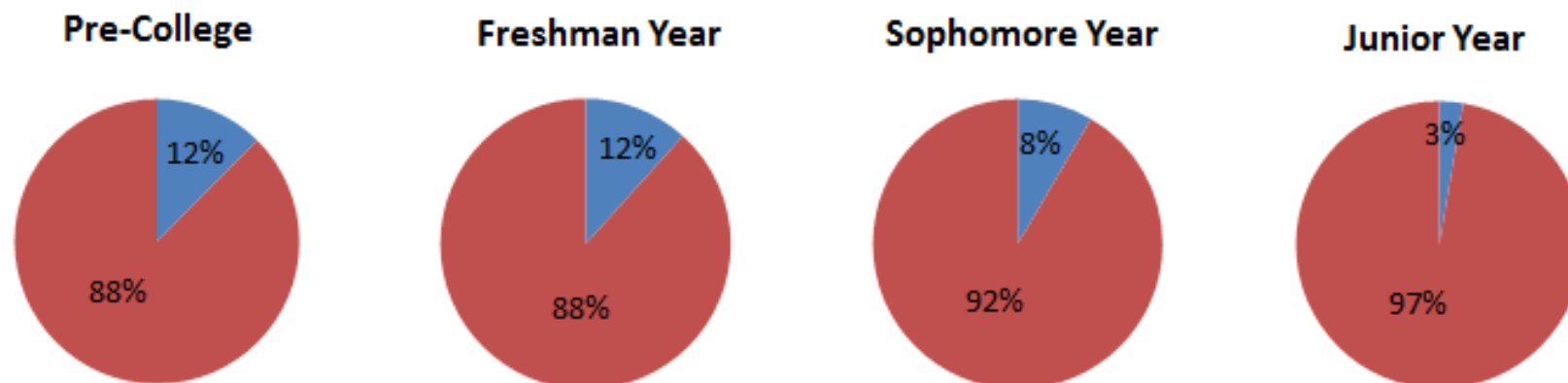


Percentage of All Attempted & Completed Rapes Committed by Repeat Rapists

Assuming No Overlap

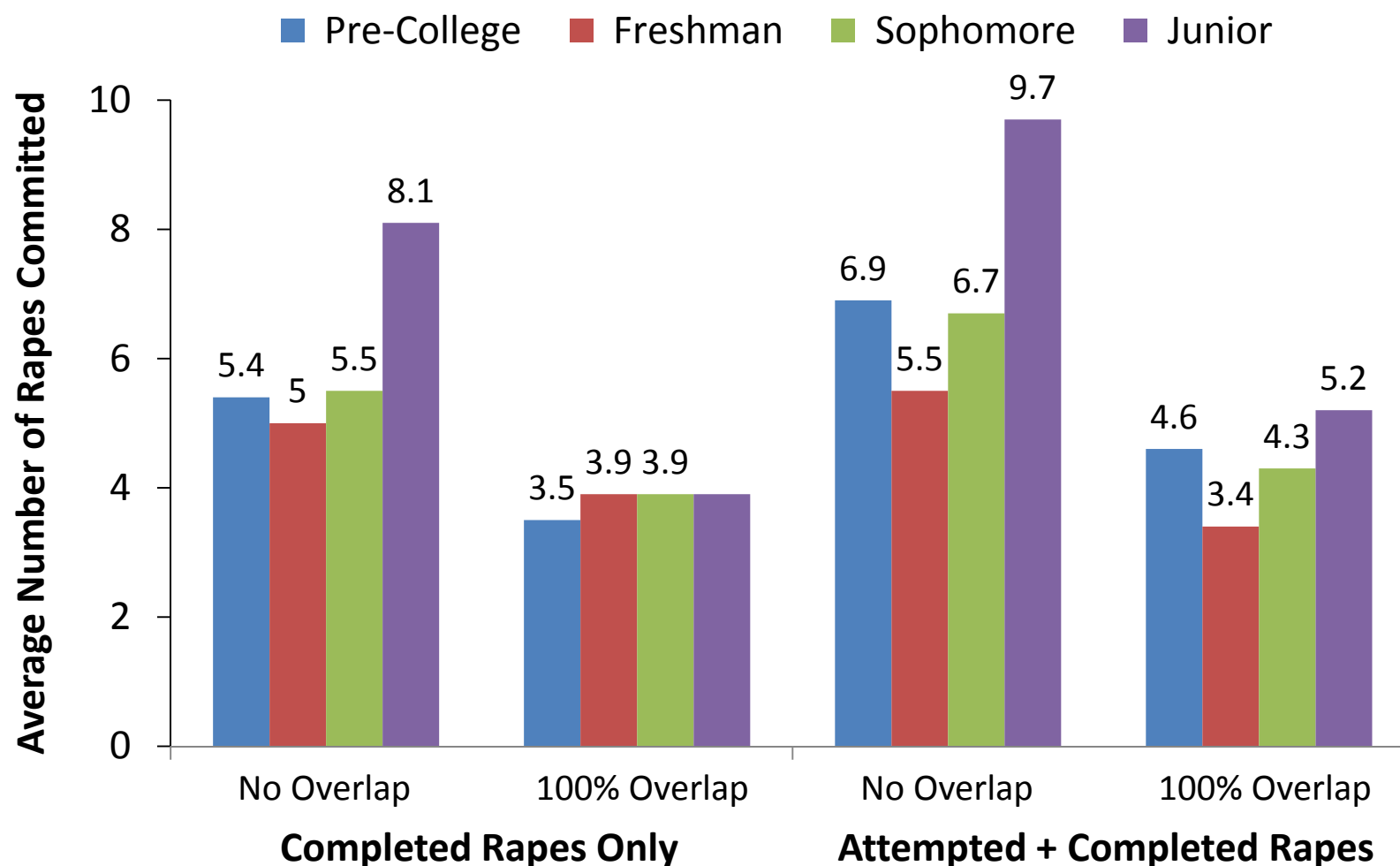


Assuming 100% Overlap



