Confidence enhanced performance
- Evidence from professional golf tournaments

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Abstract
This paper provides field evidence on the causal impact of past successes, which are assumed to build confidence, on future performances. Since persistence in success or failure is likely to be influenced by individual, potentially time-varying, heterogeneity it is intrinsically difficult to identify the causal effect of succeeding on the probability of succeeding again. We therefore employ a regression discontinuity design on data from professional golf tournaments exploiting that almost equally skilled players are separated into successes and failures half-way into the tournaments (the “cut”). We show that players who (marginally) succeeded in making the cut substantially increased their performance in subsequent tournaments relative to players who (marginally) failed to make the cut. This confidence-effect is substantially larger when the subsequent (outcome) tournament involves more prize money. The results imply that confidence is an important prerequisite when performing high-stakes tasks.

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JEL-codes: J24, L83, C93, M50

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1 Introduction

The determinants of success in different aspects of life constitute a key element within economics. As a consequence, economists have devoted considerable effort to quantifying the extent to which, e.g., education, experience, effort, cognitive and non-cognitive skills, beauty and height affect performance in various contexts such as schools and labor markets. One possible mechanism through which many of these factors may affect the ability to perform demanding or complex tasks is by altering the individual’s own perception of the ability to do so, i.e. through building confidence. In this paper we make the fundamental assumption that confidence is a function of past successes, and provide quasi-experimental field evidence on the effects of confidence, as induced by past successes, on future performance.

In the spirit of Compte and Postlewaite (2004), we interpret confidence as the perceived outcome of previous performances, and think of confidence as a mechanism which potentially can have a causal impact on future successes and failures. Conceptually, consider two identically skilled surgeons performing identical surgeries where one patient dies and the other one survives due to random chance. Arguably, the surgeon whose patient died will think of the event as a failure, whereas the other will think of it as a success. The question is if this will have a causal impact on their performances in the future. There is so far very little credible field evidence on the empirical relevance of this fundamental idea. A likely reason for the scarce set of previous evidence on the issue is that it is inherently difficult to analyze since those that perform well today also may perform well tomorrow, and vice versa, simply because they differ in the underlying ability to perform the task. More able individuals are likely to persistently perform better than less able individuals.

In this paper we use data from professional golf tournaments, relying on a special feature in these tournaments enabling us to perform a regression discontinuity (RD) analysis to identify the causal impact of succeeding in one tournament on the performance in the next.1 Midway through most of the professional golf tournaments on the European PGA Tour there is a qualification threshold (the cut) that a player must pass (make) in order to complete the tournament and earn prize money. Players around

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1 In using data from the world of sports to study fundamental economic processes we follow in a long line of previous papers. Ehrenberg and Bogmanno (1990a and 1990b), Orszag (1994) and Melton and Zorn (2000) all use data from golf tournaments to study the predictions of tournament theories and Pope and Schweitzer (2011) use data from golf tournaments to measure loss aversion.
the threshold have hence performed almost equally well but, arguably, when the players look back on their performance, those who barely passed will have a perception of success whereas those who barely failed will have a perception of failure. By studying the results in the subsequent tournament we are able to isolate the effect of present success on the probability of future success.²

The previous literature contains a number of studies where the assignment to success and failure is manipulated in a controlled laboratory setting. Most of these studies find that (perceived) successes tend to lead to improved subsequent performance while (perceived) failures deteriorate future performance (see, Bélanger et al., 2013, for a recent review). A number of relatively recent studies have also examined the importance of relative feedback information on subsequent performance, finding mixed results. Azmat and Iriberri (2010) and Tran and Zeckhauser (2012) both report that when individuals in a group receive information about their relative performance ranking in the group, the group as a whole performs better in the future. Since they find improvements in subsequent performance over the whole distribution, the results suggest that relative feedback information spur a general will to compete. Eriksson et al. (2009), on the other hand, find no general effect from relative performance feedback but instead document a reduction in the quality of low performers’ work. Along the same lines Murphy and Weinhardt (2013) find that students in English primary schools are sensitive to their local rank. They compare equally able students (same score on a normalized national test at the end of primary school) that differ with respect to their local rank. Students in the top of the performance distribution at their school outperform students with poor local rank in secondary school even though they had the same test score.³

A key difference between these studies and our set-up is that we isolate the impact of success in a setting where the subjects are fully informed regarding the underlying process, once success or failure has been determined. Players know how many strokes they used, what their final rank became, and whether they passed the cut or not. Thus,

