

Peer-Review

Perry, Henry M. 2026. "Competitive Balance in the NIL Era: Recruiting Redistribution and Stability in the Revenue–Performance Relationship." *Journal of High School Science* 10 (3): 435–60. <https://doi.org/10.64336/001c.168462>.

While the paper identifies the 2021 introduction of NIL as a transformative event in college football and uses this claim as its primary motivation, the subsequent analysis does not directly evaluate whether NIL changed the sport. Instead, the study presents a cross-sectional relationship between estimated 2025 gameday revenue and 2025 team performance. The resulting conclusion—that programs generating greater revenue tend to achieve stronger athletic outcomes—is broadly intuitive and largely consistent with long-standing expectations regarding major college athletics. Larger programs have historically possessed advantages in facilities, recruiting, coaching resources, fan support, media exposure, and financial backing. Consequently, the central finding risks reading as a confirmation of an already widely accepted observation rather than providing new insight into how the competitive landscape has changed. The study therefore feels somewhat disconnected from the compelling question raised in the introduction: whether NIL fundamentally altered the structure of college football.

A more impactful approach would be to leverage NIL as a natural experiment. Rather than examining only a single season, the study could compare the relationship between financial resources and athletic performance before and after NIL implementation. Such a design would allow the paper to investigate whether the underlying dynamics of college football changed following the July 2021 policy shift. For example, if NIL increased competitive balance by allowing smaller programs to attract and retain talent, one might expect the concentration of success among traditional powers to decline after 2021. Conversely, if NIL primarily benefited already wealthy programs, one might observe increased concentration of recruiting success, playoff appearances, and elite performance. These questions are substantially more interesting than determining whether wealthier programs tend to perform better, because they directly address the policy change that motivates the manuscript.

One possible methodology would be to divide the data into a pre-NIL period (e.g., 2017–2020) and a post-NIL period (e.g., 2022–2025, excluding the transition year if desired). The author could then examine whether the relationship between financial resources and performance changed between eras. Several outcome measures could be investigated. First, the author could compare the slope and explanatory power (R^2) of the revenue-performance relationship before and after NIL. An increase in explanatory power would suggest that financial resources became more important determinants of success in the NIL era, whereas a decrease might indicate greater opportunity for traditionally disadvantaged programs. Second, the author could evaluate competitive balance by examining the concentration of playoff appearances, top-25 finishes, conference championships, recruiting rankings, or elite SRS performances. Third, the author could analyze roster continuity and transfer activity to determine whether player mobility increased following NIL and transfer-portal reforms. Finally, the author could investigate whether year-to-year dominance became more persistent or less persistent by measuring the probability that highly ranked programs remain highly ranked in subsequent seasons.

Such an approach would transform the paper from a descriptive correlation study into an investigation of structural change within college football. Rather than merely showing that successful programs tend to generate substantial revenue, the analysis would directly test whether NIL altered recruiting dynamics, player mobility, competitive balance, and the relationship between financial resources and on-field success. In its current form, the manuscript presents an interesting dataset and a reasonable exploratory visualization; however, a pre- versus post-NIL framework would align much more closely with the paper's stated motivation and would provide a stronger contribution to understanding how modern college football has evolved.

Dear Reviewer,

Thank you for your constructive feedback on this manuscript. I knew it was missing something, and the comments were instrumental in both significantly strengthening the paper and clarifying its focus.

"The subsequent analysis does not directly evaluate whether NIL changed the sport. Instead, the study presents a cross-sectional relationship between estimated 2025 gameday revenue and 2025 team performance."

This critique has been fully addressed, employing a two-period design, comparing the 2017 season (pre-NIL) to the 2025 season (post-NIL). The manuscript now directly investigates whether the relationship between financial resources and athletic performance changed following the July 2021 NIL policy shift. This design allows the paper to treat NIL as a natural experiment.

"The resulting conclusion — that programs generating greater revenue tend to achieve stronger athletic outcomes — is broadly intuitive and largely consistent with long-standing expectations. The central finding risks reading as a confirmation of an already widely accepted observation."

