BORN OUT-OF-SEASON: TALENT ALLOCATION AND ECONOMIC CONDITIONS

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ABSTRACT. This paper studies how institutional rules shape talent allocation in tournament-style occupations characterized by low entry barriers, cumulative skill formation, and strict admission thresholds. Exploiting exogenous variation from the EU Structural Funds in England and Wales (1990–2000), we show that the policy intervention reduced the share of summer-born children while leaving GDP per capita unchanged. Using professional football as a proxy setting, we find that exposed cohorts underperform over their later professional careers. Because the policy affected fertility timing but not economic conditions, we attribute this pattern to quarter-of-birth effects operating through youth academy admissions: a lower share of relatively younger players weakens talent signals and increases selection based on short-term physical advantages.

Keywords: Talent allocation; Early-life conditions; Birth-timing; Professional sports.

JEL classification: J24, J13, Z22, R11.

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1. Introduction

Labor markets have long featured tournament-style occupations, in which many compete but only a few capture disproportionate rewards (Lazear and Rosen, 1981; Rosen, 1982). While specific occupations evolve, recent technological change and the rise of digital platforms have likely reinforced these winner-take-all dynamics by expanding market scale and increasing the returns to top-ranked talent (Cook and Frank, 1995; Autor et al., 2020).

This paper studies how a policy-induced shift in birth timing affects talent allocation in tournament-style labor markets with low entry barriers, cumulative skill formation, and strict admission thresholds. In such environments, individuals are selected into high-investment training tracks based on early-life signals of ability that are imperfect and often confounded by relative age within cohorts. As a result, small shifts in cohort composition can affect who is perceived as high-ability and admitted into elite pathways, with potential consequences for long-run productivity. A large literature emphasizes that such early selection environments are prone to misallocation of talent, as performance at young ages reflects both underlying ability and maturity differences, leading selection systems to over-reward relatively older individuals within cohorts and generate persistent inefficiencies in human capital allocation (Hsieh et al., 2019; Celik, 2023).

We use professional football data, an archetypal tournament labor market, where youth players are selected into academies based on age-group cutoffs, in England and Wales strictly defined by a September 1 rule. This generates systematic differences in relative age within cohorts, which provide a natural laboratory to study how early selection rules shape talent identification.

To study how such institutional rules affect talent allocation, we exploit exogenous variation in fertility timing generated by the European Union (EU) Structural Funds in

England and Wales during the 1990s and 2000s. Using a range of recent difference-in-differences estimators (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Borusyak et al., 2024), we show that these transfers induced regional changes in economic conditions that shifted the timing of births, reducing the share of children born in summer months. Importantly, we find no detectable effect on regional GDP per capita, suggesting that the policy primarily affected demographic timing rather than underlying economic conditions.

Applying a multi-period difference-in-differences estimator (Callaway and Sant'Anna, 2021), we find that the median player in cohorts exposed to the policy-induced shift in birth timing experiences lower long-run productivity in professional football, as measured by season-level market values. These effects emerge gradually over time, consistent with cohort exposure rather than contemporaneous economic shocks. The timing of the effects closely tracks the evolution of birth patterns, supporting a mechanism operating through relative age effects in youth selection rather than macroeconomic conditions.

We next examine the mechanism linking birth timing to productivity and assess a channel consistent with the aggregate patterns. We propose that a reduction in the share of summer-born children increased the relative prevalence of older, more mature individuals within cohorts at the time of selection into youth academies. This strengthened the role of maturity in early performance signals, making it more difficult to distinguish underlying talent from age-related advantages. This signal extraction problem led to less accurate selection and, ultimately, lower average productivity once maturity differences faded.

Consistent with this mechanism, we find that relatively younger (summer-born) players who are selected into academies exhibit higher subsequent performance, suggesting positive selection on unobserved ability. Finally, exploiting variation in exposure to the EU intervention, we show that player productivity declines with time since treatment for cohorts born after the shock, while no such pattern is present for pre-treatment cohorts.

This pattern is consistent with a mechanism operating through pre-birth fertility timing rather than post-birth developmental channels.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 outlines the institutional setting and empirical framework. Section 4 presents the main results and mechanism evidence. Section 5 concludes.

1.1. **An Overview of the Literature and Contributions.** In this subsection, we provide an overview of the three strands of literature considered in the paper: on tournament-like occupations, on socio-economic barriers, and on quarter-of-birth implications for socio-economic outcomes.

The analysis of tournament-style occupations originates with Lazear and Rosen (1981), who model compensation based on relative rank rather than absolute performance. Subsequent work extends this framework to multi-stage and elimination tournaments (Rosen, 1986; Bull et al., 1987). More broadly, these ideas have been applied in the context of human resource management practices (Lazear, 1995), and in the analysis of "winner-take-all" markets, where small differences in performance translate into disproportionately large rewards that have expanded beyond sports and entertainment to sectors such as finance, law, and corporate management (Cook and Frank, 1995). Professional football provides a particularly suitable setting to study these mechanisms, as career progression and rewards are tightly linked to relative performance, and small differences in ability can translate into large differences in outcomes.[1] In this context,

[1]Professional football is widely used as an empirical laboratory for studying labor market outcomes. First, it provides rich, high-frequency data on individual performance that are publicly observed, continuously updated, and largely verifiable. This reduces measurement error relative to many other labor market settings. Second, observed performance measures are less likely to be affected by Hawthorne-type behavior, as emphasized by Palacios-Huerta (2025). Third, European football operates under relatively market-oriented institutions compared to North American leagues, with fewer restrictions on player mobility and earnings (Hoehn and Szymanski, 1999). Finally, although clubs often face financial distress, soft budget constraints imply continued operation even under poor financial performance (Kornai, 1980; Kornai et al., 2003), which limits the sensitivity of outcomes to short-run financial shocks (Andreff, 2007; Szymanski, 2010).

we examine how institutional rules shape selection and subsequent success within a tournament-style occupation.

Foundational contributions on intergenerational transmission (*e.g.*, Becker and Tomes, 1979, 1986) highlight how parental choices and resources shape offspring outcomes. More recent empirical work quantifies the magnitude of these barriers: Bell et al. (2019) document that children from families below the median income are ten times less likely to become inventors than those from the top percentile of parental income. The same study shows that 82 percent of forty-year-old inventors are men, consistent with both historical (Lubczyk and Moser, 2024) and contemporary evidence (Hunt et al., 2013). Together, these gaps underscore how socio-economic barriers and opportunity costs prevent many potential "Einsteins" from contributing to innovation. In this paper, we move beyond parental choices and socio-economic background to study macro-level barriers, focusing on how administrative rules shape the chances of success.

A distinct literature documents differential outcomes tied to quarter of birth. It demonstrates that individuals born later in the year have systematically lower educational outcomes (among others, Angrist and Krueger, 1991; Bedard and Dhuey, 2006; Elder and Lubotsky, 2009), worse health and shorter life expectancy (Currie and Schwandt, 2013), and lower subsequent earnings. In parallel, evidence from studies on children participating in elite youth sports indicates that the oldest individuals (and thus those born immediately after the age-cutoff) are disproportionately represented. This pattern has been documented in a wide range of interdisciplinary literature and extends to adult participation (see, *e.g.*, Grondin et al., 1984; Barnsley et al., 1985; Barnsley and Thompson, 1988; Nolan and Howell, 2010).

While existing work shows that parents may shift fertility timing to confer a relative age advantage (Palacios-Huerta, 2024), evidence on such responses is mixed (Dickert-Conlin and Elder, 2010). This strategy may not be optimal for all individuals. In this paper, we show that in tournament-like settings such as professional football, relative age advantages interact with selection mechanisms. For individuals of average ability, being relatively older does facilitate early selection. However, selection based on underlying talent may be more likely among relatively younger individuals, as their performance reflects a purer signal of ability rather than a combination of talent and physical maturity. This suggests that the optimal timing of birth may depend on underlying ability, highlighting an important distinction between sports and other occupations without strict age-based entry thresholds.

2. Data

The primary dataset used in our analysis contains individual-level, season-specific data on professional male football players.[2] The dataset is self-constructed by compiling information available on the website *Transfermarkt.com*.[3] The sample includes all outfield players (*i.e.*, all players except goalkeepers) of British nationality born between 1985 and 2005 in England and Wales.[4] The data include each player's birthplace and date of birth (day, month, year), as well as player-season outcomes such as club affiliations, market value, the number of games played, the number of minutes spent on the pitch, the number of goals scored, and the number of assists provided, across national leagues as well as both domestic and international club competitions.[5]

[2] Professional football players are defined as individuals who, in at least one season of their careers, have a strictly positive market value. This restriction ensures that we focus on players who participate in professional football rather than on recreational players. Our analysis focuses on male players because the underlying data come from open-access football platforms maintained by fans and managers. Historically, especially before the 2000s, women's football was not followed or documented at a scale comparable to men's football, resulting in limited or nonexistent coverage of female leagues and player profiles.

[3] Transfermarkt, launched in 2000, operates as a relatively small organization with a structure similar to Wikipedia. Approximately 800 "data scouts" are responsible for entering and maintaining data. Each data scout typically focuses on a specific league or competition. These contributors rely on information provided by approximately 680,000 registered users who, free of charge, can propose corrections concerning players, managers, stadiums, and match records either publicly via Transfermarkt forums or privately through revision forms submitted directly to data scouts. Transfermarkt data are widely used in peer-reviewed economic studies, (see, *e.g.*, Bryson et al., 2012; Prinz and Thiem, 2021).

[4] We exclude goalkeepers because their performance metrics and career dynamics differ substantially from those of outfield players.

[5] Market values on Transfermarkt can be interpreted as estimates of the latent transfer fees that would be paid for a player in a current transfer. As such, they should be distinguished from realized transfer

We follow each player from the season in which they turn 16 until retirement, covering all seasons up to and including 2022/2023. We track players' careers across all football leagues both inside and outside the UK. For the oldest cohorts—those born in early 1985 (season 1984/1985)—we observe up to 22 seasons, or until age 38. For the youngest cohort—those born in late 2005 (season 2005/2006)—we observe only one season, up to age 17.