² Making the cut improves the ranking of a player compared to failing to make the cut; but this ranking has, unlike in some other sports (e.g. tennis), no direct benefits in the upcoming tournament.
³ A related literature studies the importance of “stereotype effects”, where subjects’ performance on tests are found to be affected by information regarding the average performance among people with similar characteristics as themselves, see e.g. Cadinu et al. (2003), Cadinu et al. (2005) and Aronson et al. (1999).
scoring one stroke less to pass the cut is not more informative regarding the own performance than scoring one stroke less to get closer to (or further from) the cut.\footnote{Notably, Compte and Postlewaite (2004) show that it can indeed be rational to have biased recollections of previous performances as long as performance is directly affected by (the perception of) previous performances.}

Empirically, we are perhaps closest in spirit to the study of “hot hand” effects within sports economics, i.e. the notion that athletes’ performances during certain periods are significantly better than otherwise; see e.g. Livingston (2012) for a recent example using data from golf tournaments, Crust and Nesti (2006) for a slightly older review and Rabin and Vayanos (2010) for a theoretical discussion. We do, however, differ from this literature by focusing more on processes beyond the immediate state of mind and by focusing on the causal component of “non-random” streaks of subsequent successes. In short, although the hypothesis that confidence affects performance predicts the presence of non-random streaks, the mere existence of such streaks can also be explained by exogenous time-varying factors which affect performance during subsequent events.

To preview our results, we show that players just above and below the cut indeed are comparable in terms of predetermined characteristics (conditional on our RD model) allowing us to infer the causal impact of the initial success on future outcomes. We further show that making the cut in a tournament has a large and statistically significant positive causal effect on the outcomes in the subsequent tournament. The number of strokes after two rounds falls by a quarter of a stroke and the probability to make the cut in the following tournament increases by 3 percentage points from a baseline of 50 percent. This is a sizable effect in relation to other variables in the data such as years of professional experience and the average score per round the previous year.

Apart from the effect on confidence that we are interested in, passing the cut also entails prize money and the opportunity to continue playing in the initial (“treatment”) tournament. In order to investigate if these factors are important we have analyzed if the effects vary depending on the stakes (prize money) in the treatment tournament and subsequent (“outcome”) tournament played during the week after. The results show that the effects are independent of the stakes in the treatment tournament, suggesting that financial rewards are of little importance. However, consistent with the notion of a
psychological mechanism, we find that the benefits of past successes are confined to the cases when the stakes in the outcome tournament are high.⁵

In general, the implied relationship between success, confidence and performance suggests that transitory events of luck or misfortune will have lasting effects on future performances, which is important for our interpretation of observed differences in economic outcomes. In addition, our results suggest that confidence and performance are reinforcing each other which would imply that performances later in life will be a function of initial confidence. Given that the literature suggests that confidence is positively correlated with social background (see e.g. Twenge and Cambell, 2002, for a review), this indicates that confidence may contribute to observed intergenerational rigidities in social mobility.⁶ Finally, the proposed process implies that confidence will be a mechanism which reinforces the usefulness of other abilities later in life, suggesting an alternative explanation for the changing returns to cognitive and non-cognitive abilities over the life cycle.⁷ Notably, some of our evidence also indicates that the effect of confidence is particularly pronounced amongst players with better endowments (lower score average in the year before).

The rest of the paper is structured as follows. In section 2 we present a simple theoretical model which rationalizes our empirical set up. Section 3 explains the empirical setting and describes the data. Section 4 deals with the validity of the empirical strategy, shows the baseline results and analyses the role of the involved stakes. Section 5 concludes.

2 Theory and Empirical Strategy
Here we outline a stylized theoretical model of a mutual relationship between performance and confidence. The purpose of the model is primarily to motivate the empirical set-up.

2.1 Theoretical model
We borrow our basic assumptions from Compte and Postlewaite (2004) who put the notion that confidence can affect performance into a formal model. They build their

⁵ Making the cut can also give a player additional positive publicity and a feeling of being on a lucky streak, we interpret any positive effects this entails as being part of the overall impact of confidence.
⁷ See, e.g., Altonji and Pierret (2001) regarding how returns to cognitive skills change with labor market experience and Lindqvist and Vestman (2011) for a discussion on the wage returns to cognitive and non-cognitive abilities.
model around two broad ideas. The first is that individuals find it easier to recollect good memories (successes) than bad memories (failures) and that individuals tend to attribute successes to their own performance and failures to various exogenous circumstances. The second notion is that emotions and stereotypes affect performance. Building on these ideas, they construct a model where performance is a function of confidence and confidence in turn is a function of the “perceived empirical frequency of success” and show that it is welfare enhancing to have a positively biased perception of previous performances if confidence affects performance.