The revised paper no longer presents the revenue-performance correlation as its central finding. Instead, the central finding is now the comparison of R^2 values between the two eras: 0.5001 in 2017 versus 0.5512 in 2025. This increase demonstrates that revenue became a stronger predictor of athletic performance following NIL's introduction and directly addresses NIL's alteration of collegiate football dynamics.

"The study therefore feels somewhat disconnected from the compelling question raised in the introduction: whether NIL fundamentally altered the structure of college football."

The revised paper directly answers the motivating question. The Introduction section now sets up a before-and-after research question rather than simply asserting NIL's impact, and the Comparison section directly synthesizes the findings from both periods to conclude whether NIL changed the revenue-performance relationship.

"A more impactful approach would be to leverage NIL as a natural experiment. Rather than examining only a single season, the study could compare the relationship between financial resources and athletic performance before and after NIL implementation."

This suggestion has been implemented completely. The revised paper explicitly frames NIL as a natural experiment in the Methodology section and compares two representative seasons: 2017 (pre-NIL) and 2025 (post-NIL).

"The author could evaluate competitive balance by examining the concentration of playoff appearances, top-25 finishes, conference championships, recruiting rankings, or elite SRS performances."

Competitive balance is addressed in the Comparison section through a direct count of Group of 5 and Independent teams with negative SRS scores in each period. Complete playoff appearance and recruit ranking analysis was beyond the scope of this study.

"The author could analyze roster continuity and transfer activity to determine whether player mobility increased following NIL and transfer-portal reforms."

Roster continuity and transfer portal data were not incorporated into the revised model, as compiling program-level transfer data for all 130+ FBS schools across multiple years was beyond the scope of this study. However, the impact of increased player mobility is discussed qualitatively in the Comparison section, where it is noted that NIL and transfer portal reforms have allowed players to seek compensation at smaller programs.

I am very grateful for the reviewer's detailed and constructive guidance. After a day of strategic rewriting, data compilation, and reconstruction, I believe the paper is much clearer and more comprehensive than before.

Although the revised manuscript represents a substantial improvement and addresses the principal conceptual concern regarding the use of NIL as a natural experiment through a before-and-after comparison, an important statistical limitation remains unresolved. The manuscript's central conclusion is based on comparing the regression characteristics of the 2017 and 2025 models, including the increase in the coefficient of determination ($R^2 = 0.5001$ versus 0.5512), the change in the regression coefficient, and the shift in the estimated break-even point. However, the manuscript never evaluates whether these observed differences are themselves statistically significant. Without appropriate statistical testing—such as confidence intervals, bootstrap resampling, or formal tests comparing regression coefficients and goodness-of-fit—it is impossible to determine whether these differences reflect genuine structural changes associated with NIL or simply arise from sampling variability. Consequently, the evidence currently supports that the models differ descriptively, but it does not yet demonstrate that the observed differences are statistically meaningful.

Furthermore, the manuscript interprets the modest increase in R^2 as evidence that revenue became a stronger determinant of athletic success following NIL implementation. This interpretation is stronger than the statistical evidence presently supports. An increase in R^2 merely indicates that the fitted model explains a larger proportion of the observed variance in the selected dataset; it does not, by itself, establish that NIL caused athletic performance to become more economically driven. Alternative explanations—including conference realignment, changes in competitive parity, differences in variance structure between seasons, or measurement variability—could also contribute to the observed differences. These alternative explanations should either be formally evaluated or acknowledged as limitations when interpreting the results.

Finally, while the author has partially addressed the previous recommendation to examine competitive balance, the current analysis remains relatively limited. Counting the number of Group of Five and Independent teams with negative SRS values provides only a coarse descriptive comparison and does not directly quantify competitive balance. More informative analyses, such as changes in playoff participation concentration, Top-25 representation, conference championship distribution, recruiting concentration, or inequality metrics (e.g., Gini or Herfindahl indices), would provide substantially stronger evidence regarding whether NIL altered competitive balance across Division I football. These analyses are not optional additions but are necessary to substantiate the manuscript's central claims. Without them, the conclusions regarding the broader impact of NIL on competitive balance remain insufficiently supported.