Table A1 in Online Appendix displays the main summary statistics on players' performance measures including market values. Our sample comprises a total of 3,006 players and 34,314 player-by-season observations.[6] On average, a player's career spans 11.4 seasons, and the average peak market value per player of 2.4 million euros. Looking at the performance, across all club competitions, players participate in an average of 19.1 games per season, spend time on the pitch for the equivalent of 15.2 full-length games, score 2.1 goals, and provide 1.4 assists.

Births are unevenly distributed across quarters: 29 percent of players are born in the first quarter (December–February), 22 percent in the second quarter (March–May), 16 percent in the third quarter (June–August), and 34 percent in the fourth quarter (September–November).[7] This compares to the birth distribution of males in the population born over the same period: 24, 25, 26, and 25 percent, respectively.

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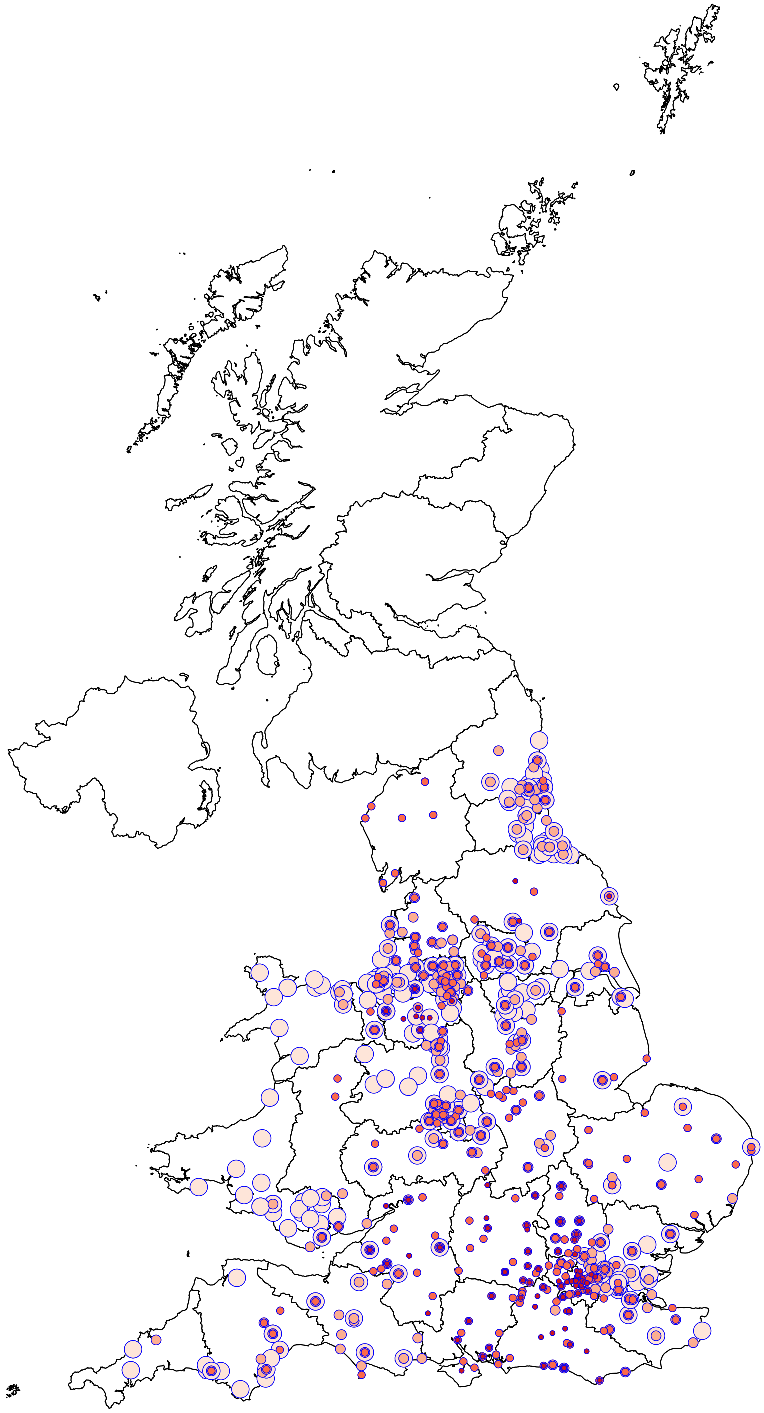
fees, which are observed only upon completion of a transfer. Market values reported on Transfermarkt are updated twice a year using a qualitative approach. The system is human-operated and relies on the "wisdom of the crowd," incorporating user feedback alongside assessments by data scouts. A potential concern with this approach is the lack of transparency and replicability of these values, as well as the possibility that user-suggested valuations may influence player values rather than reflect them (see, *e.g.*, the *New York Times* article: `https://www.nytimes.com/2021/08/12/soccer/soccer-football-transfermarkt.html`). However, Müller et al. (2017) show that market values estimated using players' performance and other characteristics are statistically indistinguishable from those reported on Transfermarkt, alleviating these concerns. They also emphasize that market values and realized transfer fees are conceptually distinct, making direct comparisons between the two difficult. For this reason, we rely on Transfermarkt market values and do not validate them using realized transfers.

[6] We restrict our sample to players with a known date and place of birth, and known club affiliations.

[7] We define birth quarters relative to the September 1 eligibility cutoff used by youth football academies in England and Wales. To ensure balance, we include players born in December 1984 and exclude those born in December 2005.

Figure 1. Distribution of players' birthplaces



Note: Each circle represents a player's birthplace (geolocalized by geographic coordinates). Circle shading reflects the GDP per capita of the NUTS 2 region in which the birthplace is located: darkest, smallest circles correspond to the highest GDP regions, while lightest, largest circles correspond to the lowest. GDP per capita is divided into four quartiles: top 25 percent, top 25–50 percent, top 50–75 percent, and bottom 25 percent, computed using birthplaces of all UK-born players in the sample (1985–2005). NUTS 2 borders are outlined.

This pattern indicates a substantial overrepresentation of relatively older players within cohorts and is consistent with the relative-age effect documented by Palacios-Huerta (2024) for professional football players in the first and second tiers of the Spanish football league during 1976–2015, where the corresponding distribution is 29, 24, 15, and 33 percent among players, compared to 25, 26, 25, and 24 percent in the population.

In our dataset, height information is available for most, but not all, players (93 percent). However, height is more likely to be recorded for more accomplished players: those with non-missing height have significantly higher peak market values (2.6 million euros versus 157 thousand euros on average). As a result, the observed height data pertain disproportionately to a positively selected subgroup of more successful players.

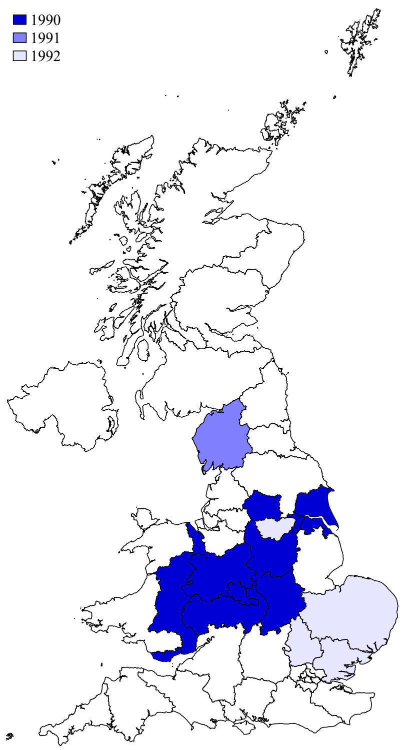
Importantly, even within this group, height varies substantially—from 160 cm to 207 cm, with a mean of 181 cm and a standard deviation of 6.2 cm. This wide dispersion suggests that there is no strict height threshold for reaching the professional level. The absence of a binding physical requirement reinforces the argument that entry into professional football is not constrained by a narrow physical cutoff.

Finally, height is only observed at the time of data collection and not at the time players were scouted. Given substantial heterogeneity in growth trajectories, current height cannot be reliably mapped to height at the time of scouting. Combined with the non-random pattern of missingness, this motivates our decision to exclude height from the analysis.

In Figure 1, we illustrate the geographical distribution of players' birthplaces across England and Wales. Each circle marks a player's birthplace: the darkest and smallest circles correspond to the highest-GDP regions, while the lightest and largest circles correspond to the lowest-GDP regions. The figure highlights substantial variation across geographies and in GDP per capita at the place and time of birth. Our empirical strategy exploits exogenous, policy-driven changes in these conditions induced by EU Structural Funds, which we introduce next.

3. Estimating the Causal Impact of EU Structural Funds on GDP and Fertility

In this section, we provide causal evidence of no response in GDP per capita and a decline in the population share of summer-born children in England and Wales in the years following the implementation of the EU Structural Funds. To the best of our knowledge, this is the first paper to document these patterns.



(A) 1990–1993 (B) 1994 (C) 2000

Figure 2. Treated regions by year of first treatment

The EU Structural Funds in the 1990s and the early 2000s were the EU's main instrument for supporting social and economic development, accounting for roughly one-third of the EU budget. The funds were administered across the EU at broader administrative units defined by the NUTS classification—a geographical standard that divides the economic territory of the EU into a hierarchical system of nested regions for statistical purposes: NUTS 1, NUTS 2, and NUTS 3. Classification into a given NUTS level is determined by population thresholds: NUTS 1 corresponds to regions with 3 to 7 million inhabitants, NUTS 2 to regions with 0.8 to 3 million inhabitants, and NUTS 3 to regions with 0.15 to 0.8 million inhabitants.[8]

The EU Structural Funds were administered under one of three objectives (Objective 1, Objective 2, and Objective 3. Objective 1 targeted regions lagging behind the EU average in terms of development, as measured by GDP per capita; Objective 2 targeted areas undergoing structural difficulties; and Objective 3 aimed to support the modernization of education, training, and employment systems. Each objective targeted a specific level of the NUTS classification: Objective 1 was designed for NUTS 2, while Objectives 2 and 3 applied to NUTS 3. All objectives were implemented over the same three programming periods: 1989–1993, 1994–1999, and 2000–2006, with additional regions included under each objective in successive waves.