Conservatively* Estimated Average Numbers of Rapes by Repeat Rapists

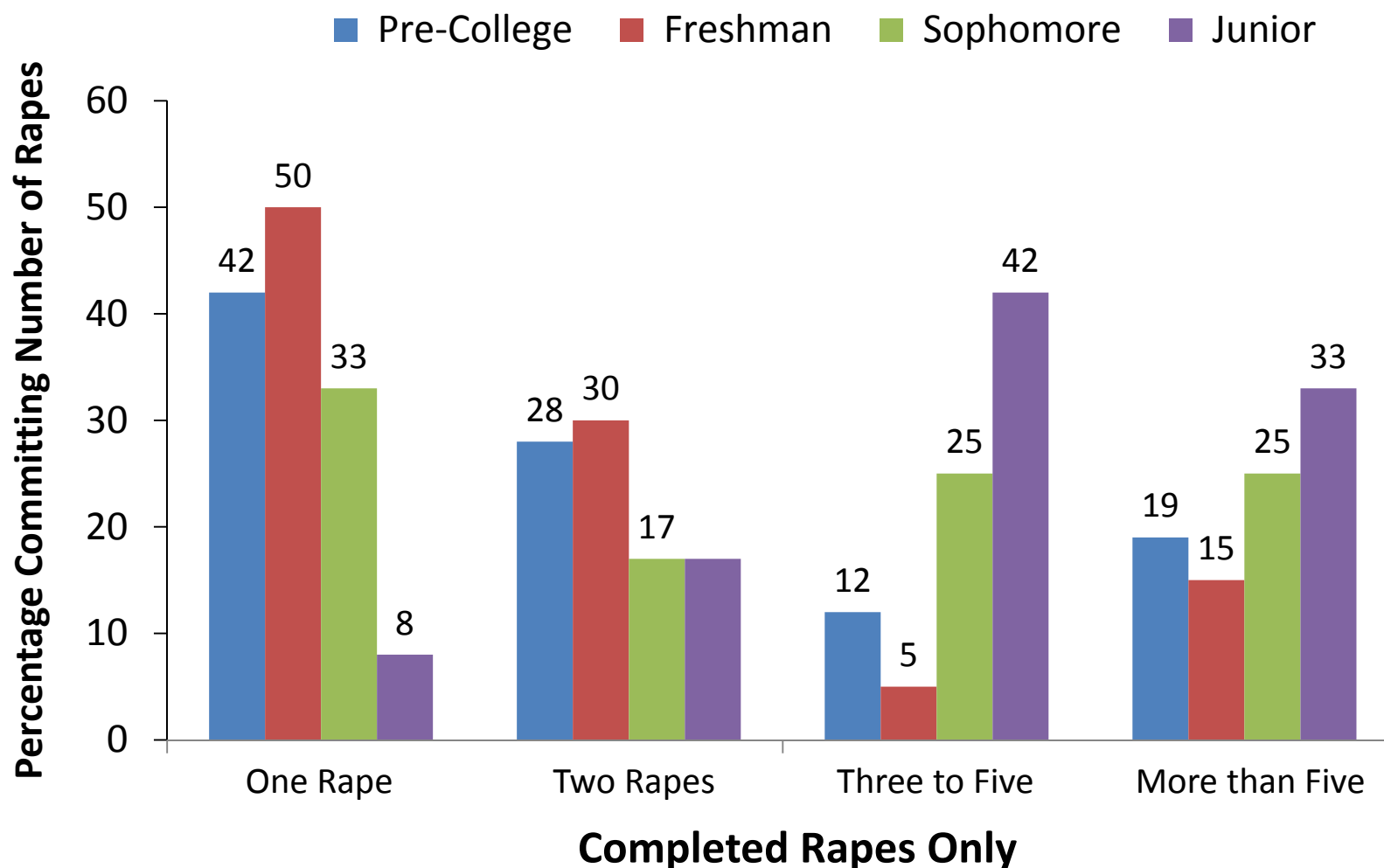
Within Each Assessment Period, Not (Even) Total Per Perpetrator



* Reports of "3 to 5" rapes were counted as 3, and reports of "more than 5" were counted as 6.

