

Leverage over Life Outcomes: Exploring Agency among College Quarterbacks

ABSTRACT

Agency is a famously “slippery” sociological topic. Scholars have long disputed its definition and characteristics, and its entanglement with its counterpart, structure, has made it difficult to examine its role in stratifying socioeconomic outcomes. To be specific, and partly because of issues with data quality, empirical research has been much more successful in establishing group- or category-level stratification than individual-level stratification. To look for this between-actor variation, I investigate a long-theorized but under-specified dimension of agency: the variability over outcomes in the social world entailed by an actor’s discrete choices, which I call “leverage.” Using high quality data from American college football, I conduct a study on the impact players’ choice of college program has on their eventual probability of being drafted into the National Football League. I present strong evidence that a socially mobile middle class of talented but non-elite players have the most leverage over their desired outcome in this case. I plot the nonlinear relationships between leverage and more canonical dimensions of agency: status, options, and performance. I propose that understanding agency not as an underlying concept that can be measured by itself but as the synthetic product of the dialectic between its dimensions helps establish a good foundation for productive sociology in practice.

INTRODUCTION

We tell ourselves that the courses of our lives hinge on important decisions. In everyday life, we are constantly confronted with small choices (like what to get for dinner), and from time to time we find ourselves at crossroads that feel much more significant (like where to go to college). When we perceive these moments to have been consequential for us, they come to dominate the narratives we tell about our lives (Ewick and Silbey 1995). They feel so impactful because we understand that choosing one option over another cuts off whatever future paths we might have had if we had gone the other way; that is, there are countless potential life courses behind door B that we will never experience if we open door A, and vice versa. Under normal conditions, it is impossible to know what range of outcomes we collapse by socially acting, so it is also difficult to determine what difference the actions we did take actually made. But whether

we can observe it or not, there is a fundamental relationship between stratification and social mobility that emerges from actions and their consequences. Making different choices, and making them under different circumstances, apparently lead to different outcomes. Over time, the difference this process induces between life courses becomes a pattern: stratification.

We often speak of something called *agency* when we talk about life choices, social action, and their outcomes, but there is no field of study or body of literature that has proven as prone to litigating and redefining agency as sociology. Indeed, agency is one of the most persistently “slippery” concepts in sociological theory (Hitlin and Elder 2007; Bazzani 2022). It seems intuitively clear that individuals make choices and take actions that have consequences, both intended and unintended, for their lives and the world around them (Merton 1936; Giddens 1984). But even as sociologists have enjoyed decades of spirited debate about who has agency and how much of it they have, we have been unable to pin down what the thing we are trying to argue about is. In the aptly titled *What Is Agency?*, Emirbayer and Mische (1998) write that “the term *agency* itself has maintained an elusive, albeit resonant, vagueness; it has all too seldom inspired systematic analysis, despite the long list of terms with which it has been associated” (962). The authors valiantly define agency as “the temporally constructed engagement by actors of different structural environments” (970), but by their own acknowledgement this definition cannot satisfy every mutually exclusive expectation of the arguments in the literature that preceded it, nor did this attempt itself bring consensus. Sociologists since Emirbayer and Mische have retooled their definition further or come up with their own to supersede it (Archer 2003; Hitlin and Johnson 2015; Bazzani 2022). It seems impossible to completely synthesize decades of sociological thinking and writing about agency. The ideas are so divergent and flexible that they cannot be woven together into a succinct or methodologically applicable definition.

Further complicating this state of affairs is the eternal sociological counterpart to agency; that is, structure. Giddens (1984) defines it as the “rules and resources” that govern social action, language Sewell (1992) refines into “schemata and resources” (virtual and material social objects that comprise the situational context of agency). Over many decades of sociological theory, the infamous “structure and agency debate” litigated and re-litigated which elements of society were apparently structural and which were apparently agentive, whether one caused the other or could simply be reduced to the other, and so on, coming to no satisfactory conclusion other than that neither is easily defined and that you cannot discuss one without considering the other (Archer

2003). Indeed, even the Emirbayer and Mische (1998) definition of agency includes “structural environments” in and of itself. Hays (1994) identifies a definitional chauvinism to structure that is all too reminiscent to scholars of agency, describing it as those “patterns of social life that are not reducible to individuals.” She contends that, for far too long, sociologists thought of structure as a kind of implicate order, pure constraint, whereas they thought of agency as a kind of underlying entropy, pure freedom. Following Giddens (1984) and Bourdieu (1984), Hays reimagines the relationship between structure and agency to be one of mutual enablement rather than competition: structure allows, encourages, and incentivizes action along culturally embedded lines, and actors question, navigate, reshape, and participate in structure.

This dialectical approach to structure and agency has proven more successful for the development of novel theory than the diametrical approach that characterized the historical “structure and agency debate,” but the lion’s share of the generative quantitative research that has come out of it has primarily concerned structure rather than structure *and* agency. In a sense, structure is the golden child of the sociology of stratification, but agency is the problem child. We understand well how life choices are stratified: different groups of people have differential access on average to opportunities for social mobility. But it is much harder to investigate how they are stratifying: what it means for someone to have or not have, or take or not take, options that they “should” according to structural factors, and how a person’s outcomes and experiences might vary even when accounting for structure are exotic problems in stratification research, largely because of a lack of sufficiently fine-grained, individual-level data to explore them. Research into individual-level stratification like this, how life courses differ between individuals based on their actions and how they *would* differ for the same individual should they choose door A or door B, are thought of as the domain of other social sciences like economics and psychology (Hitlin and Johnson 2015). The result is that there is a lot of idle theorizing about agency and a lot of productive sociological research about structure (Bazzani 2022). Even that empirical literature which does prominently feature agency, mostly that which examines major career and social decisions along the life course, ultimately concerns itself with differences in means across groups (Bowen and Bok 1998; Schoon and Lyons-Amos 2016; Billari et al. 2019).

But even though individual-level stratification has fallen by the empirical wayside, we are deeply motivated by theory to pursue it should adequate data be available. For Giddens (1984), agency depends upon “the capability of the individual to ‘make a difference’ to a

pre-existing state of affairs or course of events” (14). Granovetter (1985) tells us to look for circumstances in which actors act according to our expectations, and circumstances in which they defy our expectations. Emirbayer and Mische (1998) claim that agency “both reproduces and transforms” (970) the structure that contextualizes it. Hitlin and Johnson (2015) say that agency is essential for understanding stratification longitudinally. Bazzani (2022) presents agency as a “conversion process” where an actor uses resources to influence the world. There is an obvious thread here, an oft-implied but under-theorized dimension of agency: the variability over outcomes in the social world entailed by an actor’s discrete choices. If there is a difference that might be made along a course of action, then how great is that potential difference for each actor? What kind of data do we need to measure it?

I am going to use this paper to do three things. First, I will theoretically explicate this underdeveloped dimension of agency, which I call *leverage*, the variability in outcomes entailed by individual actors’ discrete choices under structural conditions. Second, I will study leverage empirically in a specialized social context, college football and the NFL draft. I will demonstrate that the uncommonly granular publicly available data from this *milieu* is suitable for an in-depth analysis of leverage across individuals. I will use this very specific social world as a test case for individual-level stratification research, with clear stakes, actors, and choices that produce striking patterns of leverage. I will show that the distribution of leverage in this case is concave, highest among a pinched middle class of actors with something to lose, and driven by diversity of options rather than raw number of options. I will also use several case studies to show how leverage relates to the diverse pathways to being drafted that players navigate under heavy structural constraints, following Certeau (1980). Last, I will use my results to affirm the dialectical *detente* that seems to have quieted the “structure and agency debate.” The nonlinear relationships between leverage and more canonical dimensions of agency suggest that agency, like structure, is best understood not as a quantifiable concept with a “true” underlying definition but as the synthetic product of the dialectic between its dimensions (Hays 1994, Emirbayer and Mische 1998, Hitlin and Elder 2007, Bazzani 2022).

BACKGROUND

While certain stalwarts of the “structure and agency debate” such as Archer (2000) might contend that sociology has unduly determined agency to simply be structure in disguise and

incorrectly moved on from it, most contemporary scholars follow (or anticipate) Fuchs (2001) in eschewing the “bad essentialist habit” of assuming that agency and structure are opposites, or that one creates the other, or that they can ever be fully parsed. Wharton (1991) acknowledges the tendency of feminist social theory to overindulge in either structural or agentic assumptions about gender inequality, and proposes adopting an alternative conception that involves both structure and agency. Hays’s (1994) critique of the sociology she was in conversation with for its tendency to oppose structure and agency instead of understanding them as reflections of one another, and the literature has well taken her point. Authors now tend to follow McAdams (2001) and Archer (2003) by pursuing a “deliberative, self-aware social actor embedded within an ongoing life project.” As this actor makes choices and takes social action, they influence objective outcomes and subjective experiences in their social life. The degree to which such an outcome or experience can be influenced by the actor — that is, the variability in the social world entailed by a discrete social action — is a dimension of agency we can call *leverage*. Emirbayer and Goodwin (1994) suggest that the then-new field of social network analysis adopt the so-called “anticategorical imperative” that we look first at dynamic relational properties before we look at essentialist categorical properties when trying to examine patterns of sociality. Leverage as a driver of individual-level stratification is a useful tool to be true to this principle.

But we must be careful in this task not to think of actors as atomized. Hitlin and Johnson (2015) describe “an actor motivated by the accumulated sense of efficacy and optimism (or not) that has been developed and internalized in reciprocal interaction with objective social structures and other social actors.” There is no such thing as social life without its context. Archer (2003) attempts to resolve this tension with her modern classic formulation of the “internal conversation” of agency. An actor implicitly understands that they have and exercise discretion in life and that their life course and life chances might be different should they act in one way as opposed to another. But these potential differences emerge from social structures that bear capital rewards and cultural expectations for behaving in certain ways (Meyer and Jepperson 2000), so as subjects they experience in themselves the stratification those structures create between actors competing in a capital-driven field. As the actor weighs their choices, they imagine versions of themselves that enact each option, and predict how each parallel life will fare in converting abstract leverage into a concrete difference in the world (Giddens 1984; Archer 2003; Bazzani 2022). Then they act however they choose to act, collapsing the wave function so to speak, and

discover what, if any, difference they have wrought. This is reflexivity, “the monitored character of the ongoing flow of social life” that Giddens (1984) identifies as just as essential a component of agency as the capacity to make a difference at all.

Despite the elegance of these ideas, they have proven less productive methodologically than they have theoretically (Bazzani 2022). Empirical sociology concerning opportunity structures does not ignore agency, but it does tend to treat it as a mechanistic pathway by which opportunity structures produce and reproduce differences in means between groups instead of as what actors use to navigate and shape those structures. Most available data regarding life decisions is on the group level, so quantitative research on the effects of these decisions tends to pertain to group-level stratification. Schoon and Lyons-Amos (2016) do interrogate the binary framing of young people’s paths into adulthood as “optimal” or “problematic,” but their analysis still ultimately produces five clusters of pathways instead of two. The authors show that both structural and agentic factors predict cluster membership, but the stratification they care about is still between groups rather than at the individual level. Billari et al. (2019) employ a threefold framing of “stratified socialization,” “stratified agency,” and “stratified opportunity” to mechanize the transition to adulthood: if young people come from a higher-SES background, they are likelier to intend to leave the family home earlier and less likely to intend to have their own children soon after leaving, they are likelier to be able to actualize these intentions, and they are likelier to get chances for these events to actually occur. This analysis, like that of Schoon and Lyons-Amos, is impressive, but because they lack granular individual-level data, they observe agency only as a mechanism for stratification by socioeconomic status, not at the individual level. What we need is sufficiently detailed, individual-level data that concerns the options available to individual actors, for those actors to be statistically comparable, and to have an outcome recorded for each actor over which the leverage can be measured. I will demonstrate an example of such data in this paper.