Although the revised manuscript is methodologically stronger than the previous version, it still does not establish sufficient scientific novelty. The principal conclusion—that financial resources became a somewhat stronger predictor of athletic performance following the introduction of NIL—is largely consistent with what would already be expected from the structural changes introduced by NIL. The manuscript therefore provides a quantitative description of an intuitively anticipated outcome rather than revealing a previously unknown mechanism or generating a fundamentally new scientific insight. Even if the statistical comparisons between the 2017 and 2025 models were strengthened, the resulting conclusion would remain that “money matters slightly more after NIL,” a finding that is unlikely to substantially change current understanding of collegiate athletics.

To justify publication as an original research contribution, the manuscript should move beyond confirming an expected trend and instead investigate a question whose answer is not already apparent. For example, the study could identify which categories of programs benefited disproportionately from NIL, determine whether NIL measurably altered competitive balance using established quantitative metrics, identify structural breakpoints across conferences, or examine whether the relationship differs by recruiting tier, historical program prestige, or transfer-portal activity. Such analyses would generate mechanistic insight into how and where NIL altered the collegiate football landscape rather than simply confirming that financial resources are associated with competitive success.

Dear Reviewer,

Your constructive discussion on my work was very much appreciated. I finally grasped the lack of statistical work that I had put into my work, simply assuming things rather than testing them. Through this I have pivoted the research paper into something much stronger, and I have you to thank.

“The manuscript's central conclusion is based on comparing the regression characteristics of the 2017 and 2025 models, including the increase in the coefficient of determination ($R^2 = 0.5001$ versus 0.5512), the change in the regression coefficient, and the shift in the estimated break-even point. However, the manuscript never evaluates whether these observed differences are themselves statistically significant. Without appropriate statistical testing—such as confidence intervals, bootstrap resampling, or formal tests comparing regression coefficients and goodness-of-fit—it is impossible to determine whether these differences reflect genuine structural changes associated with NIL or simply arise from sampling variability.”

This critique has been fully addressed, employing confidence intervals, bootstrap sampling, and Chow testing in order to ensure statistical strength and significance. There is now an entire Statistical Testing section to prove hypotheses rather than simply trusting them.

"Furthermore, the manuscript interprets the modest increase in R^2 as evidence that revenue became a stronger determinant of athletic success following NIL implementation. This interpretation is stronger than the statistical evidence presently supports. An increase in R^2 merely indicates that the fitted model explains a larger proportion of the observed variance in the selected dataset; it does not, by itself, establish that NIL caused athletic performance to become more economically driven.”

The revised paper has removed this central finding, in fact, as a part of the two-part finding of the revised paper, statistical testing has concluded that NIL can not predict athletic success as strongly as suggested in the last paper, but rather it can predict the broadening of the recruiting class to various schools rather than the usual historically dominant programs. This two-part conclusion is alluded to in the statistical testing and competitive balance in recruiting sections and completely answered in the comparison section.

“Finally, while the author has partially addressed the previous recommendation to examine competitive balance, the current analysis remains relatively limited. Counting the number of Group of Five and Independent teams with negative SRS values provides only a coarse descriptive comparison and does not directly quantify competitive balance. More informative analyses, such as changes in playoff participation concentration, Top-25 representation, conference championship distribution, recruiting concentration, or inequality metrics (e.g., Gini or Herfindahl indices), would

provide substantially stronger evidence regarding whether NIL altered competitive balance across Division I football.”

Completely agree that these analyses are needed. Using recruiting concentration, I found the second part of the two-part conclusion, which is that NIL has fundamentally changed the competitive balance of Division I football by allowing recruits to widen their prospective schools. Whether this will translate to a better record for these schools is an entirely different-and equally important-question, however removing the negative SRS values for this stronger, recruitment analysis allowed the paper to gain another strong component of its overall insightfulness. There is an entire section now with recruiting concentration discussed in depth.

“To justify publication as an original research contribution, the manuscript should move beyond confirming an expected trend and instead investigate a question whose answer is not already apparent. For example, the study could identify which categories of programs benefited disproportionately from NIL, determine whether NIL measurably altered competitive balance using established quantitative metrics, identify structural breakpoints across conferences, or examine whether the relationship differs by recruiting tier, historical program prestige, or transfer-portal activity.”