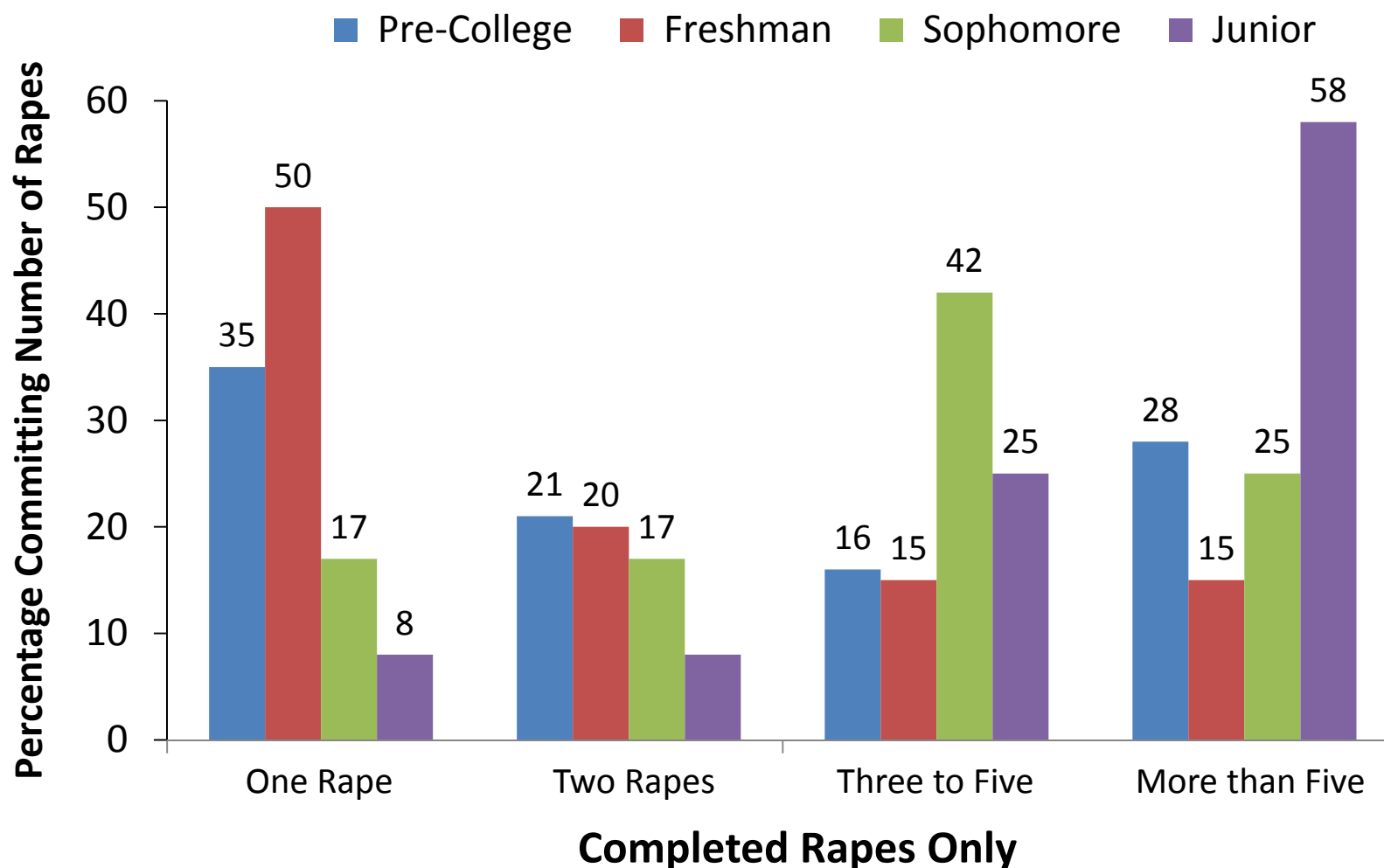
Conservatively* Estimated Percentages of Rapists Committing Different #s of Rapes

Within Each Assessment Period, Assuming Complete Overlap



Conservatively* Estimated Percentages of Rapists Committing Different #s of Rapes