I argue that sociological theory has implied the significance of the leverage dimension of agency, the variability in social outcomes that a social action may induce, for nearly a century without naming it as such. Merton (1936) is interested in the “consequences” of social action, what occurs in the world that would not have occurred had the action not taken place, which emerge from “the interplay of the action and the objective situation, the conditions of action” (895); that is, the interplay of agency and structure. Certainly Giddens (1976, 1984) but also

proto-structurationists as early as Parsons (1937) and Gurvitch (1955), in their interest in the difference that action makes in society, assume that differences are themselves differentially able to be made depending on the action and its context. Bourdieu (1972, 1984), at least interpreted in the most orthodox sense, might regard leverage at best as a determinant of the speed at which the *habitus*, for Bourdieu the only meaningful inducer of social action at least outside times of crisis, can change itself by inspiring and commanding action. Certeau (1980) and Harvey et al. (2020) can help apply some Bourdieusian ideas to develop expectations about the distribution of leverage. There are high-status “hyper-agents” who adopt capital-driven strategies that shape fields according to their leverage, and there are lower-status actors who adopt tactics to navigate those fields according to their leverage. If there were no leverage, there could be no meaningful shaping, no meaningful navigation. Sewell (1992) compares Giddens and Bourdieu to develop his own theory of structure as “schemata and resources” that govern action, ideas that will prove valuable in my analysis in this paper. Bazzani (2022) follows Emirbayer and Mische (1998) in his general definition of agency, but is primarily concerned with its role as a “conversion process” between resources and outcomes. Leverage reflects the rate and degree of this conversion; that is, how much variability might be induced by the process.

Schoon and Lyons-Amos (2016) find that Giddens’s assumptions that people are mostly free to choose their own life path, and that “planfulness” (a manifestation of reflexivity) in life decisions matters, seem to hold up well when multiple viable options exist, but break down when the economy is bad and actors have few to no meaningful choices with regard to accumulation of capital. In those cases, they say, it is not discrete choices but structural factors that primarily drive stratification. However, the economy is better for some people than it is for others. The hyper-agents have a smorgasbord of “good” options before them — a prize behind every door on the game show of life, a “phone-a-friend” or other bailout whenever they make a choice that won’t pay off — but society’s deprived seem to find only rubbish waiting for them, no matter how well they play the game. Schoon and Lyons-Amos (2016), though, argue that many young people belong to neither of these binary categories but instead to the “forgotten middle”: those for whom very long term unemployment is unlikely, but whose access to elite educational resources and the strong network benefits enjoyed by the hyper-agents is limited. For these individuals, reflexivity is not an illusion. They can make “bad” choices without being bailed out like the hyper-agents, and make “good” choices that actually yield returns they would not for the

deprived. These actors are at once constrained and agentive, socially mobile, both free and forced by the social structures they participate in to make consequential decisions that stratify the courses of their lives. As Hays (1994) encourages us to remember, social structures are not “pure constraint,” walls that keep us from acting how we please and rails that guide us fatalistically to outcomes determined solely by the circumstances of our birth and random chance throughout our lives. Just as Hallett and Ventresca (2006) argued of institutions, structures are inhabited by actors whose inhabitation of them produces and reproduces culture, structure, and agency. There is no shortage of theory pertaining to this difference-making dimension of agency, but there is a shortage of quantitative data to examine its role in driving individual-level stratification.

DATA AND METHODS

The careership research, and much of the “measuring agency” literature more broadly, relies heavily on unusually high-quality data (Dale and Krueger 2002; Hitlin and Long 2009; Jejeebhoy et al. 2010; Franzosi et al. 2012; Schoon and Lyons-Amos 2016, 2017; Billari et al. 2019). A measurer of leverage must know what options were available to individual actors, be able to statistically compare actors and options in a robust way, and relate the options chosen by actors to an outcome over which the leverage is to be estimated. These conditions must all be met for a complete exploration of leverage and how it is distributed across actors within an empirical case. For this project, I tap into an excellent source of panel data that has gone regrettably underutilized by quantitative social science: college sports.

Empirical Aims

The case I focus on is the college commitment decision for high school football players, specifically those who are highly regarded by scouts and heavily recruited by division 1 coaches. This is a major life decision that may promise truly transformative returns in social and financial capital for players, their families, and potentially their entire communities if the player is drafted into the National Football League (NFL) at the end of their college career. While these recruits must consider many of the same factors their non-sports playing peers do when deciding where to go to school, the allure of an NFL contract is impossible to ignore. These actors are reflexive: they know where they go to college might matter for the NFL and they make their decision accordingly. Even if making it to the league is not their top priority (for most recruits, it is) the

possibility that they will end up “playing on Sundays” is at the front of their minds as they decide which scholarship offer to accept (Crabtree, 2022). The pressurized, dramatic environment of college football exhibits numerous characteristics that make it appealing for sociological study. It is a crucible where talented young people compete for status, money, and fame that only a few of them will enjoy for their entire lives. The standards of this competition are established and altered by institutions like the National Collegiate Athletic Association (NCAA), college athletic conferences, and university administrations. Players and their families must navigate these changing rules of engagement or risk receiving nothing for their enormous investments of time, energy, and money into a game that is much more likely to result in permanent health problems than fame and fortune. Many of us make decisions that resemble this one — where to go to college is a question millions of Americans face every year — but rarely are the stakes so clear, the actors so aware of their options, the rewards so obvious, the risks so dangerous. In this case, we are lucky to have the data to model them.

In my analysis, I first identify a model for draft probability. This model takes into account player-side statistics including race, recruit ranking out of high school, and in-game performance metrics like passing yards and touchdowns. It also involves school-side statistics including Colley matrix rating, a statistical method used to measure a team’s performance in the context of their competition, (Colley 2002), whether the school is in a “power” conference, and offensive scheme. Both player- and school-side statistics impact the outcome considerably. By keeping the player-side statistics the same for each player (a simplification I will address later), but changing the school to each one that gave the player an offer, I use this model to predict draft probability for each plausible school choice for each player. I then graph and analyze the outputs for each plausible counterfactual, the full picture of which is a representation of the social process where recruited players out of high school commit to college in an effort to be drafted into the NFL. I represent leverage as the interquartile range of modeled draft probabilities for each player (precisely the variability over an outcome of interest induced by actors’ choices), and I examine relationships between leverage and relevant player- and school-side predictors that represent more traditional dimensions of agency like status and performance.

I want to make clear that this analysis is not meant to represent the contemporary empirical reality of American college football. This is a paper about structure and agency that happens to use football as a valuable empirical site. Significant shocks to the world of college

sports over the past decade have completely transformed the opportunity structures I examine in this paper. These include but are not limited to the introduction of the “transfer portal” (which I will discuss briefly later), the return to players of their long-abrogated rights to profit from their name, image, and likeness by the U.S. Supreme Court, and other court decisions that have reshaped how finance works in college sports. This paper will look at an extremely specific slice of American life that existed between 2011 and 2017 as a test case for studying leverage where the actors, choices, and outcomes are all well-understood, publicly recorded, and well-bounded by poor data on one end and big structural changes on the other.

Case Background

High school football players who are perceived to show promise for college and beyond are given ratings by the three main scouting agencies: ESPN, Rivals.com, and 247Sports.com. These ratings, binned from 1 to 5 stars, have some grounding in players’ season statistics, but high school football schedules are totally competitively asymmetrical and do not represent a good basis for comparison. Instead, recruit ratings are mostly subjective, determined largely by how physically fit a scout thinks a player is, or whether they perceive him to have “intangible” characteristics that won’t show up in the stats, or whether he happens to play well in the one game out of the season that the scout is present for. This means that great players are frequently overlooked and given 1-, 2-, or 3-star ratings, whereas the players with 4- and 5-star ratings — otherwise known as “blue chip” recruits — are usually outstanding performers in high school, but only at high schools that are lucky to get sufficient media attention. There are also strong herding effects in scouting, where a player being highly rated by one firm means that the other firms will give him more attention. Players who fly under the radar may have to turn to social media to get coverage or hope that their coach knows a scout.

Players are nationally ranked by scouts according to their position, so there will be, for example, a top 50 list of quarterbacks across the United States who receive the bulk of attention from division 1 (D1) universities, those that can offer full scholarships for football, each year. Players who impress these scouts and the coaches they represent are extended scholarship offers, one of which they choose to commit to by National Signing Day (usually in early February). For heavily recruited “prospects” — called as such because they are perceived to have a good chance of excelling in college or the NFL eventually — this is a highly ritualized process, with

the commitment decision often being made official in front of the player's teammates, family, and community, sometimes in publicized, special events.

Recruited high school football players are some of the most socially privileged young people in American society, and upon entering a D1 football program, they surrender most of their freedom to their coaches in exchange for the often dishonest implication that those coaches will do their best to prepare them for the NFL (Logan et al. 2015). They adjust to heavily regimented practice and travel schedules, become local and national celebrities if they succeed on the field, and in recent years see details from their personal lives broadcast on social media. All the while, they compete with their teammates for starting roles, their opponents for wins, and themselves to improve at the game enough that they begin to get noticed by NFL teams. Players who become the elect — the 1.5% of D1 players who will end up playing in the pros (NCAA 2024) — must excel at the game, get media attention, perform well in interviews with teams, succeed at the NFL combine (an invite-only group workout in front of the media and coaching staffs), and get lucky enough that a team has a weakness or an opening at the position they play. If all those conditions are met, then a player may hear his name selected during the NFL draft, a 3 day process by which teams compete against and bargain with one another for the pick of the litter from that year's college players. The NFL, unlike other American professional sports leagues, requires players to be out of high school for at least 3 years to be eligible for the draft, so most players drafted will have played in college for between 3 and 5 years. Each player drafted, especially those taken in the first round, experiences sudden notoriety and receives a lucrative rookie contract.¹ Both the financial and social capital entailed by this process will grow exponentially if the player succeeds enough in the league to continue on to a full NFL career.²

I look at only quarterbacks for this study because of certain unique characteristics the quarterback position has. These peculiarities are methodologically useful for the analysis to

¹ The lowest possible base salary for a rookie in the NFL, before many performance-based bonuses, available in 2014 — when the first player in my population was drafted — was in excess of \$400,000 yearly, for four years.

² The literature shows, and it must be noted here, that NFL teams are not actually particularly good at evaluating quarterbacks for the draft, at least in the sense that there is only a weak relationship between how teams rate QBs on draft day and how they ultimately perform in the league (Hendricks et al. 2003; Berri and Simmons 2011). Despite being a moneyed process with high stakes for team success and profit, the draft market is demonstrably inefficient, especially regarding quarterbacks. The performance of quarterbacks after the draft is well outside the scope of this paper, but just because the behavior of the market is inefficient, that does not mean it is random, and I will show in this paper that it is still quite feasible to model draft probability. My analysis is not about whether college football is a true meritocracy; it is just about how players experience and exercise leverage as they try to attain their desired outcome.

come, and they compound to create a theoretically clean decision complex where the choices of each actor correspond to a well-bounded set of counterfactual outcomes. First, each team only has one starting quarterback, which results in quarterback recruits receiving a relatively smaller number of scholarship offers compared to other positions, and puts the quarterbacks in much more direct statistical competition (across schools) with one another than are players at other positions. If a wide receiver or safety, playing in a position group with multiple members on the field at the same time, commits to one school or another, his football role might be totally different, but the QB is the QB, wherever he might be. Second, quarterback is a high-status, strategically critical, cerebral position, the focal point of the offense, the priority target for the defense, and popularly called “the most important position in sports” (Feinstein 2018). These players, more than any others in the sport, are constantly evaluated and compared to one another by scouts, coaches, fans, and analysts, so the data on them is of particularly high quality. Third, because of the importance of the quarterback position to the game of football and the increased status it entails, it is subject to well-understood structural pressures I can model, notably racialization. Between 1960 and 2020, the NFL has evolved from a mostly white league to one dominated by Black players at most positions, but a racial hierarchy where white quarterbacks are favored over Black quarterbacks persists. This manifests in several ways, including Black QBs being benched more often (Volz 2015) and discriminated against in media coverage. Despite much discussion about the so-called “rise of the Black quarterback” (Lindsay 1973; Early 2011), quarterbacks at the college and professional levels remain majority white (Marquez-Velarde et al. 2023), though QB is no longer exactly the marquee position reserved for a white player it once was.