3.1. **Identification.** In our analysis, we focus on Objectives 1 and 2, which accounted for the largest EU transfers. The official EU Annual Reports consistently list the names of all NUTS 2 regions treated under Objective 1. For Objective 2, however, the reports list the names of funded programs without directly linking them to specific NUTS 3 units. While these descriptions allow us to reliably identify the broader NUTS 2 regions in which the programs were implemented, they do not permit precise assignment to individual NUTS 3 units. To avoid misassigning treatment, we therefore aggregate Objective 2 eligibility to the NUTS 2 level: if any Objective 2 program was implemented within a NUTS 3 region, we classify the entire NUTS 2 region as treated. In Figure 2 we illustrate the treated regions by the year of the first treatment.

In this analysis, we exploit all three programming periods, each of which generates substantial staggered treatment variation. We define the year of treatment as the first year in which EU Structural Funds expenditures were recorded for a given region, as reported in

[8]In this paper, we adopt the 2016 NUTS classification and keep it constant throughout the analysis. Under this classification, England and Wales are divided into 10 NUTS 1 regions, 35 NUTS 2 regions, and 151 NUTS 3 regions, with an average population of approximately 5.2 million, 1.5 million, and 0.3 million, respectively. The corresponding average area is approximately 15,1000 km^2 for NUTS 1, 4,300 km^2 for NUTS 2, and 1,000 km^2 for NUTS 3. For more details, see Table A2 in Online Appendix.

the EU Annual Reports. During the first programming period (1989–1993), we observe considerable variation in the timing of treatment (1990, 1991, or 1992), reflecting delays in the implementation of the program. In contrast, during the second and third programming periods (1994–1999 and 2000–2006), all eligible regions were treated in the same year, at the start of the programming period (1994 and 2000, respectively).

Let i index regions and t denote time in years. Let G_i denote the treatment year of region i, with $G_i = \infty$ for regions that are never treated.[9]

We are interested in the group-time average treatment effect on the treated for the cohort of regions that first receive treatment in period g and are observed in period t:

$$ATT(g,t) = E[Y_{it}(1) - Y_{it}(0) \mid G_i = g, t \geq g], \tag{1}$$

where $Y_{it}(1)$ and $Y_{it}(0)$ denote the potential outcomes for treated and untreated regions, respectively. In period t, regions that are either never treated ($G_i = \infty$) or not yet treated ($G_i > t$) serve as valid controls for regions with $G_i = g$.

Identification requires that, conditional on region and year fixed effects, untreated potential outcomes follow parallel trends across groups:

$$E[Y_{it}(0) - Y_{i,t-1}(0) \mid G_i = g] = E[Y_{it}(0) - Y_{i,t-1}(0) \mid G_i = g'] \qquad \forall g, g'. \tag{2}$$

This assumption is plausible because treatment assignment is based on predetermined economic criteria, and its timing is driven by eligibility thresholds rather than expected future outcomes.

We implement four of the most recent DiD estimators for multiple time periods along with a baseline OLS specification. As we show below, the complementarity among the estimators—from those proving clean event-study dynamics, to those offering transparent treatment DiD contrasts, to those producing ATT—allows us to assess robustness to

[9]For never-treated regions G_i can be either 0 or missing, depending on the econometric technique applied.

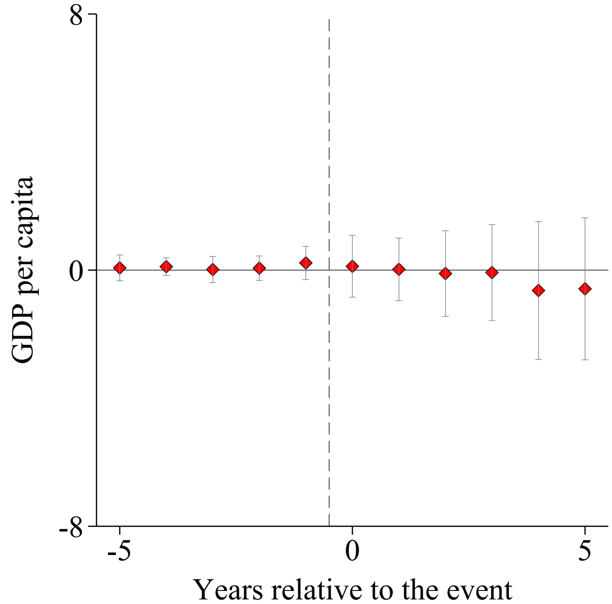
alternative identifying assumptions. (i) Two-way fixed effects (OLS). We begin with the canonical regression:

$$Y_{it} = \alpha_i + \lambda_t + \beta D_{it} + \varepsilon_{it}, \tag{3}$$

where D_{it} is a treatment indicator for eligibility under Objective 1 or 2, α_i are region fixed effects, λ_t are year fixed effects, and ε_{it} is the error term.

While widely used, this specification is biased in staggered adoption settings because already-treated units may serve as controls for later-treated units. (ii) Second, we implement the Sun and Abraham (2021) contamination-free event-study estimator that constructs cohort-specific event-time indicators and excludes already-treated cohorts from the control group when estimating Equation (3). This produces unbiased dynamic treatment effects and valid pre-trend diagnostics. (iii) Our third estimator is proposed by De Chaisemartin and d'Haultfoeuille (2020). At each year t, the estimator compares the groups that just became treated with the groups that did not change their treatment status and forms non-negative weighted averages of all valid 2×2 DiD contrasts. (iv) Next, we implement the Callaway and Sant'Anna (2021) estimator, which—unlike the previously discussed methods—directly computes the group–time average treatment effects $ATT(g,t)$. At each year t, the control pool consists of all units that are not-yet-treated or never treated. Without covariates, the outcome regression reduces to the mean untreated outcome in year t, so the treated residual is simply the difference between the treated outcome and this control mean. The doubly robust correction subtracts a stabilized inverse-probability-weighted average of the control residuals, where weights are functions on the number of units in cohort g. The resulting estimator equals the treated residual minus the weighted average control residual, ensuring unbiasedness even if either the regression or the weighting model is misspecified. (v) Finally, our second ATT-estimator is proposed by Borusyak et al. (2024). The estimator pools all never-treated and not-yet-treated observations to estimate a single outcome model for untreated potential

Figure 3. Impact of EU Structural Funds on GDP per capita



Note: The figure reports ATT estimates using the methodology of Callaway and Sant'Anna (2021), displaying point estimates and 95 percent confidence intervals for the pre- and post-treatment periods. Standard errors are clustered at the NUTS 2 level. The data used for the estimation cover the years 1985–2005. The control group comprises regions that are never treated or not yet treated. GDP per capita is expressed in 1,000 euros.

outcomes, typically implemented as a regression with region and year fixed effects and possibly additional covariates or trends. Then, this estimated model is used to impute counterfactual untreated outcomes for every treated unit in every post-treatment period. The resulting estimator equals the difference between observed and imputed outcomes.

We begin by addressing three key issues relevant for identification. First, time-invariant regional characteristics are addressed by region fixed effects, while common shocks are absorbed by year fixed effects. Second, omitted variables that evolve smoothly over time are accounted for by the use of modern DID estimators that rely only on variation across treatment cohorts. Third, treatment assignment is determined by eligibility thresholds based on macroeconomic and structural indicators, limiting concerns about reverse causality.

3.2. **Data.** In this analysis, we use annual NUTS 2-level data on GDP per capita from the ARDECO dataset and quarterly data on male live births from the UK Office for National Statistics.[10]

Our sample covers the years 1985–2005. Because our event-study specification includes five pre-treatment and five post-treatment years, this window ensures that we observe the earliest not-yet-treated regions early enough to contribute to the pre-trend analysis, as well as the latest treated regions long enough to contribute to the post-treatment dynamics.

3.3. **Results.** Figure 3 reports the estimation results from the Callaway and Sant'Anna (2021) estimator for regional GDP per capita. The horizontal axis displays five pre-treatment periods, the treatment year, and five post-treatment periods. The vertical axis plots the estimated ATT, expressed in thousands of euros per capita.