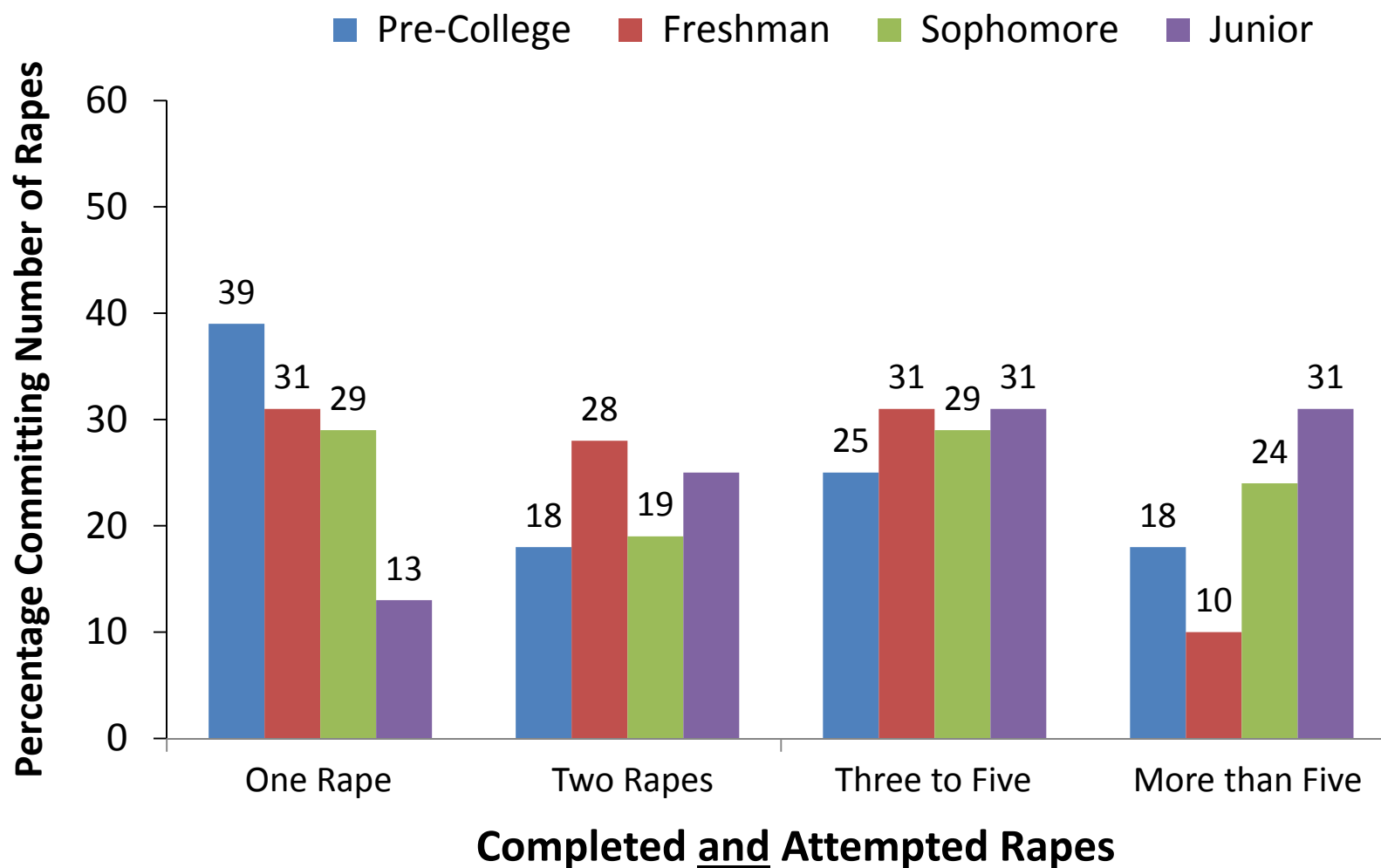
Within Each Assessment Period, Assuming NO Overlap



* Reports of "3 to 5" rapes were counted as 3, and reports of "more than 5" were counted as 6.

Conservatively* Estimated Percentages of Rapists Committing Different #s of Rapes

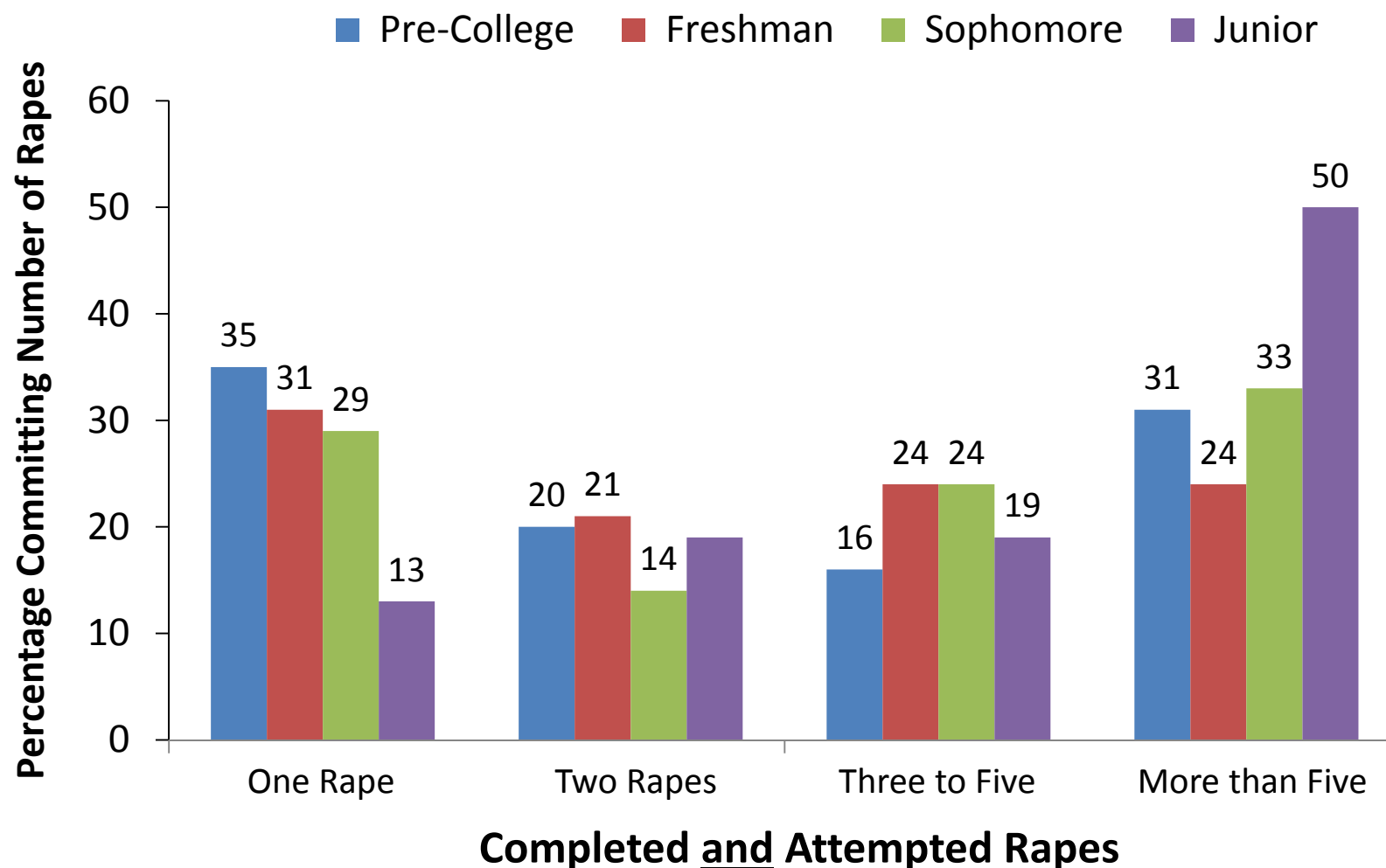
Within Each Assessment Period, Assuming Complete Overlap