It can be argued that this disparity persists even as the NFL in general has become overwhelmingly Black because the game of football revolves around protecting the quarterback. From early childhood, white players are funneled into the more cerebral quarterback position while Black players are encouraged to play more physically demanding, injury-prone positions like wide receiver, running back, and defensive back. Even those Black players who overcome this Fanonian racial schema to succeed at the quarterback position are often evaluated based on physical characteristics, as opposed to white quarterbacks, who are usually given more holistic evaluations focusing more heavily on their mental traits (Bigler and Jeffries 2008). This is an

excellent example of the sort of structural factor we have good reason to believe might have a schematic impact on leverage, so I account for it in the analysis.

Player-Side Data

I collected data on the top 50 quarterback recruits out of American high schools each year from 2011 to 2014, a population comprising 200 total individuals. They represent a very specific group of people: before 2011, recruiting data was unreliable and inconsistent, and after 2017 (when the last player in my population began his last college season), players' access to the transfer portal (see the later section on transfers) upended the properties, costs and returns of the college commitment decision. The players I look at occupy an analytical sweet spot: there are enough of them to look for patterns, but not so many that doing so erases their individuality, the data on them is highly reliable, they are all competing on basically the same terms, and they are insulated from the seismic shifts in college sports that would distract from this specific analysis.

The player-side data I collected on these 200 recruits includes several key variables. The players' recruit class (2011 to 2014), race, home state, recruit rating (3 to 5 stars), which schools gave them scholarship offers, and where they initially committed were collected from 247Sports.com, one of the three major high school sports scouting firms along with ESPN and Rivals.com. I collected data on the players' statistical performance in college football games, and what schools they transferred to if they transferred, from sports-reference.com. This data includes, for the players' final full season in college or the closest to it, passing yards, rushing/receiving yards (coded as 0 if negative due to sacks)³, total touchdowns, interceptions, and completion percentage. It may be that players are evaluated by NFL teams based on seasons prior to their final college year, but using the final full season puts the players on an even statistical playing field for comparison and represents the most recent and likely the most relevant stats in general for the draft. I collected data on whether, and at what position, players were drafted into the NFL from DraftHistory.com. I collected supplemental data from ESPN and other sources (see Appendix A). Some descriptive statistics on this population of players follow (Tables 1 and 2), with associated conditional probabilities for draft outcomes:

³ NFL fans may raise their hands here and remind the author that sacks count as negative passing yards for the team, not for the quarterback. However, that is only true in the NFL. In college, sacks count as negative rushing yards for the quarterback.

Table 1: Recruit Class by Recruit Rating

Recruit rating/ High school class	Class of 2011	Class of 2012	Class of 2013	Class of 2014	Total
5 star	3	2	2	1	8
4 star	12	24	33	25	94
3 star	35	24	15	24	98
Total	50	50	50	50	200

Table 2: NFL Outcome by Recruit Rating

Recruit rating/ NFL outcome	Drafted by NFL Team	Undrafted free agent (UFA)	No NFL career	Total
5 star	4 (50%)	1 (12.5%)	3 (37.5%)	8
4 star	17 (18.1%)	3 (3.2%)	74 (78.7%)	94
3 star	12 (12.2%)	10 (10.2%)	78 (77.6%)	98
Total	33 (16.5%)	14 (7%)	155 (75.5%)	200

I use the “consensus” recruit ratings, basically an unweighted average of the ratings assigned to these players by the three major scouting agencies. As I discussed earlier, these ratings are highly subjective and the difference between a 3-star and a blue chip recruit may come down to individual scouts’ opinions or how a player looked at practice one day versus another. Using the consensus ratings does not fix these issues, but it does help make this variable less dependent on the whims of particular scouts. 5-star quarterbacks, the most prestigious and sought-after players at one of the highest-status positions in sports, are rare, but small sample size notwithstanding, the conditional probabilities tell us that they seem to enjoy better NFL outcomes than their 4- and 3-star counterparts. Across my four-year population, more 4 stars than 3 stars are eventually drafted, but more 3 stars than 4 stars are signed to NFL teams as undrafted free agents (UFAs), which bears considerably fewer social rewards and a smaller rookie contract than does being drafted outright, but is still an NFL contract and should still be considered an NFL outcome.

I use the top 50 quarterbacks, instead of including all recruits regardless of national rank, for methodological and theoretical reasons. While it is true that there are over 100 FBS schools capable of extending scholarships to quarterbacks, not every school needs a QB every year, and after about the top 50 high school quarterbacks a year during my time period, the offers from power conference programs dry up rapidly. Most players below the top 50 receive only one or two FBS offers, if that. It is of course still prestigious to play quarterback at the collegiate level at all, but the vast majority of players in the 50-100 range never end up playing against FBS competition. Some still end up drafted, but they are not really participating in the same process as the top 50 QBs a year, almost all of whom are being actively sought by multiple FBS programs. QBs below the top 50 largely lack this discretion. The choice is made for them.

Methodologically, extending the list through all recruited QBs would introduce missingness issues (since most QBs below the top 50 will never take a meaningful snap of FBS football) without extending the dataset to nearly anyone with meaningful leverage. The quarterbacks outside the top 50 also do not occupy exactly the same elite cultural *milieu* as their sought-after counterparts. An empirical study of all quarterback recruits *qua* quarterback recruits would consider these individuals, but this is a study of top 50 quarterback recruits *qua* life choice makers with uncertain and heterogeneous leverage.

School-Side Data

Division 1 college football programs are divided into two categories, the prestigious FBS (Football Bowl Subdivision, teams eligible to play in postseason bowl games like the Rose Bowl) level and the less prestigious FCS (Football Championship Subdivision, less-resourced football programs that play in a postseason tournament) level. Both grant athletic scholarships, but the resources and media attention available for players at FBS schools dwarf those available for those at FCS schools. 130 programs existed at the FBS level during my time period. Of these schools, by 2017 (when the last player from my dataset began his final college season), almost half (64) belonged to so-called “power conferences,” whose members are generally considered to be in the upper echelon of the FBS. Power conferences, a category that in 2017 included the SEC, the Big Ten, the Big 12, the ACC, and the Pac-12, have the longest football histories, the largest fan bases, and the most media attention. Most players who are drafted come from power conference member institutions.⁴ I collected data on the 130 FBS schools during the 2011-2017 period, a timeframe covering when the players in my population were actively competing in college football. The states these universities are located in and the conferences to which they belonged in 2017 are matters of public record, and I collected information on what offensive scheme each college team utilized during this period from a variety of sources (see Appendix A), along with details about the schemes themselves. It is plausible that certain kinds of offense make a quarterback look more draftable to NFL teams, and it is likely that in-game statistics are evaluated differently for players in different offenses. Coding this variable is necessary for a plausible counterfactual analysis, not only because of its direct impact on player performance but also because of its effects on how that performance is evaluated for the draft.

The most important school-side variable I need to account for, other than power conference status, is schedule-adjusted team quality. Because college football teams have asymmetrical access to resources, there is a lack of competitive balance between them. Even at the FBS level, the best teams in the best conferences are understood to be far better (at football) than the worst teams. Wins against stronger competition are weighted over wins against subpar

⁴ One school, Notre Dame, does not belong to a conference but is considered by the college football world to compete on the level of a strong power conference member, with a long history of being an “independent” program. Notre Dame’s football team enjoys media attention, recruiting success, and draft outcomes akin to those of the best power conference schools.

opponents, and losses against good teams are not as damaging to a team's reputation as are losses against teams viewed as underdogs. Any metric for team quality I use must account for this competitive asymmetry. I use "Dr. Colley's Bias Free Matrix Ranking," otherwise known as the "Colley Matrix," a special case of the generalized row sum method for scoring elements based on internal competition designed by Wesley Colley (Colley 2002). This algorithm is designed to provide an objective score of how good every college football team was in a given year, and it is considered a "major selector" by the NCAA, meaning that it is considered a legitimate means of determining a national champion. It was used to schedule bowl games, the college football postseason, for a number of years. Colley (2002) lists the advantages of this ranking system compared to qualitative media polls and "byzantine black box" computer models: "[it] has no bias toward conference, tradition, history, etc., (and, hence, has no pre-season poll); [it] is reproducible; [it] uses a minimum of assumptions; [it] uses no ad hoc adjustments; [it] nonetheless adjusts for strength of schedule; [it] ignores runaway scores; [it] produces common sense results." I collect the end-of-season Colley Matrix ratings for each team from the 2011 season to the 2017 season, and take an unweighted average of these ratings for each team to compute what I call the "Colley average," the average evaluation of each college for the 7-year period of my analysis, which is more stable to breakout or unusually poor performances that a school has for only one season. This number ranges from 0 to 1, with a 0 score representing a team that never wins a game against the worst competition available and a 1 score representing a juggernaut expected to defeat even the strongest opposition. There is a specific Colley rating for each FBS team each season, and for schools that were in the FCS at some point in the 2011-2017 period, I use the "FCS group 1" Colley rating for the relevant seasons.

Model Specification

I specify a logistic regression model for draft probability based on various player-side and school-side covariates (denoted by the subscript p and s respectively):

$$D = R_p + \sigma_p + Ypass_p + Yrush_p + TD_p + Int_p + C_s + Power_s + Scheme_s \quad (1)$$

where D represents the probability of being drafted, R_p is player race, σ_p is recruit rating in stars, $Ypass_p$ is passing yards, $Yrush_p$ rushing and receiving yards, TD_p total touchdowns, Int_p interceptions, C_s Colley average, $Power_s$ power conference status, and $Scheme_s$ offensive

scheme. I operationalize race with a white/Black/other categorization, since 197 of the 200 players are either white or Black. I use “other” as the reference category for race. The reference category for recruit rating is 3 star status, and the reference category for scheme is air raid. The reference category for power status is the lack of that status.