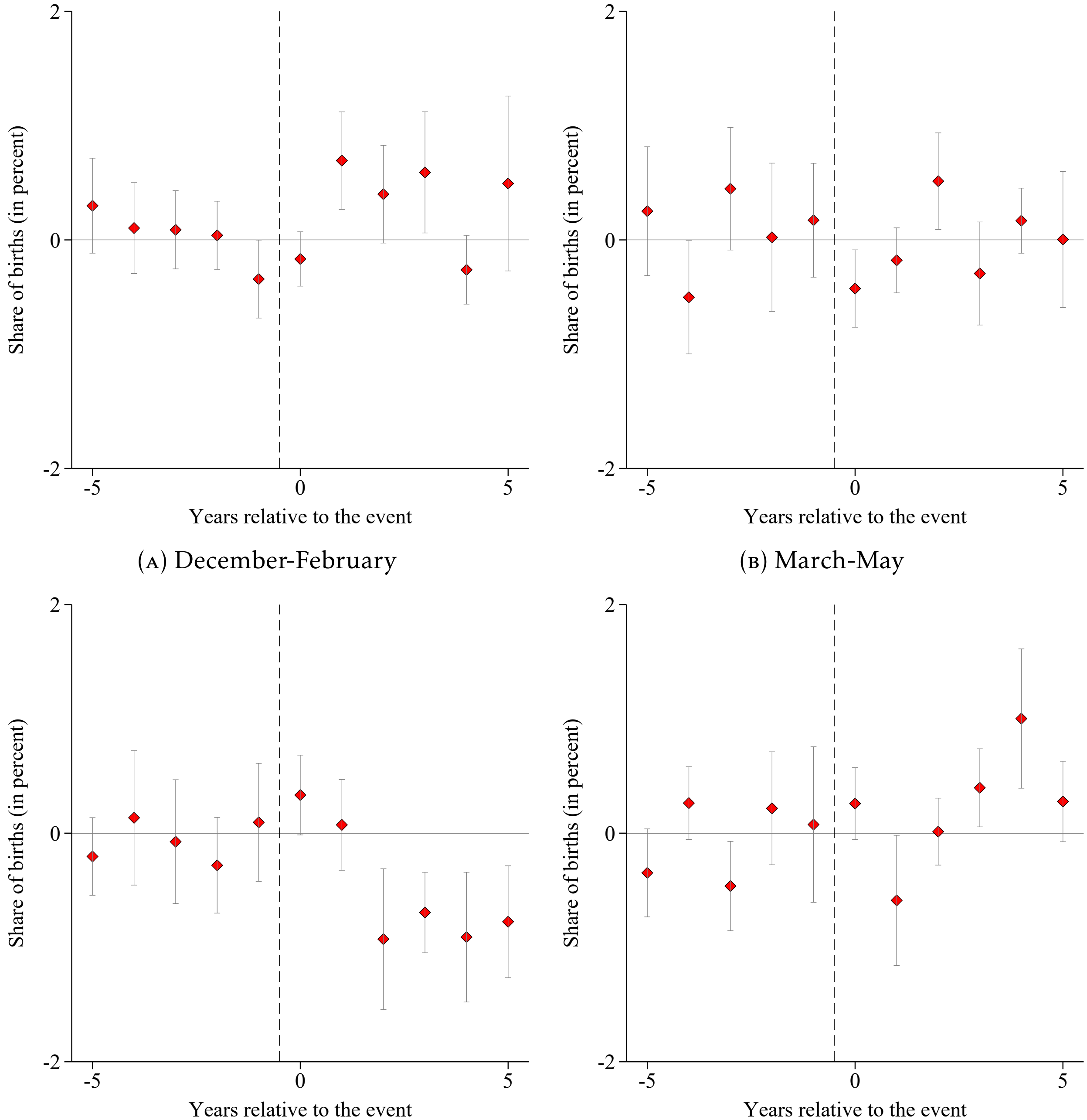
Result 1. *We find no evidence of differential pre-trends and no statistically significant treatment effects on regional GDP per capita. All estimated coefficients are both small in magnitude and statistically indistinguishable from zero. Importantly, the remaining estimators—including Borusyak et al. (2024), Sun and Abraham (2021), and De Chaisemartin and d'Haultfoeuille (2020)—yield the same conclusion. Across all methods, specifications, and assumptions, there is no indication that the EU Structural Funds had any measurable impact on GDP per capita in the studied regions over the five-year period following the intervention.*[11]

The absence of a detectable effect on GDP per capita is consistent with the design of the EU Structural Funds programs. Although Objective 1 regions were considerably poorer than the rest of the EU, the classification does not imply that eligibility should generate short-run increases in GDP per capita. Regions treated under Objective 1 typically faced

[10]Annual GDP per capita is calculated by dividing GDP at constant 2015 euros ($SNPTD$) by total annual average population ($SOVGD$). Additional information on the dataset is available at https://knowledge4policy.ec.europa.eu/territorial/ardeco-database_en#database.

[11]See Figure A1 in Online Appendix for all sets of estimates.

Figure 4. Impact of EU Structural Funds on birth distributions



(a) December-February

(b) March-May

(c) June-August

(d) September-November

Note: The figure reports ATT estimates using the methodology of Callaway and Sant'Anna (2021), displaying point estimates and 95 percent confidence intervals for the pre- and post-treatment periods. Standard errors are clustered at the NUTS 2 level. Standard errors are clustered at the NUTS 2 level. The data used for the estimation cover all male live births in England and Wales from 1985 to 2005. The control group comprises regions that are never treated or not yet treated.

deep structural challenges, and the Structural Funds financed long-horizon investments (infrastructure upgrades, human capital programs, administrative capacity).

Objective 2 programs were even less directly connected to GDP performance, as eligibility was based on sectoral restructuring needs rather than income levels. Consequently, the absence of detectable effects in our estimates is consistent with the institutional design of the program and the nature of regional development dynamics. For example, in the West Midlands region, EU Structural Funds were directed toward goals such as "creating a diverse and dynamic business base by improvements to infrastructure, businesses and industry," "creating the conditions for employment growth," and "regenerating communities." The program focused on areas characterized by multiple and overlapping forms of deprivation, including unemployment, poor skills, low incomes, poor housing, and high crime.[12]

Figure A2 presents the estimation results from the Callaway and Sant'Anna (2021) estimator for regional quarterly birth shares. The estimates are shown separately for each quarter. We display the first quarter in the top-left panel and the last quarter in the bottom-right panel, arranging two quarters per row. The vertical axis reports the quarterly share of births (in percent), while the horizontal axis shows time relative to the intervention.

To account for systematic differences in underlying economic conditions, we control for the interaction between each region's pre-treatment GDP level and a linear trend in calendar time. This specification allows richer and poorer regions to drift apart at a constant rate over event time without imposing a separate dummy for each event year.[13]

[12]Source: *European Structural Funds Programmes 2000 to 2006 – Celebrating Success* (West Midlands ERDF Programme), UK Government, available at https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/91905/ERDF_West_Midlands_celebrating_success_2000_to_2006_ERDF_programme.pdf.

[13]Including a control that interacts initial GDP levels (computed as the five-year pre-treatment average of GDP per capita) with event time helps ensure that differences in birth outcomes are not mechanically driven by pre-existing economic conditions. Regions eligible for EU funding—especially those receiving Objective 1 support—were systematically poorer, and these baseline differences may have shaped fertility decisions independently of the intervention. By conditioning on initial GDP and allowing its influence to

Result 2. *In Q3 (June–August), the pre-treatment coefficients lie tightly around zero and exhibit no discernible pre-trend. Beginning in the second year after the intervention, the estimates display a noticeable and persistent decline. The post-treatment coefficients remain consistently negative for several years, indicating that the pattern is unlikely to be driven by sampling variation alone. The combination of the absence of pre-trends, the stability of the post-treatment decline, and statistical significance suggests that the summer-month reductions may reflect a modest but genuine response to the intervention. This interpretation is further supported by the absence of a statistically significant effect in the year of the intervention itself, which is expected given that births in that year largely reflect conceptions that occurred before the policy—approximately nine months earlier.*[14]

For the remaining three quarters (Q1, Q2, and Q4), the coefficients fluctuate considerably in both sign and magnitude before and after the intervention, suggesting that these estimates are driven largely by noise.

Taken together, Results 1 and 2 point to no detectable effects on GDP per capita alongside meaningful shifts in fertility timing, suggesting that the intervention affected birth timing rather than underlying economic conditions.

4. Decline in Productivity and Suggestive Evidence on Mechanisms

In Section 3, we document our first-stage results: the EU Structural Funds changed quarterly birth-share patterns in England and Wales and did not cause statistically significant

vary linearly over event time, we isolate variation more plausibly attributable to the policy rather than persistent cross-regional disparities in economic development. This adjustment also improves comparability between treated and untreated regions in the post-treatment period and reduces concerns that differential underlying trends could confound the estimated treatment effects. For never-treated regions, we define the pre-treatment window as the three years preceding the earliest treatment in the sample (1986–1989). We also deseasonalize the birth shares by subtracting region-specific pre-treatment averages, ensuring that the estimated treatment effects are not confounded by persistent seasonal patterns.

[14] The estimated effects are robust to including year-specific interactions with baseline GDP per capita and to using non-seasonally adjusted birth shares. The decline in share of birth in Q3 is further supported by the De Chaisemartin and d'Haultfoeuille (2020) and Sun and Abraham (2021) estimators, even though the effect does not reach conventional levels of statistical significance. The Borusyak et al. (2024) estimator cannot be implemented due to insufficient variation in the data as the data on births cover only England and Wales.

changes in GDP per capita during the five-year window following the intervention. In this section, we shift our focus to the subpopulation of players. First, we provide causal evidence of a decline in their lifetime productivity following the EU intervention. Then, we present suggestive evidence pointing to quarter of birth as a potential mechanism underlying the decline.

We begin by estimating Equation (1) using the Callaway and Sant'Anna (2021) estimator for repeated cross sections, where the unit of observation is a year-of-birth-by-region cohort. We weight each cohort-level observation by the number of players in the cohort to account for differences in precision, giving greater weight to cohorts with more observations.

A key empirical challenge is that cohorts born in the year of implementation of the third wave of the EU Structural Funds program (2000) can only be observed up to age 22 in our data. Since players peak at different stages of their careers, this truncation would lead to non-comparable measures of lifetime productivity across cohorts. To ensure comparability, we restrict attention to a common age window (16–24) and exclude the third wave of the intervention. As a result, the first two waves (1990–1994) each contribute observations from five years prior to five years after treatment.[15]

We measure cohort-level performance by first computing, for each player, the average log market value over ages 16–24, and then taking the median across players within each year-of-birth-by-region cohort.[16] We prefer an average to a peak market value, as the latter can be driven by transitory shocks—such as a single exceptional season, temporary hype, or reporting errors—and may occur outside the observed age window. In particular, if players peak at different ages, peak-based measures within a fixed window

[15]Extending the age window beyond 24 would imply that outcomes observed five years after treatment are driven only by the first wave of the intervention. Shortening the age window would unnecessarily reduce the observable portion of players' careers.