* Reports of "3 to 5" rapes were counted as 3, and reports of "more than 5" were counted as 6.

Conservatively* Estimated Percentages of Rapists Committing Different #s of Rapes

Within Each Assessment Period, Assuming NO Overlap



* Reports of "3 to 5" rapes were counted as 3, and reports of "more than 5" were counted as 6.

Methodological Critique: Executive Summary

On the face of it, the July 13, 2015 *JAMA Pediatrics* article entitled, “Trajectory Analysis of the Campus Serial Rapist Assumption,” authored by Swartout, Koss, White, Thompson, Abbey, and Bellis appears to be scientifically sound. The paper reports a study conducted with sizeable samples from both a derivation and validation dataset to fit latent trajectories of the probability of raping across the college years, using perpetrators’ own reports. The article reads well and the naïve reader would likely be impressed and ready to accept the authors’ conclusions – that most campus rapists are *not* predatory serial rapists but rather opportunistic, time-limited rapists. However, a closer look reveals a number of problems that call the science and conclusions of the paper into question.

Problems with the Analyses

The dataset and the statistical programming code used to conduct the analyses provided by Dr. Swartout revealed a number of irregularities. The model for the derivation dataset yielded a warning automatically generated by the Mplus software that the model was under-identified, so that key growth parameters were fixed rather than estimated. In essence, the model was unable to define or differentiate trajectory groups.

Despite reporting that the fifth timepoint was omitted from the derivation analysis sample due to low response rates, Dr. Swartout’s programming code included the fifth timepoint. The consequence of using the “low response” fifth timepoint is that the analysis relied heavily on how the missing data were handled in the analysis. The authors claim (E3, 1st paragraph) that “...missing data were not related to reports of sexual violence across the study,” citing the Pearson chi-square test. When the final timepoint was *excluded* from the analysis model (and the proportion of missing data was reduced), this test indicated a significant departure from this assumption. In other words, the missing data handling technique used in the JAMA article may have resulted in biased estimates of the probability of rape.

Another thing that changed when 4 rather than 5 timepoints were used was the shape of the trajectory curves (Figure 1). Patterns that yield 100% and/or 0% likelihoods, as in Class 2 (panel B of Figure 1), often indicate that there are too few members in a given class, causing the parameter values for that class to be “overfitted” to these specific individuals. In both the 4 and 5 timepoint models, less than 2% of the sample was assigned to each of the two “rapist” classes (sample sizes ranged from 6 to 12). The statistical power for detecting such rare latent classes is low (70%). When sample sizes are this small, the predicted probabilities tend to be unstable, likely to change with small changes to the model. This is evident when examining the classification quality, where individuals in the two smallest classes had a significant likelihood of being misclassified (4 timepoint model: 32% & 35%; 5 timepoint model: 49% & 39%).

The authors do not demonstrate that the two populations are identical and thus their approach to replication is questionable. For example, from the information given, it is apparent that the two samples are markedly different with respect to race and Hispanic ethnicity (Derivation Sample: White=68.5%;

Black=25.7%; Other=5.8%; Validation Sample: White=89.4%; Black=7.3%; Other=3.3%). Differences in data collection technique and/or the willingness of the validation sample to participate in the study over time also call into question the validity of the replication approach.

The results of the model using the validation dataset didn't match the numbers reported in the article – except the sample sizes in the trajectory classes. All the growth parameters were estimated, not fixed, and the missing data assumption held in this dataset. However, as with the derivation model, the sample sizes for the rapist trajectory classes were very small ($n=19$ and 39), constituting 2.3% and 4.9% of the sample. As noted above, this means that the predicted probabilities tend to be unstable and are likely to change with small changes to the model. Indeed, the likelihood of being misclassified was 16% for the derivation model and 20% for the validation model. A Monte Carlo simulation study showed that the statistical power associated with the trajectory parameters were all less than 60% and the power to distinguish between the Increasing and Decreasing trajectory classes was 44%, substantially lower than the recommended 80%.