I believe the paper is now ready for publication, as it demonstrates both quantitatively and qualitatively how NIL altered competitive balance, reinforcing this demonstration with statistical work and explanatory reasoning by modification of the recruiting landscape. All parts of the paper now work towards painting a picture of, statistically proving, and explaining the final, two-part conclusion that NIL did not alter the financial-performance relationship of FBS football, but that it significantly altered the recruiting landscape.

After a week of rebuilding my statistical strength, pivoting the main finding of the paper into a two-pronged conclusion, and ensuring the paper clarifies a question that is currently in contention, I believe the paper is much clearer and more comprehensive than before.

The manuscript has been substantially improved, and I commend the author for responding constructively and seriously to the previous review. In particular, the addition of formal statistical testing changes the scientific strength of the paper considerably. The author now appropriately demonstrates that the apparent changes in the revenue–performance regressions between 2017 and 2025 are not statistically distinguishable, rather than interpreting the modest change in R^2 as evidence of a structural effect. The addition of the recruiting analysis is also a meaningful improvement: the Gini coefficient decreased from 0.1767 in 2017 to 0.1451 in 2025, and the bootstrapped confidence interval for this difference excludes zero. Thus, the revised manuscript now contains a statistically supported finding beyond the original descriptive comparison, and the author’s willingness to modify the central conclusion in response to the data has made the paper considerably stronger.

I do, however, think that one important interpretive issue remains and should be corrected before acceptance. The Gini analysis demonstrates that total recruiting-point scores were more evenly distributed among FBS programs in 2025 than in 2017. This is a valid and potentially interesting result. However, the manuscript sometimes interprets this finding more specifically as showing that recruiting talent, particularly top talent, became less concentrated among historically dominant programs. The Gini statistic used here cannot by itself establish that mechanism. It measures inequality in schools’ aggregate recruiting-point totals; it does not identify which recruits moved between which categories of programs. The manuscript itself describes the Gini as indicating whether dominant programs capture the highest-talent recruits, but that interpretation goes beyond what the aggregate statistic directly measures.

Several different changes could produce the observed reduction in the Gini coefficient. Lower- and middle-ranked programs could have improved their overall recruiting classes without acquiring substantially more of the very highest-ranked recruits; the recruiting scores of the strongest programs could have declined; or the middle of the distribution could simply have become more compressed. Because the analysis uses total recruiting points, changes in recruiting class composition or size could also contribute. Moreover, the author appropriately acknowledges that recruiting points are an opaque scoring system and that a season's total can reflect roster-building decisions extending across multiple seasons. Consequently, "recruiting-point totals became more evenly distributed" and "elite talent became more widely distributed" are related hypotheses, but they are not statistically synonymous.

This distinction is important because the manuscript currently states that recruiting talent became significantly less concentrated "among the top programs" and suggests that NIL may be allowing top talent to consider a broader range of schools. The first part of that interpretation is not directly demonstrated by the current Gini analysis. To establish it, the author would need a more targeted analysis—for example, comparing the proportion of 5-star, top-50, or top-100 recruits captured by the top 5, 10, or 20 recruiting programs in 2017 versus 2025. Such an analysis would distinguish genuine redistribution of elite recruits from a more general equalization of aggregate recruiting scores.

I do not necessarily require this additional analysis for the present manuscript, provided the claims are appropriately narrowed. The demonstrated result is already scientifically useful: the distribution of aggregate recruiting-point totals across FBS programs was significantly more equal in 2025 than in 2017, whereas the study detected no statistically significant change in the revenue–performance relationship. The manuscript should therefore state this result directly and describe redistribution of elite talent as a possible interpretation or hypothesis rather than as an established finding. Similarly, because the study compares two years rather than implementing a design capable of isolating NIL from other contemporaneous changes, the author should continue to use formulations such as "in the post-NIL period" or "consistent with a possible effect of NIL," rather than stating that NIL itself caused the observed redistribution.

With these interpretive revisions, I believe the manuscript would be substantially better calibrated to what the analyses actually demonstrate. The author has done considerable work in response to the previous review, and the revised paper now has a much more defensible and interesting result. The remaining changes are principally about ensuring that the strength and specificity of the conclusions match the evidence, rather than requiring another major reconstruction of the study.