[16]Because market values are highly skewed, we use log market value as our main outcome variable. Specifically, we define $\ln(1 + \text{market value})$ to retain players with a recorded market value of zero. The logarithmic transformation compresses the upper tail of the distribution and reduces the influence of extremely high-valued players.

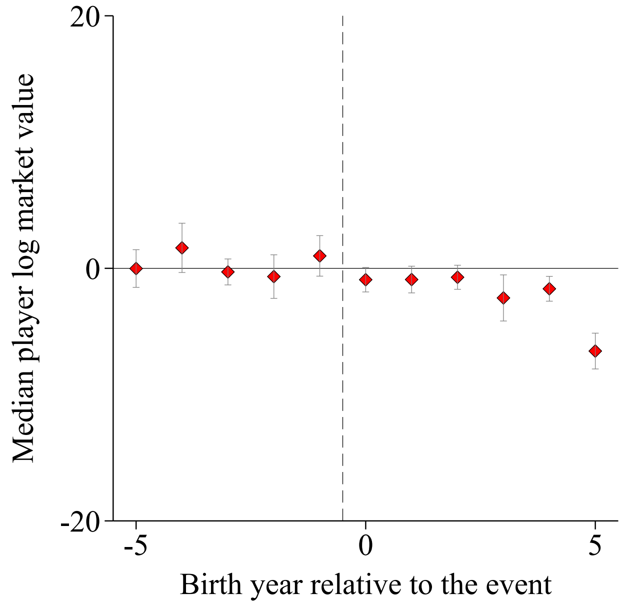
may be downward biased for late-peaking players, reducing comparability across cohorts. Averaging over a common age range mitigates this concern by capturing sustained performance along the career trajectory. We then take the median across players to reduce the influence of extreme observations and to capture the performance of a representative player, rather than outcomes driven by a small number of high-value "super star" individuals.

Finally, a key feature of our design is the choice of the control group. We restrict the control group to never-treated cohorts and exclude cohorts that are "not yet treated." This restriction is crucial for isolating the quarter-of-birth mechanism from post-birth channels. Individuals born prior to the intervention but whose formative years overlap with the implementation period may be indirectly exposed to treatment through improved childhood environments, changes in local labor market conditions, or enhanced access to sports infrastructure. Including such cohorts would therefore confound the interpretation of the estimates, as post-birth exposure could operate through channels distinct from birth timing and cohort composition. By focusing exclusively on cohorts that are either born under treatment or never exposed at any point during childhood, the comparison isolates variation in birth timing induced by the intervention. Under this design, estimated effects can be attributed to differences in cohort composition and relative age at selection, rather than to changes in economic conditions experienced during childhood or adolescence.

Figure 5 presents the resulting average treatment effect on the treated (ATT) estimates. The vertical axis reports median log market value (in millions of dollars), while the horizontal axis shows years relative to the intervention.

Result 3. *We find no evidence of differential pre-trends in median log market values prior to the intervention: the estimates are small, fluctuate around zero, and are statistically indistinguishable from zero.*

FIGURE 5. Impact of EU Structural Funds on median player log market values



Note: The figure reports ATT estimates using the methodology of Callaway and Sant'Anna (2021), displaying point estimates and 95 percent confidence intervals for the pre- and post-treatment periods. Standard errors are clustered at the NUTS 2 level. The data used for the estimation cover the years 1985–2005. The control group comprises regions that are never treated.

Following the intervention, the decline in median log market value begins with a delay of three years and remains negative and statistically significant thereafter.

This delayed response is consistent with a mechanism operating through quarter of birth. In particular, the absence of an immediate effect at the time of the intervention, combined with the emergence of statistically significant effects only in subsequent years, aligns with a channel driven by birth timing. Because quarter of birth is determined at birth, only cohorts conceived and born after the intervention can be affected. As a result, any impact on outcomes should arise with a delay, rather than contemporaneously.

4.1. **Suggestive evidence: Non-intervention specific.** The first piece of suggestive evidence for the quarter of birth mechanism as a driver of the documented decline in players' productivity relies on OLS estimates of the relationship between a player's birth quarter and several indicators of sporting performance. These outcomes reflect player success and include log market value, the number of games played, minutes spent on the pitch, goals scored, and assists provided. This analysis establishes a general relationship between quarter of birth and player performance, independent of the EU intervention. It includes all players in the sample born between 1985 and 2005 and uses all available season-specific observations across ages.

In each regression, we control for regional economic conditions at the time and place of player's birth. We consider four levels of spatial granularity. Three of these correspond to the NUTS classification (NUTS 1 through NUTS 3). The fourth level of granularity corresponds to the geographical coordinates of the city of birth. At the NUTS 1–3 levels, we measure economic conditions using data on GDP per capita from the ARDECO dataset. At the geographical-coordinate level, we proxy for economic conditions using global satellite nighttime light data from the World Bank - Light Every Night dataset.[17]

Table 1 presents OLS regression estimates that relate player's log market value in each season to the player's birth quarter, for all players in the sample. In all four specifications, we allow the effect of birth quarter to vary with local economic conditions by including interactions between birth-quarter indicators and the economic activity measure at birth. All specifications include fixed effects for the season of observation, the player's year of birth, and the player's place of birth.[18] Columns (1) to (4) correspond to increasingly disaggregated territorial units—that is, from the most aggregate level (NUTS 1) to the most detailed level. Due to the interactions and standardizations, each birth-quarter coefficient reflects the effect of that quarter relative to Q4 for the mean value of each measure of economic activity.

Regarding the quarter of birth, we observe that:

[17]Additional information on the dataset can be found at https://registry.opendata.aws/wb-light-every-night.

[18]Place-of-birth fixed effects match the spatial unit used in measuring economic conditions: NUTS-region fixed effects in the NUTS specifications, and $0.1° \times 0.1°$ grid-cell fixed effects ($\approx 80 - -120\text{km}^2$) in the nighttime-light specification.

Table 1. OLS results: Birth conditions and market values

Dependent variable: **Log of market value**	GDP per capita in NUTS level: 1	2	3	Light intensity
	(1)	(2)	(3)	(4)
Q1 (Dec-Feb)	0.106***	0.097***	0.095***	0.117***
	(0.023)	(0.027)	(0.029)	(0.038)
Q2 (Mar-May)	0.134***	0.133***	0.124***	0.137***
	(0.020)	(0.033)	(0.035)	(0.047)
Q3 (Jun-Aug)	0.144***	0.136***	0.138***	0.130**
	(0.044)	(0.044)	(0.040)	(0.060)
Observations	34,314	34,314	34,314	16,476
Adjusted R^2	0.064	0.067	0.082	0.187
Season FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Birth region FE	Yes	Yes	Yes	Yes

Note: The table reports four separate OLS regressions in which the dependent variable is regressed on interactions between the player's birth quarter and one of four measures of economic activity at the time and place of birth. Each regression includes controls for the player's birth year, birth location, and season fixed effects. Following Angrist and Pischke (2009), we refrain from including club fixed effects due to their potential to act as "bad controls." All measures of economic activity (GDP per capita, light intensity) as well as the dependent variable are standardized. The omitted birth quarter is Q4 (Sep–Nov). Due to the interactions and standardizations, the coefficients on measures of economic activity reflect the average effect for the base quarter (Q4), and each birth-quarter coefficient reflects the effect of that quarter relative to Q4 for the mean value of each measure of economic activity. Place-of-birth fixed effects match the spatial unit used in measuring economic conditions: NUTS-region fixed effects in the NUTS specifications, and $0.1^\circ \times 0.1^\circ$ grid-cell fixed effects ($\approx 80--120\text{km}^2$) in the nighttime-light specification. Standard errors are clustered at the place-of-birth fixed-effect level. Regressions using GDP include all players born between 1985 and 2005, while regressions using light intensity (due to data availability) are restricted to cohorts born between 1992 and 2005. Significance at the 90 percent, 95 percent, and 99 percent confidence levels is indicated by *, **, and ***, respectively.

Result 4. *Across specifications controlling for various measures of regional geography and for the average level of economic activity, we find that players born in Q4 (September to November) have lower market values than those born in other parts of the year, particularly those born in the summer. For example, a player born in Q3 (June to August) has approximately a 0.14 standard deviation higher market value than a player born in Q4.*

Beyond market value, we also document systematic differences across other performance outcomes. As shown in Tables A3–A6 in Online Appendix, summer-born players outperform those born in all other quarters. Compared to fall-born players, summer-born players perform about 0.1 standard deviations better in terms of games played, minutes on the pitch, goals scored, *and assists provided. They also outperform players born in winter and spring months, with estimated coefficients roughly 50 percent larger across these performance measures.*

These patterns are robust across alternative definitions of geographic controls. However, when using more granular measures—such as average light intensity within narrowly defined spatial bins—the estimates become less precise and are not consistently statistically significant.

Notably, Q3 is the only quarter for which all five outcomes (market value, games played, minutes, goals, and assists) are statistically significant across all geographic specifications, with the exception of the light-intensity-based measure.