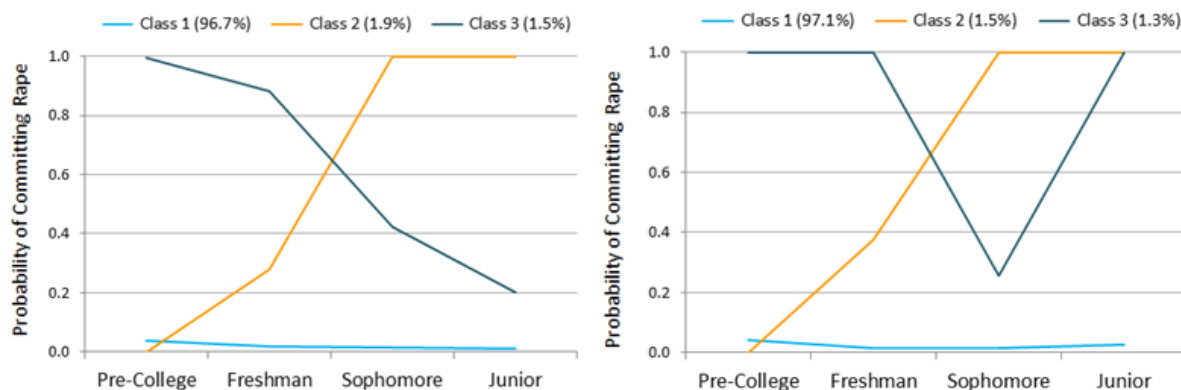


Figure 1. Different trajectory classes when 5 timepoints (left panel, to parallel the figure in the article the fifth point is not shown) versus 4 timepoints are used to model the derivation dataset. The trajectories on the left match those for the derivation dataset in the figure in Swartout et al.'s paper; those trajectories were produced only when senior year was included, but the authors explicitly said that senior year was excluded from their analyses. The trajectories on the right are what their model produces when senior year data actually are excluded; note the absence of a “decreasing” trajectory.

Swartout and Colleagues' Own Data Tell a Very Different Story

It is not unusual that a very large subpopulation in a latent trajectory analysis heavily influences the solution. To focus the analysis on college-aged rapists, the very large subgroup of non-rapists (i.e., no rape reported at any timepoint) was omitted, and the derivation and validation datasets were combined, and every timepoint was included – all of which maximized the number of rapists on which to base the trajectory analysis. Under these conditions, the same syntax provided by Dr. Swartout yielded a very different solution. In this model, the classes were more balanced (61%, 22%, 17%) and therefore more likely to describe prevalent subpopulations. In this model, only the smallest class (17%) had a pattern that described time-limited rape (Figure 2, Class 3, green line).

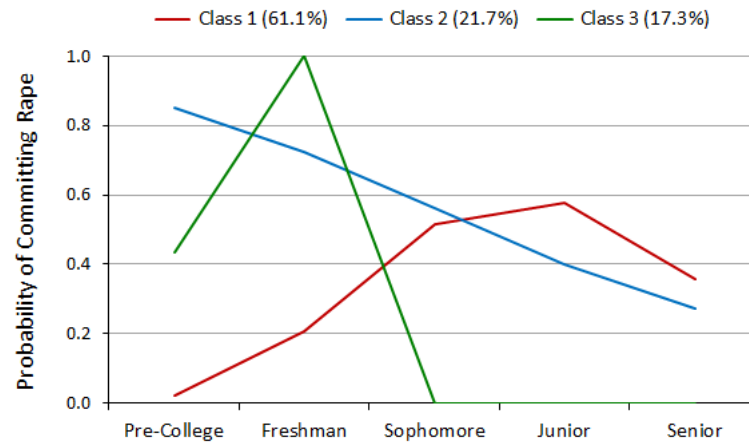


Figure 2. Trajectory classes among rapists (only) in the combined derivation-plus-validation dataset, using Swartout and colleagues' model.

By using trajectory analysis, Dr. Swartout and his colleagues imposed a restriction on the data, specifically that the probability of rape can best be described by a smooth line over time, which is a severe constraint that mismodels men who rape at non-consecutive timepoints. Further, using the pre-college timepoint, for which all participants have data, heavily influenced the growth trajectory – particularly the intercept term – on which the trajectory classes were based. Indeed, the “increasing” class, which is shown graphically in the *JAMA* article's figure, had a 0% probability of rape at pre-college, and the “decreasing” class had a 100% probability of rape at pre-college. Such heavy reliance on the pre-college timepoint, paired with the (incorrectly) assumed independence of pre-college and college rapes (E6, 1st paragraph), does not directly address rapes committed *during the college years*.

When Swartout and colleagues' untenable trajectory constraint was released, and the probabilities of rape were instead estimated at each timepoint, this resulted in a 5-class model in which 65% of the rapist sample had at least 40% probability of rape at two or more of the four timepoints (Figure 3).