I pare down the full population of 200 quarterback recruits to isolate those of them who have valid data for this model specification. Of these players, 196 went on to play college football, with the other 4 being multi-sport athletes who chose baseball over football (all 4 of whom ended up with major league careers). Of these 196 quarterbacks, 20 made a move to a different position at some point, making them statistically incomparable to the rest of the population. Of the 176 remaining quarterbacks, 41 did not put up any passing yards at any point in college at an FBS program, which indicates that their college football careers did not pan out. Some quarterbacks are pure “pocket passers” who tallied zero rushing or receiving yards in college, but recording zero *passing* yards is tantamount to not being a quarterback in a meaningful sense, and I exclude these players from the model.⁵

After this process, I am left with 135 college quarterbacks with valid data. Whether a unit has invalid data is not random in this case, but selective of several features that are relevant to this study design. Whether a player ends up at another sport or another position over playing football as a quarterback is not entirely up to them; they are constrained by the willingness of their coaches to accommodate these choices, and indeed it is often the coaches who are making choices like these for them (Logan et al. 2015). The majority of invalid quarterbacks — 41 people, just over one fifth of the original population — did not give up on football, but they did not achieve the goal of playing in a college football game in the FBS. This could be for a variety of reasons. Perhaps they lacked the talent or physical gifts required to play quarterback at the college level, or perhaps they were outshone and outcompeted by another QB at their school, or perhaps they had off-field issues that prevented them from playing, or perhaps they were injured, and so on. My study design does not give me the ability to speculate on what if any of these things might have been different had players attended other schools — although in many cases there surely would have been such differences — but generally speaking, if someone never

⁵ One player, Chad Kanoff, transferred from Vanderbilt to Princeton. The Ivy League schools are all FCS football programs and thus included in the Colley Matrix, but they play fewer games in a normal season than other Division 1 programs and Kanoff’s stats are not directly comparable to those of his peers, so I exclude him from the model. Kanoff ultimately signed as an undrafted free agent with the Arizona Cardinals.

played in a college football game at the FBS level, there is no plausible counterfactual based simply on choice of school that would see them getting playing time elsewhere. Much more fundamental structural, agentic, and stochastic factors would have to be different for it to make sense to include these players in the model. In that sense, there is a consistent pattern with the players who *do* end up included in my model: they are (at least) competent, dedicated players whose coaches have some level of faith in them, and who got their chance, at least relatively speaking, at the dream of parlaying a college career into an NFL contract.

Transfers

The largest methodological difficulty when working with this data is determining how to deal with players who transferred between colleges during the course of their college career. In 2018, the NCAA introduced the so-called “transfer portal,” a new system for managing college athletes’ transfer decisions, which along with the loosening of regulations on transferring for college athletes contributed to an enormous increase in the number of college athletes who transfer each year, especially in revenue-generating sports like football, women’s volleyball, and men’s and women’s basketball. My data concerns only players whose entire college careers predated the portal, when transfer agency was much more tightly restricted (Logan et al. 2015). However, a large proportion of players in my dataset still transferred, including 58 of the 135 quarterbacks with valid data for inference. I handle this simply by treating the school where the player ended up as though the player committed there originally, and I treat the school where they were originally as a counterfactual.

This is not a perfect solution for a few reasons. First, if a player “transfers down” from a big-name program to a less prestigious one, draft analysts may still associate the player with the big-name school, which will be hidden to the model. Second, if a player does “transfer down,” it likely indicates that they would not have earned the starting quarterback job at their original school, making that counterfactual less plausible. Of the 58 players who transferred, 30 transferred to a school that had extended them a scholarship offer out of high school anyway, essentially getting a do-over of their commitment decision and choosing differently. Questions of who gets a do-over in this way and who doesn’t, and how the transfer portal and direct payments to college athletes have revolutionized this system, are fascinating and worthy of future research, but they are outside the scope of this paper.

Despite these concerns, I have several reasons for handling transfers in the way I do. First, the transfer decision is an important part of player agency in college football, and it is part of a broader decision complex that includes the player's original commitment decision. Players may well commit to one program knowing that they intend to transfer somewhere else, even before the era of the transfer portal. Second, because the proportion of transfer players who transferred to a school that originally offered them is so high, it indicates that most top 50 high school quarterback recruits who transfer look to schools that originally offered them first, meaning the transfer decision is colored by the commitment decision such that one cannot be extracted from the other. Third, the aim of this study is not to determine why a player chooses one school over another; it is to determine how much impact on the desired outcome of being drafted into the NFL a player's choice has. I cannot opine on whether a player is "transferring down" or otherwise, or why they would transfer versus stick around. What I can examine is the effect of college choice on NFL draft probability, and handling transfers in this way is a plausible, coherent means of measuring that effect.

I perform a robustness check (see Appendix B), showing that using players' original school instead of their final school for transfers somewhat degrades model performance and may mask some of the impact of attending a power conference school.

Endogeneity

The other main methodological issue with this data is that a player's statistical production is not only a player-side variable; as anyone who has ever criticized a "system QB" knows, school-side effects like scheme, teammates, and coaching heavily influence it. The counterfactuals I entertain are not "what would have happened, causally, if player A had attended school B," but "how would draft odds differ if player A's stats had been produced at school B instead." My calculation of leverage may understate it, because choice of school entails variation in statistics, particularly for the "middle class" of players who in the factual case produced good but non-elite stats with a diverse set of scholarship offers. The overall pattern I will demonstrate in the analysis, that leverage has a concave relationship with status that is greatest for the "forgotten middle" (Schoon and Lyons-Amos 2016), is perhaps more pronounced than I show in the results section. The exception might be an underdog player for whom a different choice of school would have produced outlier statistics, a concern I address in Appendix B.

ANALYSIS

Logistic Regression Model for Draft Probability

I specify a logistic regression model based on the parameters laid out in the preceding section, and train it on the factual outcomes for the 135 valid quarterbacks only. UFAs are considered not drafted. The covariate table follows:

Table 3: Primary Logistic Regression Model Covariate Matrix

	Estimate (log odds)	Standard Error	<i>p</i> -value
(Intercept)	-15.978390	6.772880	0.0183 *
Race (Black)	1.275651	5.041695	0.8003
Race (White)	1.841427	5.039918	0.7148
4-Star Player	0.392310	0.935174	0.6748
5-Star Player	2.590796	1.583069	0.1017 ~
Season Passing Yards	0.002655	0.001205	0.0276 *
Season Other Yards	0.001820	0.002280	0.4247
Season Total TDs	0.023696	0.105577	0.8224
Season Total INTs	-0.172233	0.142201	0.2258
Colley Average	5.132354	3.365243	0.1272 ~
Power Conference	4.111060	2.067633	0.0468 *
Scheme (Option)	-12.208796	1747.919088	0.9944
Scheme (Pro-Style)	-0.674781	2.872129	0.8143
Scheme (Spread)	-1.965336	2.838629	0.4887
Scheme (Multiple)	-0.248600	2.874212	0.9311

p -values marked with * are significant at the $<.05$ level, and those marked with ~ are not considered significant but are low enough to merit consideration.

Null deviance: 147.863 on 134 degrees of freedom

Residual deviance: 51.655 on 120 degrees of freedom

AIC: 81.655

The purpose of this model is to be used to predict counterfactual draft odds for the purpose of estimating leverage, not to explain what causes quarterbacks to be drafted or not, so I will not linger on reporting the coefficients here, but I will touch on them to establish that the directions and scales of the effects we observe are plausible given our expectations for the structural and agentic factors that influence draft odds. There are statistically significant effects at the 95% level of confidence for passing yards, the primary quarterback performance metric, and power status, the primary indicator of program prestige. The scales of these variables are very different, but it is clear that passing for more yards in a player's final season and coming from a power conference school (or Notre Dame) are associated with statistically significant positive effects on a player's draft odds (as we would expect). In fact, power conference status increases the odds of being drafted by about a factor of 4, nearly as much as would going to a school with a Colley average of 1 (the maximum possible score, the best program in the country in a given year) over a school with a Colley average of 0 (the minimum possible score, the worst program in the country in a given year).

As for race, white quarterbacks are slightly advantaged over Black quarterbacks according to the model, but these effects are not statistically significant. Although "other" status seems to be a negative for draft odds, controlling for the other covariates, the number of non-white, non-Black quarterbacks is so small that this effect size is unreliable. This says something about the racial makeup of the quarterback position on its own. Once Black quarterbacks have overcome the forces pushing them to move to other positions, the evidence that they are seriously unfavored compared to white QBs in the draft is not strong, but the practical nonexistence of "other" quarterbacks supports findings in the literature about how Black quarterbacks are stereotyped as their own category, albeit one that is flourishing in the modern NFL, but non-white, non-Black quarterbacks are hard to imagine (Niven 2005; Bigler and Jeffries 2008; Early 2011). Sewell (1992) proposes "schemata" as the "virtual" component of

structure (as opposed to the “material” component, “resources”), an expansion of Giddens’s framing of “rules and resources.” This can help us understand the role of race at the quarterback position: an emerging schema of the black quarterback has risen as a culturally legible competitor for the white quarterback, but players who fit neither role find themselves at a disadvantage. There is no “rule” against a non-white, non-Black quarterback (Marcus Mariota, who won the Heisman Trophy and was drafted second overall, is part of my dataset and belongs to this category), but there are racial schemata in place that establish expectations for what an NFL-ready white or Black quarterback looks like that racial “others” who match neither archetype cannot embody.

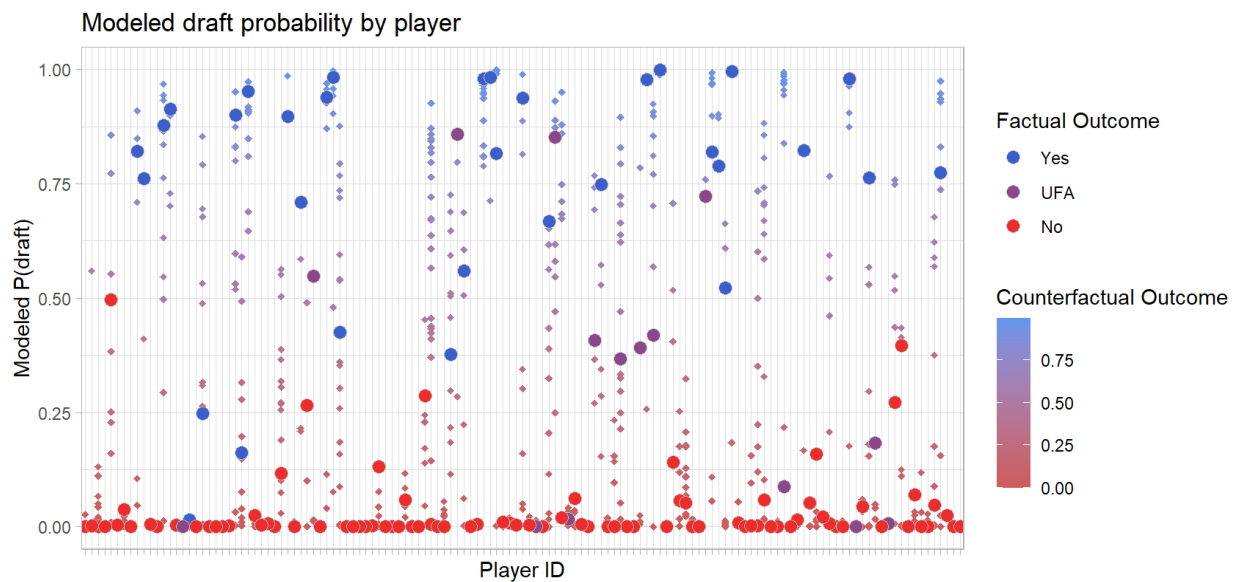
Rushing/receiving yards and touchdowns have positive effects on draft odds, but these effects are not considered statistically significant. Interceptions, predictably, have a negative effect, but it is also nonsignificant. Colley average has a positive effect on draft odds, which is not quite significant at the 90% level of confidence but close to it. Recruit rating is also interesting: 4-star status does not bear a large effect on draft odds relative to 3-star status, but 5-star status does (though just shy of statistical significance at 90% confidence), reflecting the similar pattern visible in the descriptive statistics. Controlling for the other independent variables, 5-star status seems to increase the odds of being drafted, suggesting that being so highly rated out of high school is a lingering source of status for NFL-hopeful quarterbacks years later (NFL teams might think that the scouts identified some characteristic about a recruit that might be useful in the league if they take a chance on him). Bourdieu would not be surprised that status-advantaged players have a competitive advantage in the draft over their lower-status peers, even controlling for their actual on-field performance and the quality of the programs they play for (Bourdieu 1972, 1984; Harvey et al. 2020). As to the career decision literature, this is reminiscent of the King et al. (2005) finding that prior history with recruiting organizations matters more for career prospects among IT professionals than do their actual qualifications. The courses of their careers are bounded by structural factors.