4.2. **Suggestive evidence: Intervention-specific.** To further assess the quarter-of-birth mechanism as a driver of the decline in players' productivity, we construct an empirical exercise around the EU treatment periods in which we relate player productivity to the timing of birth relative to treatment (before versus after birth). In particular, we consider the following baseline specification:

$$\begin{aligned}\ln mv_{is} = \alpha_0 + \sum_{j=1}^{3} \alpha_j \text{BirthQuarter}_{ij} + \beta_0 \text{Distance}_i + \sum_{j=1}^{3} \beta_j \text{Distance}_i \times \text{BirthQuarter}_{ij} \\ + \text{SeasonFE}_s + \text{BirthYearFE}_i + \text{BirthRegionFE}_i + \varepsilon_{is},\end{aligned} \tag{4}$$

where $\ln mv_{is}$ denotes log(1 + market value) of player i in season s, BirthQuarter_{ij} is a dummy variable equal to 1 if player i is born in quarter j, with the fourth quarter omitted as the reference category (September–November, based on the relevant age-group cutoff), and Distance_i is the variable of interest capturing the timing of birth relative to treatment for player i, defined in more detail below. The fixed effects SeasonFE_s, BirthYearFE_i, and BirthRegionFE_i absorb systematic differences across seasons, birth years, and regions of birth, respectively. The error term is denoted by ε_{is}.[19]

[19]We do not include age controls, as age is mechanically determined by season and year of birth, both of which are already absorbed by fixed effects. We also do not include height, because height information is missing in a non-random way, as discussed in Section 2. Finally, following Angrist and Pischke (2009), we refrain from including club fixed effects due to their potential to act as "bad controls." Since we are

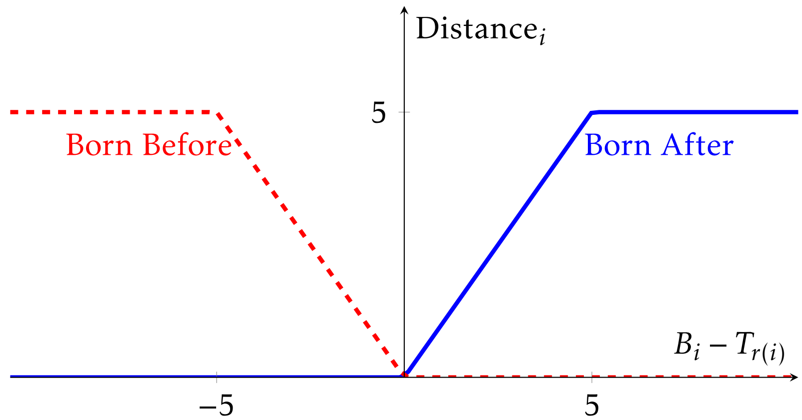


FIGURE 6. Distance to treatment as a function of birth timing

To disentangle the quarter-of-birth mechanism from other channels, we estimate two sets of regressions based on Equation (4), differing only in the definition of Distance$_i$.

Let B_i denotes the birth year of player i, and $T_{r(i)}$ denotes the year in which treatment starts in player i's region of birth $r(i)$. In the first set of regressions, we define Distance$_i$ as the number of years elapsed between the start of treatment and the player's birth for players born *after* treatment begins, capped at five years:

$$\text{Distance}_i^{\text{BornAfter}} = \min\Big\{5,\ \max\Big(0,\ B_i - T_{r(i)}\Big)\Big\}. \tag{5}$$

In the second set of regressions, we define Distance$_i$ as the number of years between the player's birth and the start of treatment for players born *before* treatment begins, capped at five years:

$$\text{Distance}_i^{\text{BornBefore}} = \max\Big\{0,\ \min\Big(5,\ T_{r(i)} - B_i\Big)\Big\}. \tag{6}$$

interested in post-educational outcomes, we include year-of-birth fixed effects instead of academic-year fixed effects.

We plot both distance functions in Figure 6. The measure $\text{Distance}_i^{\text{BornAfter}}$ equals zero for players born before treatment or in the year treatment begins, and increases linearly with time since treatment up to five years, after which it remains constant. Conversely, $\text{Distance}_i^{\text{BornBefore}}$ equals zero for players born after treatment begins or in the year treatment begins, and increases with the number of years prior to treatment (up to five years) for players born before treatment. For players born in never-treated regions, both $\text{Distance}_i^{\text{BornAfter}}$ and $\text{Distance}_i^{\text{BornBefore}}$ are set to zero, as no treatment occurs.

By construction, the two distance measures never overlap: individuals exposed before birth are never exposed after birth, and vice versa. This separation isolates the mechanism operating through birth timing (quarter of birth) from other channels.

The quarter-of-birth mechanism operates through $\text{Distance}_i^{\text{BornAfter}}$, as it captures variation in birth timing induced by the intervention. However, a significant relationship between $\text{Distance}_i^{\text{BornAfter}}$ and outcomes could also reflect other channels, such as changes in outside options or broader environmental conditions following the intervention.

In contrast, the quarter-of-birth mechanism does not operate through $\text{Distance}_i^{\text{BornBefore}}$, since these cohorts are born prior to the intervention. Nevertheless, other channels—such as changes in outside options—may still operate for these cohorts.

We therefore assess the relevance of the quarter-of-birth mechanism by comparing the relationship between distance and outcomes across the two specifications. Evidence of a statistically significant relationship between $\text{Distance}_i^{\text{BornAfter}}$ and outcomes, combined with the absence of such a relationship for $\text{Distance}_i^{\text{BornBefore}}$, is consistent with a mechanism operating through quarter of birth.

The primary object of interest is the coefficient on Distance_i, which captures how outcomes vary with the timing of birth relative to the intervention. The interaction terms allow this relationship to vary across quarters of birth. While not the focus of our analysis, they provide a useful check on whether the effects are concentrated in specific birth quarters.

Table 2 provides the results of the estimations. Across specifications, we incorporate additional sets of fixed effects.

TABLE 2. OLS results: Distance to treatment and log market values

Dependent variable: Log market value	(1)	(2)	(3)	(4)	(5)	(6)
Born after	-0.055	-0.060	-0.052			
	(0.060)	(0.067)	(0.067)			
Born after x Q1	-0.016	-0.021	-0.022			
	(0.038)	(0.037)	(0.038)			
Born after x Q2	-0.031	-0.032	-0.033			
	(0.045)	(0.044)	(0.044)			
Born after x Q3	0.002	0.001	0.000			
	(0.049)	(0.050)	(0.051)			
Born before				-0.065**	-0.093**	-0.103**
				(0.027)	(0.040)	(0.042)
Born before x Q1				-0.026	-0.024	-0.025
				(0.034)	(0.034)	(0.034)
Born before x Q2				-0.022	-0.025	-0.026
				(0.040)	(0.038)	(0.038)
Born before x Q3				-0.078*	-0.072*	-0.074*
				(0.042)	(0.041)	(0.041)
Observations	34,314	34,314	34,283	34,314	34,314	34,283
Adjusted R^2	0.067	0.069	0.061	0.067	0.070	0.062
Season FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth region FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth region x season FE	No	Yes	Yes	No	Yes	Yes
Linear region-cohort trend	No	No	Yes	No	No	Yes

Note: Estimation of Equation (4). Standard errors are clustered at the NUTS 2 level. The data used for the estimation covers the years 1985–2005.

Columns (1)–(3) present coefficient estimates for $\text{Distance}_i^{\text{BornBefore}}$, with Column (1) reporting the baseline specification. Columns (2) and (3) introduce additional controls to assess robustness. Columns (4)–(6) present coefficient estimates for $\text{Distance}_i^{\text{BornAfter}}$ following the same structure: Column (4) reports the baseline specification, while Columns (5) and (6) include additional controls.

Result 5. *The coefficient estimate for* $\textit{Distance}_i^{\textit{BornAfter}}$ *is negative and statistically significant across all specifications. Players conceived and born following the intervention exhibit systematically lower market values, and this result is robust to the inclusion of linear region-by-cohort trends as well as region-of-birth–by-season fixed effects. Depending on the specification each additional year of distance from treatment is associated with a 7–8 percent decline in market value, corresponding to a total reduction of approximately 20–30 percent over the full five-year window.*[20]

In contrast, $\textit{Distance}_i^{\textit{BornBefore}}$ *has no detectable effect on market values. This suggests that the observed decline cannot be attributed to outside-option mechanisms operating during childhood. Instead, the results are consistent with a mechanism operating through quarter of birth as the primary driver of the decline in market values.*

The heterogeneity analysis further indicates that assumign 10 percent significance level the decline is most pronounced among players born in Q3 (June–August), who are youngest within the cohort under a September 1 cutoff. These players face a higher selection threshold and are therefore more strongly selected on underlying ability. A reduction in their representation may lower the overall signal quality in the cohort, leading to a noisier selection process and contributing to the observed decline in market values.

[20] This effect should be interpreted with caution. Because the outcome variable is defined as log(1 + market value), the usual percentage interpretation is only approximate. For larger market values, the estimates correspond to roughly a 7–10 percent decline. For smaller market values, however, the interpretation is less direct, as the +1 term plays a larger role. We therefore refrain from making precise quantitative statements and instead emphasize the direction of the effect: the intervention is associated with a substantial decline in player market values.

While other prenatal mechanisms cannot be fully ruled out, the absence of any detectable post-birth effects, together with the timing of exposure and the fertility responses, points to birth timing and relative age as the dominant channel.

5. Conclusion

This paper studies how institutional birth cutoffs shape the allocation of talent in occupations characterized by high uncertainty and highly skewed returns, such as professional sports and creative industries.

Our identification exploits exogenous variation generated by the EU Structural Funds in England and Wales. Using recent advances in multi-period difference-in-differences methods (*e.g.*, Borusyak et al., 2024; Callaway and Sant'Anna, 2021), we show that the intervention reduced the share of summer-born children while leaving GDP per capita unchanged.

We then examine the lifetime outcomes of professional football players born before and after the intervention and document a decline in median player log market values. Given the absence of changes in local economic conditions, we attribute this decline to a season-of-birth mechanism. This pattern suggests that selection processes relying on early-life performance may overweight temporary physical advantages and underweight underlying talent. When the share of relatively younger players declines, the pool of candidates whose performance reveals high latent ability shrinks, leading to a lower average quality among selected individuals.

Supporting this interpretation, we show that summer-born players—who are relatively younger at the time of selection—outperform players born in other quarters, consistent with the relative age effect documented in the literature (Palacios-Huerta, 2025). We also show that for players born before the treatment, distance to treatment does not explain variation in log market values, whereas for players born after the treatment, log market value declines with the number of years since the intervention. These results point to mechanisms that operate prior to a child's birth—such as fertility timing—and rule out mechanisms that would operate after birth during the early formative years of a child's life. T

These findings contribute to the literature on talent allocation (Hsieh et al., 2019; Celik, 2023) by highlighting how seemingly innocuous institutional rules can generate persistent inefficiencies. More broadly, they suggest that similar mechanisms may operate in other tournament-style labor markets, where early selection decisions interact with temporary advantages to shape long-run outcomes.