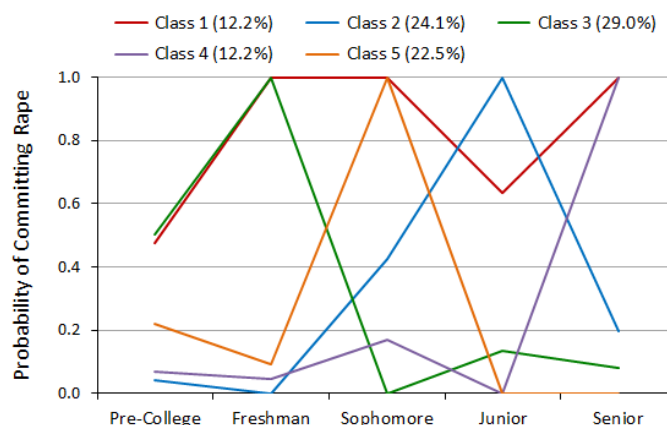


Figure 3. Five trajectory classes in the combined subsample of rapists (derivation and validation datasets), after release of the untenable smooth trajectory constraint of Swartout et al.'s model.

Importantly, these latent class models, including the *JAMA* model and the model lacking the unwarranted smooth trajectory constraint of the *JAMA* model, had estimation problems and most were under-identified.

An alternative model, logistic autoregression, tests the serial rapist assumption in a way that avoids the computationally intensive and heavily assumption-dependent latent trajectory modeling. This approach uses rape at one timepoint to predict rape at the next timepoint. *This* model converged on a solution without difficulty; it had ample statistical power, since all participants' data were used (rapist and non-rapists); and it minimized the impact of missing data, since estimates depended only on consecutive timepoints rather than the full study period. The fit of this model was excellent and each regression parameter was significant to the $p < .001$ level. Based on this model, the probability of a man raping was strongly predicted by whether he raped the previous year. In the derivation dataset, the odds of a man committing rape during Freshman year were more than 5 times higher if he had raped pre-college; the probability of raping during sophomore year was almost twice as high if a man had raped during freshman year; and the probability of raping during junior year was more than 2 times higher if a man had raped during sophomore year.

In the validation dataset, this autoregression model found that pre-college rapists are 6.7 times more likely than pre-college non-rapists to rape in the freshman year. Freshman year rapists were twice as likely as freshman non-rapists to rape in the sophomore year. Sophomore rapists were 2.6 times more likely to rape in their junior year, and junior rapists were 2.1 times more likely to rape in their senior year. In addition, these effects accumulate over time, further increasing the likelihood that man raping pre-college or as an underclassman will rape as an upperclassman (Figure 4).

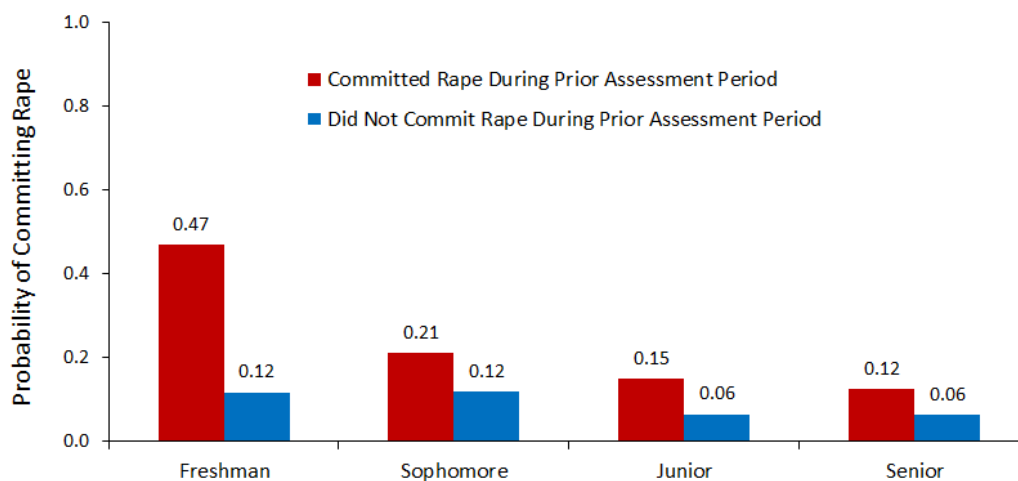


Figure 4. For the validation dataset, predicted probabilities of committing rape during one assessment period as a function of having committed rape during the prior assessment period.

Returning to the Primary Dataset: Problems with Data Integrity and Validity

Additional irregularities in the models reported in the *JAMA* article were found when analyzing the original public-use dataset from which Dr. Swartout and his colleagues created their derivation dataset; the *Longitudinal Study of Violence Against Women: Victimization and Perpetration Among College Students in a State-Supported University in the United States, 1990-1995*, [ICPSR 3212](#)). Unfortunately, the original case ID numbers were absent in the Mplus analysis dataset used for the article and, despite multiple requests, no file was provided to match the *JAMA* dataset to the public-use dataset.