I test the model for predictive performance using k-fold cross-validation with 5 groups and ridge regression (L2 regularization). The model performs quite well under both tests (see Appendix B). Having established the plausibility of the model for establishing counterfactuals, I can proceed to the heart of this analysis: estimating and graphing those counterfactuals to find and visualize the measures of individual-level variability that represent leverage in this case.

Draft Probability Analysis and Plots

I match players to the schools that gave them scholarship offers (or where they transferred), then use the model to estimate a draft probability for each valid player-school combination, a total of 1224 combinations across the 135 players (~9.07 average plausible schools per player). I do not report confidence intervals around these points because the important thing for analysis is not the specific predictions, but the variation between the point estimates. I plot these modeled draft probabilities for both the factual and counterfactual cases below, broken down by recruiting class.

Figure 1: Modeled draft probabilities by player. The factual outcomes are represented by the larger circles, colored according to the outcome in the factual case (blue: drafted, purple: undrafted free agent, red: no NFL career), and placed on the y-axis according to the model's predicted probability of being drafted for the player at their factual case school. The counterfactual probabilities are represented by the smaller diamonds, and colored according to the model's predicted probability of being drafted for the player at each alternative school they could have attended. The closer the predicted probability is to 1, the bluer the diamond, and the closer it is to 0, the redder the diamond.

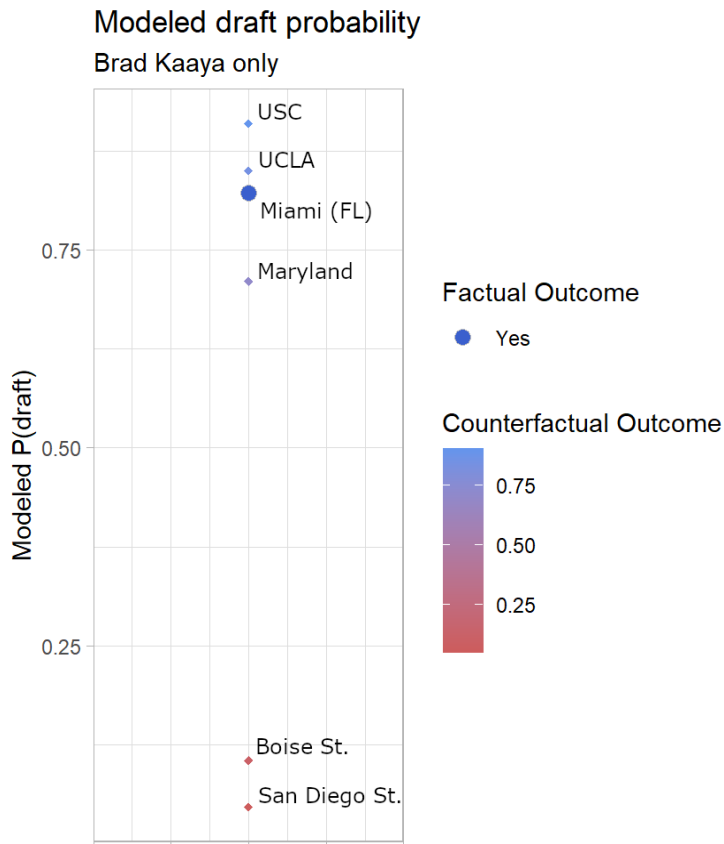


Modeled draft probabilities are distributed heterogeneously across the players in the sample. Most players, represented by the sedimentary layer of red circles across the bottom of

the plots, have low enough in-game stats that the model predicts they will likely never be drafted, regardless of what school they choose. These players, those whose choices do not make a significant difference to their NFL outcomes, fall into two categories: those who received few FBS offers, were not regarded as elite recruits, and did not end up impressing in college, and those players who were regarded highly enough in high school to get many offers, but who did not perform well enough in college to get drafted, no matter where they went. Some players have average or above-average statistical performance and a high diversity of scholarship offers, leading to very large ranges of modeled draft probabilities across the schools they could plausibly have attended. This reflects a high level of leverage, a “forgotten middle” of difference makers (Schoon and Lyons-Amos 2016). Some players with average to above-average performance have a lower diversity of options, which results in moderate leverage. Their likelihood of being drafted depends mostly upon which offers they happen to get, but they may still experience wide variation in outcomes depending upon what choices they make among those offers. A few players have exceptional or very good in-game stats coupled with offers from just a few solid programs, perhaps due to being overlooked in high school. These players — who are certainly elite — have *lower* leverage; that is, the only choices before them eventually lead to a high probability of being drafted into the NFL. Some quarterbacks are highly touted out of high school, receive many scholarship offers, and make good on their potential in college. These players may have plausible “bad” options, but experience lower leverage than the difference makers do: should they make the “wrong” choice, the system is much likelier to bail them out because of elements like their race, five-star status, or on-field performance. In this way, they are free to choose whatever course of action they want because they are unlikely to entail meaningful capital penalties for doing so. Consequently, the commitment decision is significantly less important in terms of the draft outcome for them than it is for the forgotten middle, who are “on the bubble” in terms of talent and status and have much more to lose by committing to a lower-profile program that might struggle to get them drafted.

To help understand the apparent phenomena here, we can take a closer look at a few players as examples. First, consider the case of Brad Kaaya, who attended Miami (Florida) but also received scholarship offers from USC, UCLA, Maryland, Boise State, and San Diego State. Brad represents the archetypal high-leverage player: diverse scholarship offers, good but non-elite in-game performance, and reasonably typical contextual characteristics.

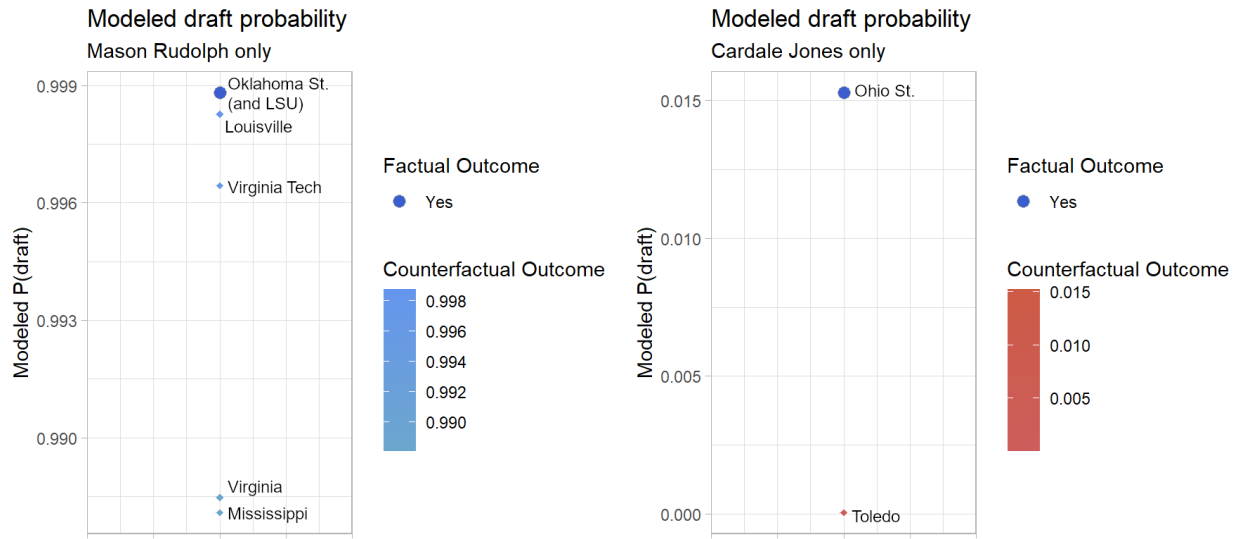
Figure 2: Modeled draft probabilities by player, Brad Kaaya only.



The factual outcome is represented by the large circle, which is colored blue because Brad Kaaya was indeed drafted in the factual case, an outcome with a modeled probability of ~0.822. Brad had two scholarship offers the model believes were even likelier to get him drafted, USC (~0.910) and UCLA (~0.849). At Maryland, the model gives Brad a draft probability of ~0.710. Brad's non-power scholarship options, Boise State and San Diego State, are associated with much lower modeled draft probabilities of ~0.106 and ~0.047 respectively.

Next, I want to compare the pathways of two quarterbacks, Cardale Jones (high school class of 2011) and Mason Rudolph (high school class of 2014), both of whom were drafted, and both of whom had low leverage, but whose journeys to the NFL could not have been more different. We see here examples of archetypal elite and deprived players: neither Mason nor Cardale experienced much leverage over their NFL draft outcome based on their college choice, but this was for very different reasons. Mason's plentiful high-profile offers and elite in-game stats virtually guaranteed him a draft selection, whereas Cardale's few offers and career as a backup meant that his only viable path to the draft flowed through a completely different opportunity structure and required a lot of luck.

Figure 3: Modeled draft probabilities by player, Mason Rudolph and Cardale Jones.



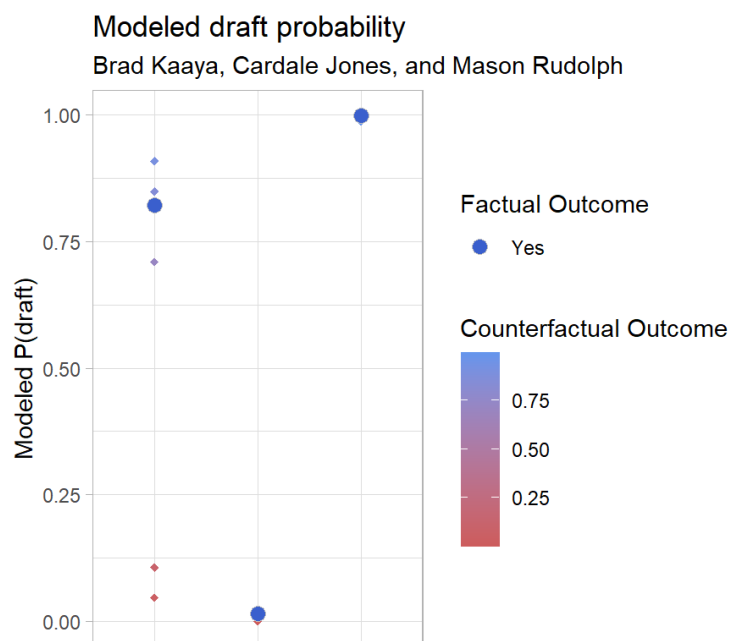
Mason Rudolph is a classic example of an elite prospect who delivered in college. Like Brad Kaaya, he received six publicly reported scholarship offers, but all from power conference schools with moderately-to-very large investments in football (Oklahoma State, LSU, Louisville, Virginia Tech, UVA, and Mississippi). These schools were likely attracted to his stellar, blue chip recruiting profile. Mason chose Oklahoma State and went on to become their winningest quarterback in school history, putting up excellent stats and earning a day 2 draft selection. He is the definition of a hyper-agent, but that means that there was no choice of school he could have made that would have significantly worsened his odds of being drafted. His “worst” option, Mississippi (“Ole Miss”), gives him a modeled draft probability of ~0.988.