From a policy perspective, our results point to the importance of reducing distortions created by rigid age-based cutoffs. Adjustments to evaluation practices—such as accounting for relative age or emphasizing long-term development—may help mitigate these inefficiencies and improve the allocation of talent.

One potential explanation for the observed quarter-of-birth effects is that youth coaches—often early in their own careers—face limited incentives to make unconventional selection decisions. Evaluations tend to rely heavily on observable characteristics such as relative age and physical development, which are associated with immediate performance advantages. When coaches face short-term performance pressures, they may favor safer, more immediately productive players rather than investing in later developers whose returns are more uncertain and realized only over the long run.

Addressing these distortions may therefore require adjustments to incentive structures. For example, longer-term contracts or mentorship arrangements pairing less experienced coaches with senior counterparts could encourage more forward-looking talent development strategies. Evaluation systems could also place greater weight on long-term player progression rather than short-term outcomes, tracking the number of players who advance to higher competitive tiers. In addition, performance metrics that account for relative age or biological maturation may reduce reliance on observable but developmentally transient characteristics. More broadly, institutional reforms—such as rotating cut-off dates across cohorts—could help mitigate the systematic advantages created by rigid age-based classification rules.

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Born Out-of-Season:

Talent Allocation and Economic Conditions

by Marta Boczoń and Battista Severgnini

Appendices for online publication

Appendix A. Tables and Figures

Table A1. Summary statistics

Player performance	Mean	Median	Standard deviation	Min	Max	N
Panel A: Per player						
# of seasons	11.4	11.0	4.4	2.0	23.0	3,006
Highest market value	2.4	0.2	8.7	0.0	150.0	3,006
Panel B: Per player per season						
# of games	19.1	18.0	16.5	0.0	68.0	33,169
# of minutes	1,368.7	1,112.0	1,315.0	0.0	5,610.0	33,169
# of goals	2.1	0.0	3.9	0.0	46.0	33,169
# of assists	1.4	0.0	2.4	0.0	23.0	33,169

Note: Per-season statistics denote players' performances in national leagues and domestic and international club competitions. Market values are expressed in millions of euros. The number of player-by-season observations is lower than 34,314 due to missing information on players' performance beyond club and season identifiers.

Table A2. UK NUTS classification (England and Wales)

NUTS characteristics	NUTS 1	NUTS 2	NUTS 3
Number of Regions			
England	9	33	139
Wales	1	2	12
Total	**10**	**35**	**151**
Average Population per Region (millions, 2000)			
England	5.47	1.49	0.35
Wales	2.91	1.45	0.24
Weighted Average	**5.21**	**1.49**	**0.34**
Average Land Area per Region (thousand km^2)			
England	14.5	3.9	0.9
Wales	20.7	10.4	1.7
Weighted Average	**15.1**	**4.3**	**1.0**

Table A3. OLS results: Birth conditions and number of games

Dependent variable:	GDP per capita in NUTS level:			Light
Number of games	1	2	3	intensity
	(1)	(2)	(3)	(4)
Q1 (Dec-Feb)	0.053*	0.050*	0.047*	0.081**
	(0.028)	(0.027)	(0.026)	(0.035)
Q2 (Mar-May)	0.093***	0.097***	0.091***	0.093**
	(0.023)	(0.028)	(0.030)	(0.038)
Q3 (Jun-Aug)	0.142***	0.147***	0.151***	0.117**
	(0.041)	(0.028)	(0.028)	(0.047)
Observations	33,169	33,169	33,169	15,731
Adjusted R^2	0.059	0.064	0.074	0.184
Season FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Birth region FE	Yes	Yes	Yes	Yes

Note: See notes to Table 1.

Table A4. OLS results: Birth conditions and minutes played

Dependent variable:	GDP per capita in NUTS level:			Light
Number of minutes	1	2	3	intensity
	(1)	(2)	(3)	(4)
Q1 (Dec-Feb)	0.031	0.026	0.028	0.051
	(0.031)	(0.029)	(0.026)	(0.034)
Q2 (Mar-May)	0.075**	0.079**	0.077**	0.079**
	(0.027)	(0.029)	(0.032)	(0.038)
Q3 (Jun-Aug)	0.115**	0.120***	0.122***	0.097**
	(0.043)	(0.029)	(0.029)	(0.046)
Observations	33,169	33,169	33,169	15,731
Adjusted R^2	0.057	0.062	0.073	0.171
Season FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Birth region FE	Yes	Yes	Yes	Yes

Note: See notes to Table 1.

Table A5. OLS results: Birth conditions and number of goals

Dependent variable:	GDP per capita in NUTS level:			Light
Number of goals	1	2	3	intensity
	(1)	(2)	(3)	(4)
Q1 (Dec-Feb)	0.067***	0.073***	0.058*	0.090**
	(0.014)	(0.024)	(0.030)	(0.046)
Q2 (Mar-May)	0.037	0.043	0.030	0.014
	(0.028)	(0.029)	(0.031)	(0.048)
Q3 (Jun-Aug)	0.118***	0.124***	0.130***	0.094*
	(0.035)	(0.036)	(0.034)	(0.054)
Observations	33,169	33,169	33,169	15,731
Adjusted R^2	0.017	0.020	0.035	0.088
Season FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Birth region FE	Yes	Yes	Yes	Yes

Note: See notes to Table 1.

Table A6. OLS results: Birth conditions and number of assists

Dependent variable:	GDP per capita in NUTS level:			Light
Number of assists	1	2	3	intensity
	(1)	(2)	(3)	(4)
Q1 (Dec-Feb)	0.061**	0.060*	0.049*	0.068*
	(0.026)	(0.030)	(0.026)	(0.034)
Q2 (Mar-May)	0.069***	0.074**	0.065**	0.069*
	(0.020)	(0.030)	(0.030)	(0.037)
Q3 (Jun-Aug)	0.122***	0.122***	0.122***	0.063
	(0.025)	(0.028)	(0.031)	(0.049)
Observations	33,169	33,169	33,169	15,731
Adjusted R^2	0.025	0.028	0.036	0.069
Season FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Birth region FE	Yes	Yes	Yes	Yes

Note: See notes to Table 1.

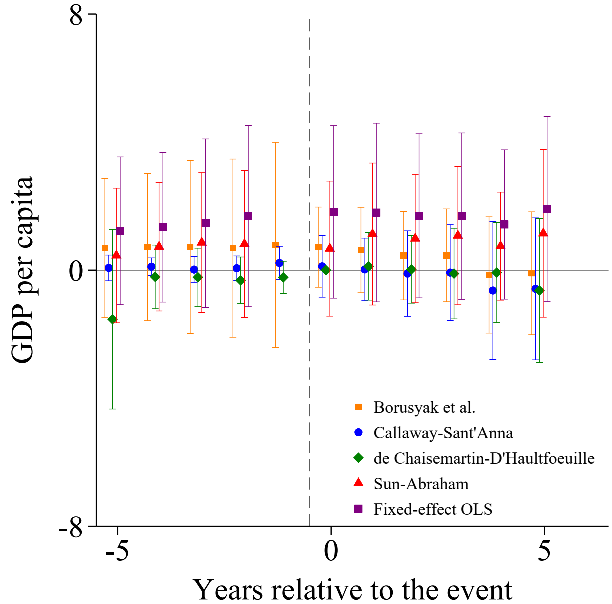
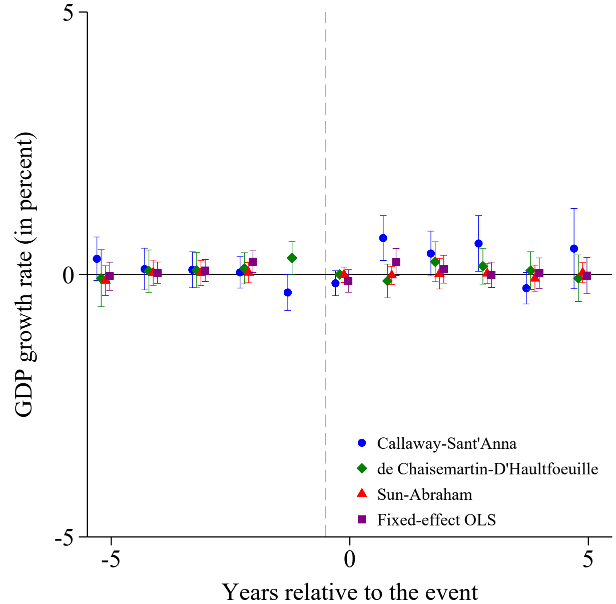


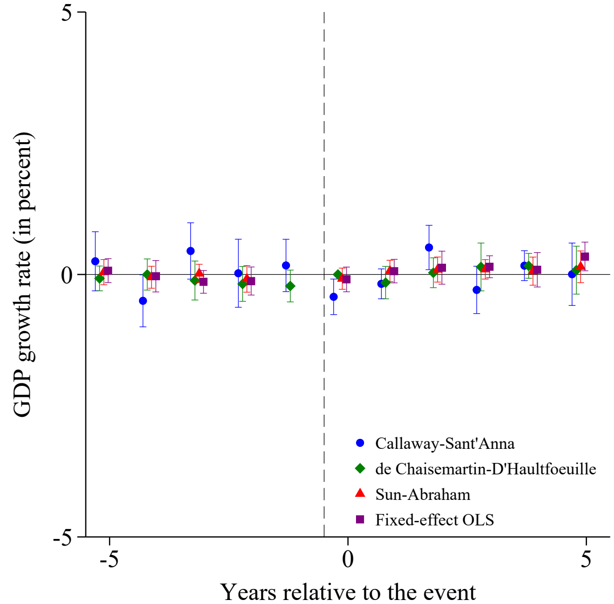
Figure A1. Impact of EU Structural Funds on GDP per capita

Note: Standard errors are clustered at the NUTS 2 level. The data used for the estimation covers the years 1985–2005. GDP per capita is expressed in 1,000 euros.