Dear Reviewer,

Thank you for your constructive feedback on this manuscript. I believe, after fixing improper wording, that it should be ready for publication.

"The Gini analysis measures inequality in aggregate recruiting-point totals; it does not identify which recruits moved between which categories of programs. The manuscript's interpretation goes beyond what the aggregate statistic directly measures."

To this, I revised all Gini-related language to refer to "recruiting point totals" rather than "recruiting talent", which is found in many sections, including the Methodology, Results, Lorenz Curve, and Comparison sections.

"Several different changes could produce the observed reduction in the Gini coefficient. "Recruiting-point totals became more evenly distributed" and "elite talent became more widely distributed" are not statistically synonymous."

Here, I added a brief note in the Gini Coefficient Methodology section that the result is consistent with multiple possibilities and cannot isolate elite-talent movement specifically.

“Because the study compares two years rather than isolating NIL from other contemporaneous changes, use formulations like ‘in the post-NIL period’ or ‘consistent with a possible effect of NIL,’ not causal claims.”

I replaced casual wording throughout the Lorenz Curve, Introduction, and Comparison sections to avoid informal claiming.

“With these interpretive revisions, the manuscript would be better calibrated to what the analyses demonstrate.”

To respond to this, I have reviewed the full manuscript, aligning my claim strength with the statistical evidence, leaving data and analysis unchanged.

The manuscript has been substantially improved, and I commend the author for responding carefully and constructively to the previous comments. In particular, the revised manuscript now correctly distinguishes between the distribution of aggregate recruiting-point totals and the distribution of individual elite recruits. The Gini methodology appropriately states that a lower Gini indicates that recruiting-point totals are more evenly distributed across programs and explicitly acknowledges that this analysis cannot determine which programs acquired the highest-rated recruits or track the movement of individual recruits. The Lorenz-curve discussion similarly identifies redistribution of elite talent as a possible hypothesis rather than a directly demonstrated result. These revisions substantially improve the scientific calibration of the manuscript.

However, this distinction has not yet been applied consistently throughout the manuscript. For example, the Introduction still concludes that “recruiting access has measurably broadened” and suggests that NIL’s primary impact may have been to “redistribute talent.” The Post-NIL Analysis similarly states that smaller programs are now able to compete for elite recruiting talent through NIL and discusses how elite programs deploy their financial resources. These statements go beyond what the present analysis demonstrates. The statistically supported finding is narrower: aggregate recruiting-point totals were significantly more evenly distributed across FBS programs in 2025 than in 2017. Whether this resulted from redistribution of elite recruits, improvement among middle- or lower-ranked programs, declining recruiting scores among dominant programs, or some combination of these possibilities cannot be determined from the Gini coefficient alone.

A similar problem remains in the Comparison section, where the manuscript states that NIL has “measurably” reduced the concentration of recruiting-point totals “among the top programs.” The Gini analysis establishes greater equality across the entire distribution of FBS programs; it does not specifically establish reduced concentration among the top programs. Demonstrating that claim would require an upper-tail analysis, such as comparing the proportion of recruiting points or top-50/top-100 recruits captured by the top 5, 10, or 20 programs. I am not requiring such an additional analysis, provided the present claims are narrowed. This sentence could instead state that aggregate recruiting-point totals were significantly more evenly distributed across FBS programs in 2025 than in 2017, a finding consistent with, but not proof of, reduced recruiting concentration among historically dominant programs.

The manuscript should also undergo one final review for causal language and tense (please write in third person wherever possible; else convert all the personal pronouns (I) to (We)). Because the study compares 2017 with 2025 and does not isolate NIL from other changes occurring during this period, it can establish an association with the post-NIL period but cannot establish that NIL caused the observed change. Phrases such as “what NIL has done” or “after the application of NIL” should therefore be replaced with formulations such as “in the post-NIL period,” “between 2017 and 2025,” or “consistent with a possible effect of NIL.” This distinction is especially important because

the manuscript itself appropriately acknowledges that two proxy years cannot establish a complete temporal trend.