Cardale Jones was a 3-star recruit from Toledo, Ohio who received offers from his hometown university, Toledo, a non-power conference FBS school with a solid football program but a modeled draft probability of below 0.001 given Cardale’s final season stats, and Ohio State, one of the premiere football schools in the country, which expected him to be their backup. He signed with Ohio State, where he indeed played as a career backup, but the paltry stats he put up (because, being the backup, he rarely came into games) should not have given him anywhere near the national profile needed to be drafted (a modeled probability of ~0.015). When Cardale *did* come in, however, he became a legend, famously winning the National Championship as a backup in 2014. Cardale had no significant ability to influence his draft odds through his college commitment decision in the traditional sense, but when something unexpected happened — a

sudden injury to the starter ahead of him — he outperformed the expectations of the national football media (and my model). Being at Ohio State gave him a uniquely strong position to make a name for himself *as a backup*, likely a much better one than being an average starter at Toledo would have. Cardale had no way to guarantee that he got his shot. It could well have happened that the starter never got injured, Cardale never came in, and he never led Ohio State to the National Championship. But when the stochasticity of college football gave him a chance, he delivered, and he built enough of a reputation that he was drafted in the 5th round. This is on the one hand an analytical limitation of my way of calculating leverage: as a counterfactual analysis, it cannot predict these unexpected outcomes. But this is part of why looking at individual-level stratification is valuable. Through sensitive analysis of individual actors' choices, we can appreciate our outliers on a case-by-case basis (and there really is nobody else in the population with a story quite like Cardale's). The model, despite its otherwise excellent performance, doesn't predict Cardale's outcome well because he was able to bypass the usual opportunity structure, and because we know who he is, we know how he did it.

Now, let us look at our three cases together to anticipate the relationship between leverage and status that will emerge from the rest of this analysis.

Figure 4: Modeled draft probabilities by player, Brad, Cardale, and Mason.



There is an emerging, concave relationship between leverage, status and performance visible when we graph these three cases. We can see that high-status, high-performing Mason (right), and lower-status, situational performer Cardale (middle) have virtually no variation in their predicted draft odds based on school choice relative to moderate-status, moderately high-performing Brad (left).

The general trends I talked about earlier in my analysis hold up for these three players: Mason was elite, only had good options, took one of them, and got drafted. Brad was good but not elite, had some options that were better than others, took a good one, and got drafted. Cardale was not a starter, had just two options, and took one that the model predicts shouldn't have gotten him drafted. He got a lucky chance, rose to the stochastic occasion, and got drafted. Even with a clearly bounded case like this, players like Cardale can still defy the expectations of a very predictively accurate model. A group-level approach to stratification might aggregate and compare the characteristics of the players who were drafted to those who weren't, and conclude that certain characteristics (for example, five-star status) make some players on average likelier to be drafted than others. But it wouldn't be able to examine the differences between these three drafted players, who were ultimately drafted for very different reasons: elite status and performance (Mason), a good tactical choice in a high-leverage situation (Brad), and a combination of good luck and a legendary clutch factor (Cardale). In terms of the NFL draft outcome, I argue that Brad's choice mattered, Mason's largely did not, and Cardale's may have, but through the interplay of serendipity and structure rather than by altering what his draft odds would have been under ordinary conditions. Because he was ultimately *not* subject to ordinary conditions, he experienced an extraordinary outcome.

Leverage Analysis and Plots

To measure leverage over the NFL draft outcome for my population of interest, I compute the interquartile range (IQR) of the modeled draft probabilities for each player. I use IQR instead of another measure of variability like range or standard deviation so as to be more robust to outliers, which are visibly not uncommon in my data (Figure 1). These outliers may represent the non-power schools that occasionally extend scholarship offers to highly touted recruits otherwise deciding between prestigious power conference programs, who are very unlikely to choose the non-power programs, or they may correspond to the power conference schools who occasionally take a shot on a recruit who is otherwise flying under the radar.

Below, I plot the IQR for each player against three metrics representing more canonical dimensions of agency. First, I visualize the relationship between this measure of leverage and the raw number of scholarship offers received, which is a cruder but more straightforward dimension of agency than leverage is. This raw count is highest for five stars and the other most pursued

recruits, meeting the Bourdieusian expectation that the elites should have the most agency in the sense that they have the most *options*; however, they do not necessarily have the most *leverage*. Like Mason Rudolph, many of these hyper-agents had a large number of scholarship offers, but nothing they could have chosen would have made them significantly less likely to be drafted. Next, I plot leverage against the Colley average of the school each player attended in the factual case, the simplest representation of college football program status. Then, I plot it against passing yards, the most statistically significant continuous metric for player-level performance. These plots include non-parametric smoothing lines, calculated with the Locally Estimated Scatterplot Smoothing (LOESS) method, to visualize the trends I will be discussing regarding these relationships. In Appendix B, I include similar plots for leverage calculated with standard deviation instead of IQR. I also expand on my reasoning for using IQR and discuss in what contexts other measures of variability might be more appropriate.

Figure 5: Leverage (IQR) plotted against the raw number of scholarship offers each player had.

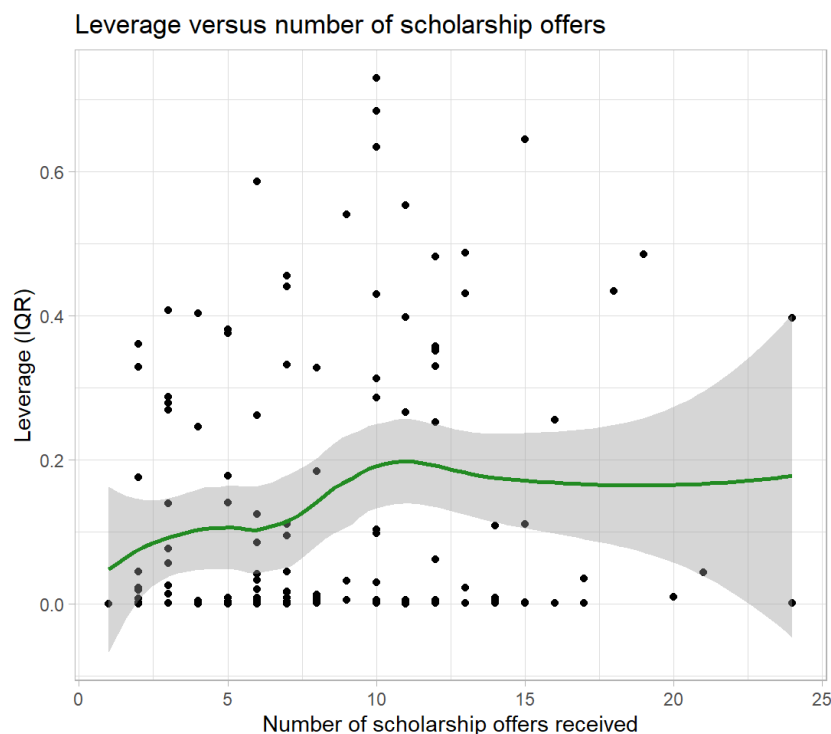


Figure 6: Leverage (IQR) plotted against the Colley average of each player's factual school.

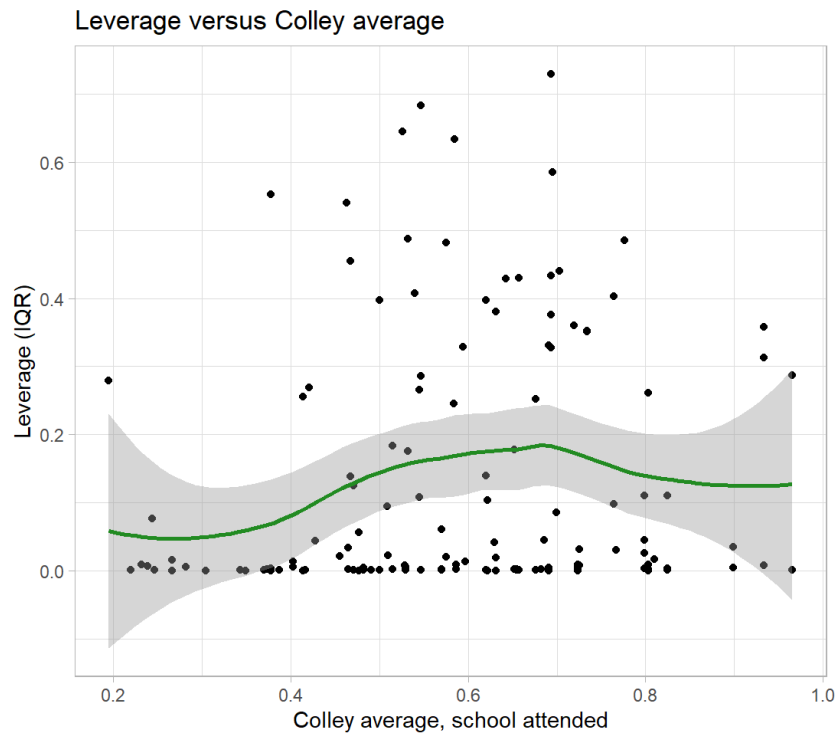
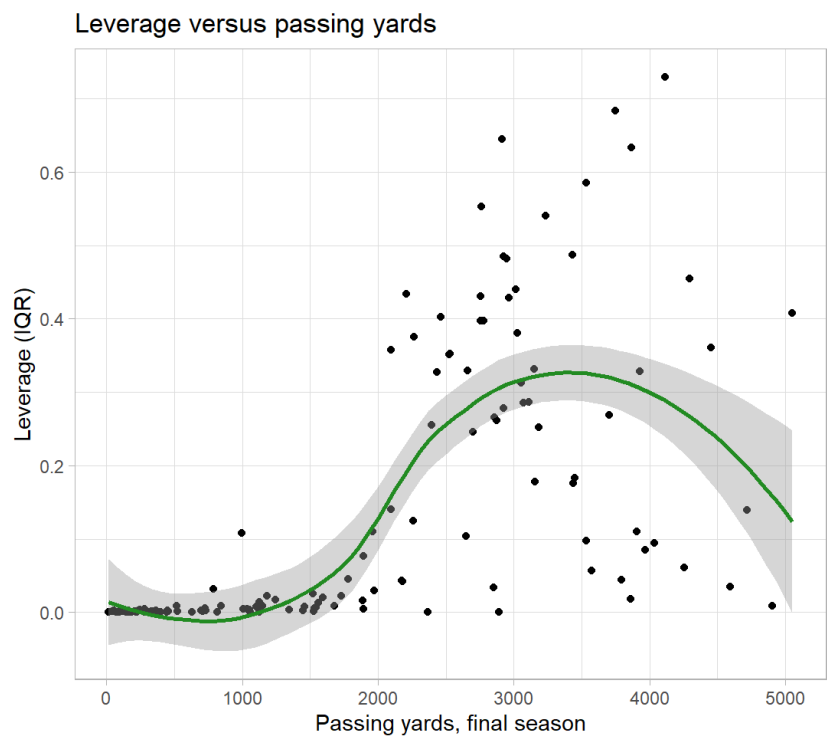


Figure 7: Leverage (IQR) plotted against each player's final season passing yards.