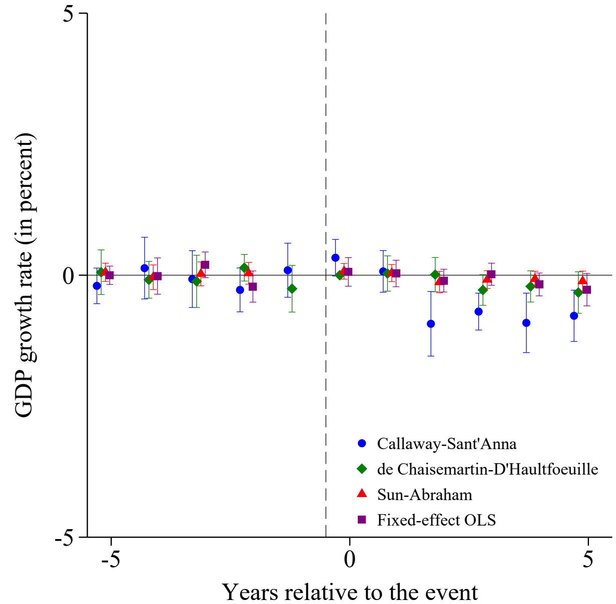
Figure A2. Impact of EU Structural Funds on quarter of birth



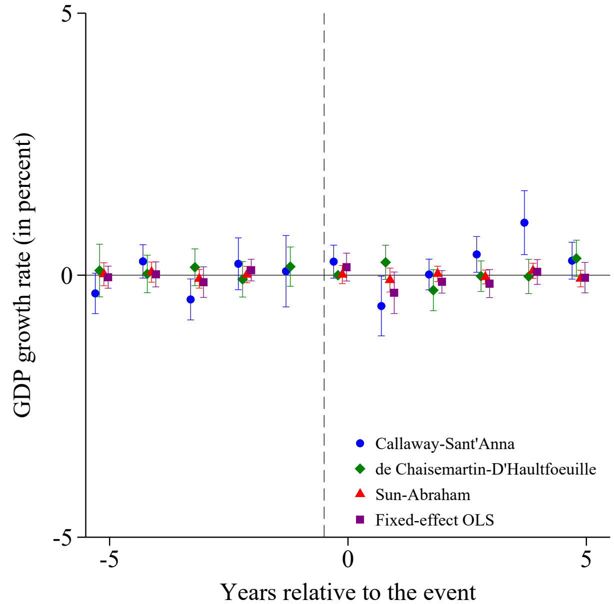
(a) December-February



(b) March-May



(c) June-August



(d) September-November

Note: Standard errors are clustered at the NUTS 2 level. The data used for the estimation cover the years 1985–2005.

Appendix B. Appendix: Selection, Birth Composition, and Conditional Talent

Let: S denote the event that a player is born in summer, W denote the event that a player is born in winter, P denote the event that a player is picked, and T denote the event that a player is talented.

We assume S and W form a partition of the population, i.e.,

$$\mathcal{P}(S) + \mathcal{P}(W) = 1. \tag{7}$$

The probability that a picked player is talented $\mathcal{P}(T)$ is given by

$$\mathcal{P}(T \mid P) = \frac{\mathcal{P}(T \cap P)}{\mathcal{P}(P)}, \tag{8}$$

which can be rewritten as:

$$\mathcal{P}(T \mid P) = \frac{\mathcal{P}(S)\mathcal{P}(T \mid S)\mathcal{P}(P \mid T, S) + \mathcal{P}(W)\mathcal{P}(T \mid W)\mathcal{P}(P \mid T, W)}{\mathcal{P}(S)\mathcal{P}(P \mid S) + \mathcal{P}(W)\mathcal{P}(P \mid W).} \tag{9}$$

Let $\bar{\mathcal{P}}$ denote the corresponding probabilities after the intervention.

We are going to make two assumptions, one that pertains to the distribution of talent across seasons and the other that pertains to the uniformity of the scouting mechanism.

Assumption 1. *The talent distribution of players is constant across seasons and time.*

Assumption 2. *The intervention did not change the season-specific selection mechanism of talented players.*

Both assumptions are plausible in our setting.

First, there is no reason to believe that innate talent differs systematically across birth seasons. Season of birth is orthogonal to underlying ability, and the intervention does not affect biological talent formation. Hence, assuming a constant talent distribution across seasons and over time is natural.

Second, the intervention did not modify the scouting technology, evaluation criteria, or selection thresholds. Scouts continue to assess players using the same performance metrics and physical benchmarks both before and after the intervention. Therefore, the probability that a talented player of a given birth season is selected remains unchanged.

Together, these assumptions imply that any change in the composition of selected players must operate through changes in birth-season composition rather than through changes in talent distribution or scouting behavior.

Under Assumption 1:

$$\mathcal{P}(T \mid S) = \mathcal{P}(T \mid W) = \bar{\mathcal{P}}(T \mid S) = \bar{\mathcal{P}}(T \mid W) := c \tag{10}$$

Under Assumption 2:

$$\mathcal{P}(P \mid T, S) = \bar{\mathcal{P}}(P \mid T, S) := a_S, \quad \text{and} \quad \mathcal{P}(P \mid T, W) = \bar{\mathcal{P}}(P \mid T, W) := a_W \tag{11}$$

Under Assumptions 1 and 2, $\mathcal{P}(T \mid P)$ and $\bar{\mathcal{P}}(T \mid P)$ can be rewritten as:

$$\mathcal{P}(T \mid P) = c\frac{\mathcal{P}(S)a_S + \mathcal{P}(W)a_W}{\mathcal{P}(S)\mathcal{P}(P \mid S) + \mathcal{P}(W)\mathcal{P}(P \mid W).} \tag{12}$$

$$\bar{\mathcal{P}}(T \mid P) = c\frac{\bar{\mathcal{P}}(S)a_S + \bar{\mathcal{P}}(W)a_W}{\bar{\mathcal{P}}(S)\bar{\mathcal{P}}(P \mid S) + \bar{\mathcal{P}}(W)\bar{\mathcal{P}}(P \mid W),} \tag{13}$$

respectively.

Since we do not observe changes in the birth-season composition among selected players,

$$\mathcal{P}(S \mid P) = \bar{\mathcal{P}}(S \mid P) \quad \text{and} \quad \mathcal{P}(W \mid P) = \bar{\mathcal{P}}(W \mid P), \tag{14}$$

the odds ratio

$$\kappa := \frac{\mathcal{P}(S \mid P)}{\mathcal{P}(W \mid P)} \tag{15}$$

is constant before and after the intervention. Bayes' rule implies

$$\kappa = \frac{\mathcal{P}(S)}{\mathcal{P}(W)} \cdot \frac{\mathcal{P}(P \mid S)}{\mathcal{P}(P \mid W)} = \frac{p}{1-p} \cdot \frac{b_S(p)}{b_W(p)}, \tag{16}$$

where $p := \mathcal{P}(S)$, $b_S(p) := \mathcal{P}(P \mid S)$, and $b_W(p) := \mathcal{P}(P \mid W)$.

Rearranging Equation (16) yields

$$b_S(p) = \kappa\, b_W(p) \frac{1-p}{p}. \tag{17}$$

Therefore,

$$f(p) = c \frac{pa_S + (1-p)a_W}{pb_S(p) + (1-p)b_W(p)} = c \frac{pa_S + (1-p)a_W}{(1-p)b_W(p)(\kappa+1)}. \tag{18}$$

Now, we take the first derivative of $f(p)$ with respect to p. Since

$$f(p) = c\, \frac{pa_S + (1-p)a_W}{(1-p)\, b_W(p)\, (\kappa+1)},$$

taking logs yields

$$\ln f(p) = \ln c - \ln(\kappa+1) + \ln\left(pa_S + (1-p)a_W\right) - \ln(1-p) - \ln b_W(p).$$

Differentiating with respect to p gives

$$\frac{d}{dp} \ln f(p) = \frac{a_S - a_W}{pa_S + (1-p)a_W} + \frac{1}{1-p} - \frac{b'_W(p)}{b_W(p)}. \tag{19}$$

The sign of the derivative is governed by two forces. The first term is increasing in $(a_S - a_W)$ and captures season-specific selection of talented players: if $a_S > a_W$, then talented summer-born players are more likely to be selected than talented winter-born players.

The second term, $1/(1-p)$, is mechanically positive. The final term reflects how the winter selection rate $b_W(p)$ varies with population composition. In particular, $\frac{d}{dp}\ln f(p) > 0$ whenever

$$\frac{b'_W(p)}{b_W(p)} < \frac{a_S - a_W}{pa_S + (1-p)a_W} + \frac{1}{1-p}.$$

Thus, provided $b_W(p)$ does not increase too rapidly with p (e.g., $b'_W(p) \approx 0$) and $a_S \geq a_W$, $f(p)$ is increasing in p. Consequently, a decline in the population share of summer-born players (p) reduces $\mathcal{P}(T \mid P)$ even if the selected birth-season composition (and hence κ) remains constant.

In practice, a particularly transparent case arises when winter selection intensity is approximately stable,

$$b'_W(p) \approx 0.$$

In that case, the condition simplifies to

$$f'(p) > 0 \quad \Longleftrightarrow \quad a_S > a_W. \tag{20}$$

This provides a simple sufficient condition: if talented summer-born players are more likely to be selected than talented winter-born players, then a decline in the population share of summer-born children reduces $\mathcal{P}(T \mid P)$, even when the selected birth-season composition remains unchanged.