Descriptively, the raw frequencies for each of three sexual assault items used by the JAMA study to operationalize rape clearly showed that the majority of college rapists rape multiple times – not only within timepoints but across them as well. (See Figure 5 for the most conservative possible estimation of the percentage of rapists who were repeat rapists, and additional pie charts that include perpetrators of attempted rapes in Dr. Hopper’s presentation of simple frequency analyses.)

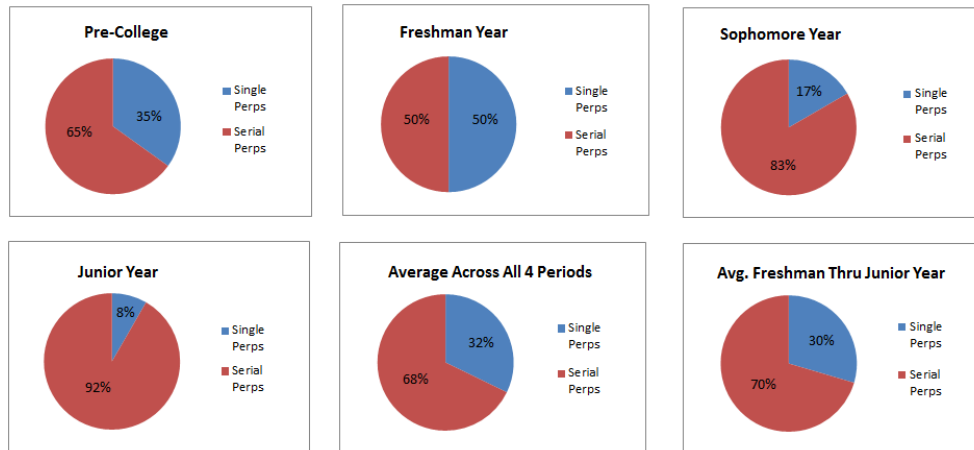


Figure 5. Percentages of rapists committing completed rapes who perpetrated more than one rape during that assessment period, assuming complete overlap (i.e., assuming that rape reported on any of the three different SES items measuring completed rape referred to the same rape(s) reported on the other two of those three SES items; see Dr. Hopper’s presentation for findings when no overlap is assumed and when attempted rapes are included).

Using the data management programming code, exactly as it was provided by Dr. Swartout, the dichotomous rape variables were reconstructed (i.e., variables indicating yes vs. no for rape at particular timepoints). But the analysis using these variables did not replicate the results presented in the article, and the frequencies of the rape variable did not match the frequencies of the variables in the JAMA analysis dataset. Swartout’s code did not draw directly from the raw frequency data that the respondents provided, but instead drew from recoded dichotomies of each of three sexual assault items operationalizing rape. A cross tabulation of the dichotomized sexual assault indicators with the raw data showed that in some cases missing data were assigned as “never” raped, which has the effect of underestimating the rapes committed.

There was a great deal of missing sexual assault data in the original dataset and the proportion of missing data in the original dataset did not match the missing data in the JAMA analysis dataset (JAMA: 25%, 47%, & 65%; Original: 25%, 60%, & 73% at Times 2, 3, & 4, respectively). In addition to study attrition, there was a substantial amount of unexplained missingness, where men participating in the study (completing at least some of the survey) did not provide data for any of the sexual assault indicators (i.e., percentages of participants within each time period with any missing data: 18%, 13%, & 29% at times 2, 3, & 4, respectively). The extent of missing data, the unexplained missing data patterns, and the missing data miscoded as never raped are serious issues that, even if the latent class analyses were valid, would undermine the validity of the conclusions reported in the JAMA article.

Choices that Underestimate Serial Rape and Serial Rapists

In addition to the miscoded missing values, several other decisions and actions by the authors resulted in underestimation of the numbers of serial rapes and serial rapists. For example, by creating a single dichotomous indicator of rape for each timepoint, the authors ignored multiple rapes within timepoint, thereby underestimating and “defining away” serial rapists (e.g., men who reported committing “more than 5” rapes freshman year, but then dropped out of the study, were not defined as serial rapists). By assigning a single rape indicator to multiple reports of rape across sexual assault items, the authors assumed that all responses to rape items refer to the same rape incident(s), that is, assumed total overlap across items, again underestimating serial rapists. Finally, the authors underestimated serial rapists by excluding attempted rapes.

Conclusion

The scientific integrity of the study described in the JAMA article is highly suspect at best. As it stands, the article is based on a study that uses erroneously coded data and underestimates both the prevalence of serial rapists in the data and the percentage of rapes those serial rapists report committing. Further, the models used in this study are based on untenable assumptions and ill-considered constraints. No reasonable and scientifically grounded debate over the “serial (campus) rapist assumption” can depend on this study.

Note: A full technical report is available [here](#), and the appendix to this document (next page) summarizes peer review of this executive summary and the technical report by Hanno Petras, PhD.

Appendix

Peer Review of Dr. Tracy's Methodological Critique and Technical Report

I have reviewed and provided feedback on Dr. Tracy's document, "Methodological Critique: Executive Summary," and her lengthy technical report. I am impressed with Dr. Tracy's work and I believe that she has well demonstrated that the study by Swartout and colleagues, "Trajectory Analysis of the Campus Serial Rapist Assumption," has numerous problems, including data integrity, data coding, model estimation, model presentation, and comparability of the two data sets.

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