There are also several remaining causal interpretations of the regression that should be corrected. For example, the manuscript states that each additional \$100,000 in revenue “will produce” a particular increase in SRS. An observational regression does not demonstrate that adding \$100,000 causes the corresponding improvement in performance. These statements should instead describe the fitted association—for example, “at this revenue level, the fitted regression corresponds to approximately 0.80 additional SRS units per \$100,000.” Similarly, the regression zero-crossing should be described as the revenue level at which the fitted model predicts SRS = 0, rather than implying that programs spending above this threshold can causally expect above-average performance.

Finally, the citation and reference formatting requires correction before publication. The manuscript does not currently use Vancouver-style numbered in-text citations. Sources discussed in the text should be cited sequentially using the journal’s required numerical Vancouver convention. The reference section is also not consistently formatted in APA style: the first several website references use a different format from the final 247Sports references, and the section as a whole is presented as an inconsistent bulleted list. Please ensure that all in-text citations follow the required Vancouver numerical convention and that every entry in the final reference section is consistently formatted according to APA requirements.

Overall, the author has successfully addressed the central scientific concern of the previous review, and I do not believe another major analysis is necessary. The remaining revision is primarily one of consistency and interpretation: the manuscript should state precisely what the statistical analyses demonstrate and clearly separate those findings from plausible but untested explanations. The central supported conclusion is that aggregate recruiting-point totals were significantly more evenly distributed across FBS programs in 2025 than in 2017, while no statistically detectable structural change was found in the revenue–performance relationship. Once the remaining stronger causal and elite-talent claims are brought into alignment with that evidence and the citation/reference formatting is corrected, the manuscript should be considerably better positioned for publication.

Dear Reviewer,

Thank you for your continued feedback on this manuscript. Below, I address each remaining comment in turn.

"However, this distinction has not yet been applied consistently throughout the manuscript. For example, the Introduction still concludes that 'recruiting access has measurably broadened' and suggests that NIL's primary impact may have been to 'redistribute talent.' The Post-NIL Analysis similarly states that smaller programs are now able to compete for elite recruiting talent through NIL... These statements go beyond what the present analysis demonstrates."

Systemically edited the Introduction and Post-NIL Analysis to consistently refer to "recruiting point totals" rather than "recruiting talent," removing all language implying elite-talent redistribution specifically.

"The manuscript should also undergo one final review for causal language and tense (please write in third person wherever possible; else convert all the personal pronouns (I) to (We)... Phrases such as 'what NIL has done' or 'after the application of NIL' should therefore be replaced with formulations such as 'in the post-NIL period'..."

Converted first-person pronouns to third person or "we" throughout the Methodology, Chart Domains, and remaining sections, and replaced causal NIL phrasing with statements such as "in the post-NIL period" and "consistent with a possible effect of NIL."

"There are also several remaining causal interpretations of the regression that should be corrected. For example, the manuscript states that each additional \$100,000 in revenue 'will produce' a particular increase in SRS... Similarly, the regression zero-crossing should be described as the revenue level at which the fitted model predicts $SRS = 0$, rather than implying that programs spending above this threshold can causally expect above-average performance."

Revised the marginal-gain tables in both the Pre-NIL and Post-NIL Analysis sections to describe fitted associations ("the regression corresponds to approximately X additional SRS points per \$100,000") rather than causal production language, and revised both break-even point descriptions to state the revenue level at which the fitted model predicts $SRS = 0$, rather than a causal expectation of above-average performance.

"Finally, the citation and reference formatting requires correction before publication... Please ensure that all in-text citations follow the required Vancouver numerical convention and that every entry in the final reference section is consistently formatted according to APA requirements."

Added Vancouver-style numbered in-text citations [1]–[6] at each point a source is referenced and reformatted the reference list into a consistently APA-formatted, numbered list corresponding to those citation numbers.

Thank you for addressing my comments. Please provide either a .docx or an .odt file of the manuscript correctly formatted to the Journal's requirements (see website). We will send out a formal acceptance once we receive this document.

Best,

Shireesh Apte

Dear Reviewer,

"Correctly formatted to the Journal's requirements"

The manuscript has now been sufficiently reviewed and formatted to the Journal's requirements.

Let me know if I missed anything.

Thank you for addressing my comments. Accepted.