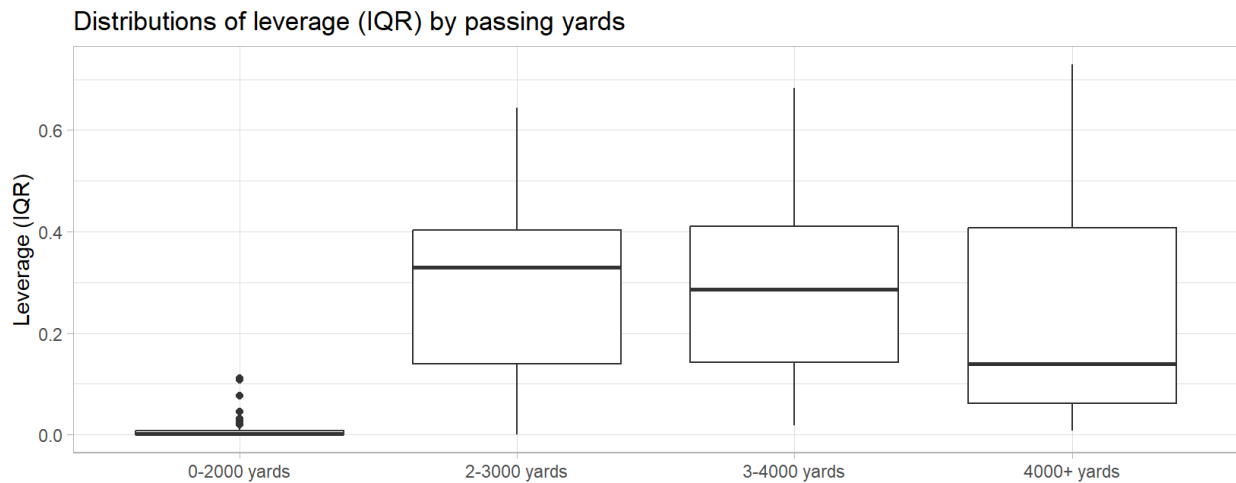


These results visualize the concave relationship between leverage and status suggested by the comparison between Mason, Brad, and Cardale. The highest status hyper-agents seem to have less leverage than the “forgotten middle,” meaning they are free to choose their life path (at least, choose where to go to college) without risking serious capital penalties. There may be a loose positive association between leverage and the number of scholarship offers (Fig. 5), but there is no clear linear relationship, and certainly not after 10 offers, which is about the average number. Receiving more offers — that is, having more options — does not necessarily mean a player has more leverage, once a certain number of options are available. It is not the number of options, but the diversity of options available to an actor that drives leverage over draft probability in this case: if a bunch of similar schools give a quarterback scholarship offers, like we see in the case of Mason Rudolph (Figs. 3 and 4), choosing between them has very little impact on whether he ends up drafted. A player needs offers from schools that are different in terms of their draft-related characteristics for his choice to significantly affect that outcome.

The findings regarding Colley average (Fig. 6) also suggest a clear pattern where the middle class of players have the most leverage. Players who in the factual case attended schools with about average Colley ratings having the highest leverage when measured in this way. As for passing yards (Fig. 7), the same relationship emerges. Below about 2000 passing yards, players all have low leverage because their statistical performance is not good enough for them to be considered draftable: they are deprived, no matter what school they choose. The 2000-yard cutoff is in fact quite stark. It seems that NFL teams are uncomfortable drafting players whose passing yard total starts with “1,” but once that largely arbitrary bar is cleared, a player becomes draftable. Above the 2000-yard margin, leverage rapidly increases, but the best performing players with over about 4000 passing yards do not display more leverage than their solid but not exceptionally well performing counterparts with between 2000 and 4000 passing yards. The median leverage for the 9 players with at least 4000 passing yards is ~ 0.139 , and the median leverage for the 29 players with between 2000 and 3000 passing yards is considerably larger, ~ 0.329 . For the 23 players between 3000 and 4000 passing yards, the median leverage is ~ 0.286 . The 74 players with below 2000 passing yards are, on the whole, undraftable, and a median leverage of ~ 0.002 , essentially zero, reflects that (Fig. 8). Of course, we know a player who was drafted despite falling into that bin: Cardale Jones (Figs. 3 and 4), who found an unexpected path

to being drafted. Because the leverage he experienced through his choice between colleges was so low, we might surmise that his decision to go to Ohio State instead of Toledo had a Mertonian (1936) *unexpected* impact on his draft odds, not a Parsonian (1937) *expected* impact.

Figure 8: Box plots displaying leverage (IQR) for players, grouped by passing yards.



DISCUSSION

Leverage is just as much a measure of risk as it is a dimension of agency. When a recruited high school quarterback considers the proverbial shoebox of scholarship offers before him, he imagines what might be downstream along the flow of social life from each option. This is a manifestation of reflexivity (Giddens 1984, Archer 2003): the player predicts how his life will be different in the future depending upon what he chooses, and by choosing he tries to enact the version of his life course he prefers. The starkness of this opportunity structure makes college football a particularly rich *milieu* for the study of leverage and agency. There are Bourdieusian hyper-agents likely to win the status competition no matter how they act, a deprived class barred from the reward without stochastic intervention, and a middle class of actors who are technically free to do as they wish, but strongly incentivized by the scale of the reward to be instrumentally rational and try to adopt tactics, situational plans of social action under Certeau (1980)’s theory of practice that help this high-leverage class navigate complex social structures.

In the case of college football, one such tactic we observe players adopt is the heuristic, “never go to a non-power conference school when a power conference school is available.” For example, consider again Brad Kaaya (Fig. 2). According to my model, Brad’s leverage (IQR)

over the probability of being drafted is ~ 0.586 , and his median draft probability is ~ 0.766 . By adopting the “never go to a non-power conference school” heuristic, he can reduce his leverage to ~ 0.071 , even though his median draft probability only increases modestly, to ~ 0.836 . Thanks to this tactic, Brad decreased his leverage to protect himself from the risk of going undrafted, even though it does not actually increase his median chance of being drafted very much. We see this kind of pattern elsewhere in the sociology of risk: Beck (1992) famously describes how modern institutions have shifted from determining the allocation of scarce goods to managing the allocation of “bads,” improving public welfare by removing negative structural factors and discouraging, outlawing, or otherwise making infeasible “bad” choices. But we might imagine that the reduction in leverage induced by policies like these could make people feel less free; their choices don’t make as much of a difference as they would without the policies in place.

The nonlinear relationships between leverage and other dimensions of agency like status, number of options, performance, and subjective freedom may help us investigate some of the definitional arguments about agency more broadly. Giddens (1984) tells us that “an agent ceases to be such if he or she loses the capability to ‘make a difference,’ that is, to exercise some sort of power” (14), but what it means to exercise power is a matter of debate. By Giddens’s logic, “power” is the “capability to make a difference,” that is, leverage, and the middle has more of that than the hyper-agents, at least when it comes to risk and opportunity in the case I analyze in this paper. However, the Bourdieusian understanding that agency comes with capital also makes sense; it is the expenditure of capital that fuels the “conversion process” between choices and outcomes (Bazzani 2022). It follows from this that the middle class should have more agency than the deprived, but less than the hyper-agents.

We can borrow from Certeau (1980) to think through this apparent contradiction: perhaps the hyper-agents exercise less leverage through their *tactics* (individual decisions), but more through their *strategies* (overarching patterns of behavior that shape entire fields). Perhaps the reason the elites seem to have no bad choices is that the *habitus* of a successful college quarterback is defined by the choices the elites tend to take, and it is those middle-of-the-road quarterbacks who adopt the best tactics to emulate that *habitus* who enjoy the most success in the draft. For the dimension of agency which is “being unconstrained in one’s decisions,” the inverse of Hays (1994)’s “pure constraint” strawman for structure, it is the middle class who have the *least*. The deprived may have fewer options than the hyper-agents, but both can operate without

feeling so strongly the constraints of risk and pressure that dominate the internal conversation of the “forgotten middle.” Think of Cardale, Brad, and Mason again: Mason doesn’t stand to lose anything in his college choice and is free to choose whatever he wants without reducing his expected draft odds, Brad sits on the “bubble” of the logistic curve, stands to lose everything and is incentivized to adopt tactics to prevent that loss, and Cardale has to rely on stochastic factors to get drafted anyway, so he might as well optimize for something other than draft position, hope to get lucky, and be prepared to improvise and navigate decisively if he does.

Just as structure is not “pure constraint,” agency is not pure freedom. It is better to think of it less as a sociological term with a definition we can articulate and more as a fundamental social process whose dimensions can be understood and measured individually, but which is counterintuitive and richly structured when examined on the whole. Sewell (1992) and Hays (1994) encourage us to understand structure as complex and dialectical, its material, virtual, and cultural dimensions interacting and intersecting to produce the resources and schemata people experience in everyday life. I want to follow Emirbayer and Mische (1998), Archer (2003), Hitlin and Elder (2007), Bazzani (2022) and others by embracing agency as multidimensional, dialectical, and synthetic, just like structure.

Sociological thinkers with strong attachments to specific dimensions of structure and agency have fueled the notorious debate around them by litigating and re-litigating definitions for social processes that defy simple definitions in the first place. This might explain why agency has been so “slippery” (Bazzani 2022): it is the synthetic product of the dialectic between its dimensions, not an underlying quantity that can be defined or measured in a way that will line up every time with what we expect from the literature or our own subjective understanding. Much productive sociological research into structure has been accomplished by scholars willing to set that debate aside and deal with structure on a dimension by dimension basis, comparing and complicating the role of those dimensions in patterns of stratification we see in the world (Hays 1994; Hallett and Ventresca 2006; Edelman et al. 2011). By being more curious and less defensive about the dialectic of agency, willing to allow it to subvert our expectations and contradict itself, we can pursue more generative quantitative research into agency in practice.

I would be remiss not to acknowledge the body of qualitative sociological research that does engage with individual-level stratification and the potential significance of life choices for life chances experienced later down the line. The managerial behavior described by Zelizer in

Economic Lives (2011), for example how people navigate corporate ethics issues, life insurance, and “risky exchanges” like adoption and care decisions, stratifies individual life courses based on deeply personal decisions. Fennell (2011) uses in-depth interviews with heterosexual couples to examine how individual decisions about contraception are stratified by gender, handing more and more of the responsibility for managing these potentially life-changing, high-leverage practices to women the more serious and long-term relationships become. Sherman and Schafft (2022) find a deep contradiction regarding the traditionally understood role of education in rural communities. Through interviews, they determine that parents’ resources make the socially mobilizing role of education more effective for children of better-off parents, so educational decisions are more consequential for households that have more to work with. The lack of quantitative research into individual-level stratification results more from the paucity of high-quality data to investigate the consequences of life choices than it does from a lack of related ideas in qualitative research and theory (Alkire 2005).

My analysis in this project is deliberately narrow. I cannot even make strong claims about the empirical reality of college football beyond the particular structural period I examine, which existed for less than a decade, and the 200 quarterbacks who inhabited it. But this is not a paper about the empirics of college football. This is a paper about how we can better understand a ubiquitous feature of the subjective “internal conversation” of agency (Archer 2003), a theoretical explication of that feature and how it has been present in the literature without being named for many years, and a suggestion that we involve that feature in future research as a way to approach opportunity structures we study as inhabited by actors. That feature, leverage, is an analytical tool we as sociologists can use to reinvest ourselves into individual-level stratification as a worthy and necessary empirical counterpart to group-level stratification, just as agency is a worthy and necessary theoretical counterpart to structure. It may be that, as Meyer and Jepperson (2000) argue, sociology has fallen into “the core conceit of modern culture,” that actors are autonomous and not really embedded in culture. They claim that “agency” as such is culturally constructed, essentially defending the “cultural dopes” critique Garfinkel famously leveled at Parsons (1937). This does not change the strong assumption people have, partially supported by my analysis, that some of their decisions matter. It is worth a deeper examination of the “series of traps” specific individuals navigate by making these choices in everyday life (Mills 1959). We cannot forget the people embedded in the systems. The traps may have teeth.

Almost nobody ever becomes a top 50 quarterback recruit. The overwhelming majority of Americans never play organized football. Yet everyone regularly faces key life decisions where they experience leverage. They move to new cities or new countries for work or relationships. They apply to college, graduate school, and competitive jobs. They take risks on business ventures, investments, activism, and passion projects. They may guarantee success thanks to structural or performance factors like Mason, make decisions that prove critical to the result like Brad, or be graced by serendipity like Cardale. They may be structurally boxed out from what they want like that substrate of red circles across the bottom of Figure 1. Most people likely embody many or all of these phenomena during the courses of their lives. There are many walks of life where the leverage dimension of agency is subjectively important. I strongly encourage more empirical research that investigates leverage across cultural *milieux*.

Consider a family choosing between neighborhoods when moving to a new city. Just a few neighborhoods that differ considerably in terms of amenities, transit, schools, local ordinances, demographics, and so forth would give this family more leverage over their life chances than would many homogeneous neighborhoods, at least according to my findings in this paper that greater diversity of options means more variation in outcomes than does a larger raw number of options (Fig. 5). Or consider a young person choosing between expensive hobbies when their family can only afford one. This family is structurally constrained by resources, but the young person is still faced with a consequential decision, one that changes depending upon what options are available and one which is more consequential precisely because the resources are scarce. Perhaps choosing between many musical instruments bears less leverage over what social scene the young person will belong to in high school than choosing between one instrument, one sport, and one visual art. It could be that the student is navigating tactically, just trying to get through high school with a resume item for their college application or looking for a way to make friends within rigid social mores, or they could be navigating strategically, a tastemaker whose decision shapes the preferences and leverage of their peers. Or the young person may discover that, like it was for Cardale Jones, the effect of their choice is completely unexpected (Merton 1936). Wherever actors participate in social structure, they are making consequential decisions. Wherever the data exists at the individual level with clear expectations for available choices, made choices, meaningful outcomes, and the structural and agentive factors that drive them, we can study how consequential those decisions were.

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APPENDICES

Appendix A: Data Sources

Data on player-side statistics was sourced from sports-reference.com wherever possible and ESPN otherwise. Data on schools was sourced from sports-reference.com and colleyrankings.com, with additional information on offensive schemes obtained from media coverage, team playbooks where available, the *NCAA Football 14* video game (itself researched from actual playbooks), and Wikipedia. Recruiting data was obtained from 247Sports.com whenever possible, with supplemental information from On3.com, media coverage, and ESPN.

Appendix B: Model Tests and Robustness Checks

Endogeneity

I return to the example of Cardale Jones to check my question from the data and methods section regarding the hypothetical where a player who would be a backup at a higher-ranked school attends a lower-ranked school and performs well. As he has repeatedly proven, Cardale is a valuable case study here; his choice is binary between the extremely prestigious Ohio State, where he was indeed a backup, and the mid-major Toledo, where he could plausibly have started. The model predicts that Ohio State, thanks to its power conference status and sky-high Colley average (and surely the associated media coverage that brought national attention to Cardale's magical championship run), increases Cardale's log odds of being drafted by 4.111 (the power status gap) $+ 0.290 \times 5.132$ (the Colley average gap) $= 5.599$, relative to Toledo. To overcome this difference, counterfactual Cardale at Toledo would need to produce, at the log odds per yard estimate from my model, 2109 additional passing yards over his Ohio State total (1460) to equal his Ohio State probability of being drafted, which is only about 1.5% as it is. To reach 50% probability of being drafted at Toledo, the model estimates he would have had to pass for approximately 5140 yards, tying David Klingler for the tenth-most any college quarterback has passed for in history. My model may underestimate leverage for people like Brad Kaaya, but for people like Cardale Jones, the counterfactual production differences would have to be outlandish.

Cross-validation

I test the model's performance using k-fold cross-validation with 5 groups. The ROC score from resampling is ~ 0.904 , representing about 90% of the values falling under the ROC

curve. The sensitivity is ~ 0.923 , and the specificity is ~ 0.719 . This performance is not perfect, but it's quite good, and I regard this model as plausible for conducting counterfactual analysis. The extremely large standard error for the triple option offensive scheme results from the triple option being a perfect predictor that a quarterback will not be drafted, which is expected because the triple option is a highly unconventional strategy with essentially no relationship to how NFL football is played. A variance inflation factor (VIF) test associates the scheme variable with a score of only ~ 1.82 , with no variable associated with a score above ~ 7.08 (touchdowns). These scores are not high enough to suggest serious issues with collinearity.

Ridge regression

I also test the model using ridge regression (L2 regularization). I find that the value of the “ λ ” parameter that results in the least error is very small (0.0631), which indicates that regularization may help slightly with model stability, but does not change the model coefficients very much; that is, the model is predictively accurate without being significantly simplified. Ridge regression is my preferred method of regularization because it does not drop variables that might contribute less to the outcome but are theoretically important (for example, race or offensive scheme) like LASSO (L1 regularization) does.

Alternative transfer operationalization

I build the model again under the same specifications, but with players' originally committed school, regardless of if they transferred. The resulting covariate matrix is below: effect sizes are similar across the board, but the AIC is increased, suggesting that players' final schools seem to have a cleaner statistical relationship to their eventual draft odds than their original schools (should those schools differ). An odd quirk of this model is that, controlling for other player-side stats, touchdowns for some reason have a small, nonsignificant negative effect on draft probability. This may be related to the modest collinearity concerns associated with this variable by the VIF test. The impact of attending a power conference school is also smaller than in the original model, likely because of players who “transfer down” and ultimately go undrafted. The other effects are all as expected from the original model, and this check does not alter my analytical conclusions or lessen my confidence in my model's ability to produce plausible counterfactual outcomes.

Table A1: Alternative Logistic Regression Model Covariate Matrix

	Estimate (log odds)	Standard Error	<i>p</i> -value
(Intercept)	-11.190421	4.214588	0.00793 **
Race (Other)	-1.215368	4.131451	0.76862
Race (White)	0.061676	0.910317	0.94598
4-Star Player	0.265355	0.801702	0.74065
5-Star Player	2.014801	1.417930	0.15533
Season Passing Yards	0.002889	0.001169	0.01341 *
Season Other Yards	0.002223	0.002090	0.28744
Season Total TDs	-0.010373	0.093382	0.91155
Season Total INTs	-0.219241	0.122065	0.07248 ‘
Colley Average	5.161987	2.981869	0.08343 ‘
Power Conference	1.519292	1.725505	0.37859
Scheme (Option)	-13.304879	2161.635904	0.99509
Scheme (Pro-Style)	-1.070655	2.386614	0.65371
Scheme (Spread)	-2.592386	2.419155	0.28390
Scheme (Multiple)	-0.474153	2.356877	0.84056

p-values marked with * are significant at the <.05 level, and those marked with ‘ are considered significant at the <.1 level.

Null deviance: 147.320 on 133 degrees of freedom

Residual deviance: 62.548 on 119 degrees of freedom

(1 observation deleted due to missingness)

AIC: 92.548

Alternative leverage calculation

I plot leverage, calculated with standard deviation (SD) instead of IQR, against the same three predictors I use in the results section. The results are mostly similar, but with some apparent inflation of leverage values for players with few offers and players with high passing yards, which slightly reduces the visual impact of the middle class effect I discuss in the analysis. These are the players for whom SD should be larger than IQR, since the players with few offers have more sparse distributions of offers and since elite quarterbacks have skewed draft probability distributions from non-power programs taking a chance by offering them a scholarship. This does not meaningfully change my findings, but it is an interesting point of comparison. It is also worth noting that 5 players have non-finite standard deviations due to receiving only one offer.

Figure A1: Leverage (SD) plotted against the raw number of scholarship offers each player had.

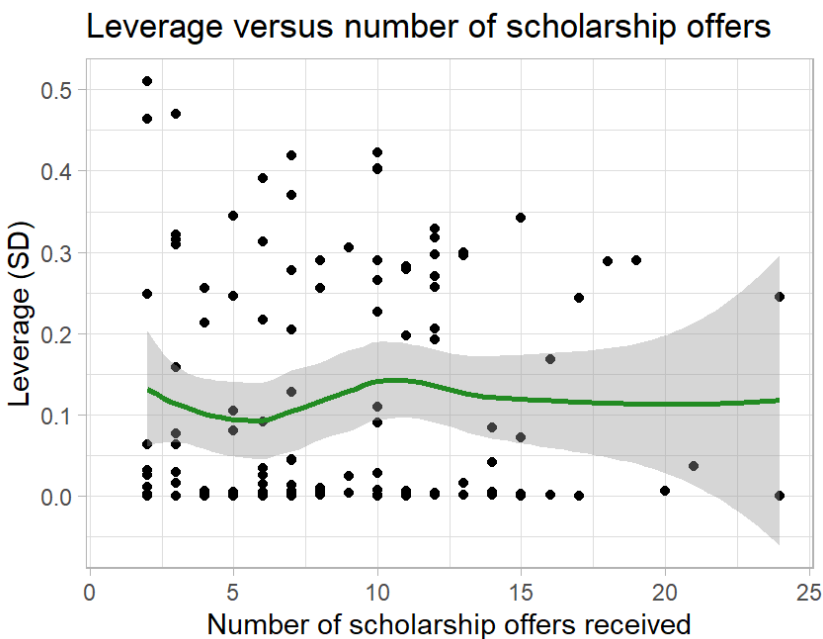


Figure A2: Leverage (SD) plotted against the Colley average of each player's factual school.

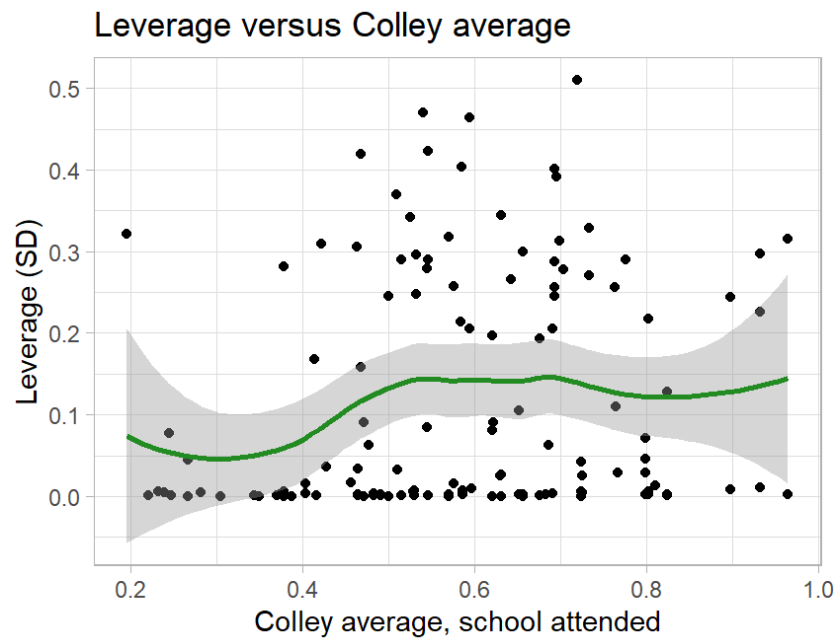


Figure A3: Leverage (SD) plotted against each player's final season passing yards.