Following Compte and Postlewaite (2004) we assume that higher confidence ($C$) will increase performance, which for practical reasons we formulate as a reduction in the number of mistakes ($Z$). In addition, we assume that individuals differ in their abilities ($A$) and that the frequency of mistakes also has a random orthogonal component ($\varepsilon$) with mean zero. Hence, consider the following two-period model of mistakes:

$$Z_{it} = \alpha_t + \beta A_i + \gamma C_{it} + \varepsilon_{it}, \quad (1)$$

where $t = 1, 2$. Since $Z$ is the number of mistakes we expect that $\beta$ and $\gamma$ are negative. It should be evident that the model includes two measures of fundamentally unobservable concepts (ability and confidence) which both will provide a reason for intertemporal correlations in the number of mistakes, and that an empirical separation of the impact of these two variables is intrinsically difficult. To mimic situations such as when a player makes (or misses) the golf cut, a surgeon’s patient survives (or dies) during surgery, a student passes (or fails) his entrant exam, or a researcher gets his paper accepted (or rejected), we assume that performance leads to success ($S = 1$) when the number of mistakes underscores a threshold ($T$), but leads to failure ($S = 0$) otherwise. Thus:

$$S_{it} = I(Z_{it} \leq T_t) = I(\alpha_t + \beta A_i + \gamma C_{it} + \varepsilon_{it} \leq T_t), \quad (2)$$

where $I(.)$ is an indicator function taking the value 1 if the argument is true and zero otherwise. Further, again following Compte and Postlewaite (2004), we assume that a
success will increase the agent’s level of confidence in the next period (i.e. we expect $\mu$ to be positive) and therefore that confidence evolves according to:

$$C_{i2} = C_{i1} + \mu S_{i1} - \mu/2$$

Hence, the performance in the second period can be rewritten as:

$$Z_{i2} = \alpha_2 + \beta A_i + \gamma C_{i1} + \gamma \mu I(\alpha_1 + \beta A_i + \gamma C_{i1} + \epsilon_{i1} \leq T_1) - \gamma \mu/2 + \epsilon_{i2}$$

Three things are worth noting from equation (4). First, it implies that initial confidence ($C_{i1}$) effects the performance in the second period. Second, since the success function (2) propagates the impact of ability, the unconditional returns to ability are higher in the second period than in the first period due to the confidence effect. Finally, note that transitory lucky circumstances in the first period (i.e., $\epsilon_{i1}$) improves performance in the second period.

### 2.2 Empirical identification

For empirical purposes it is convenient to rewrite the model as:

$$Z_{i2} = Z_{i1} + (\alpha_2 - \alpha_1) + \gamma \mu I(Z_{i1} \leq T_1) - \gamma \mu/2 + (\epsilon_{i2} - \epsilon_{i1})$$

Equation (5) is a model that can be estimated directly from data. With this structure it is evident that we can think of the first period as the treatment period, and the second period as the outcome period, where the (potential) success in the treatment period is allowed to have a causal impact on the performance in the outcome period corresponding to $\gamma \mu$. Once we control for the linear impact of previous mistakes, the impact of making the threshold can be interpreted as the causal effect of succeeding. This argument highlights our general strategy for separating the impact of ability from the impact of success, but it crucially relies on the linear structure of the model. A more flexible identification can be achieved by focusing the identification on the agents closest to the threshold.

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8 Assuming that the probability of success is one half, as in our empirical application, this formulation ensures that the average confidence level is stable.
Relying on the set of agents at the margin of the threshold in the treatment period allows us to separate out the impact of success from the impact of innate ability, using a regression discontinuity (RD) approach (see e.g. Lee and Lemieux, 2010 for a detailed discussion), without relying on the linear structure of the model. Conceptually, the RD-design serves as a comparison of outcomes between players that are placed infinitely close to a threshold on the two sides. As long as the impact of potential confounders (i.e. other factors relating scores and outcomes) are continuously distributed around the threshold, these infinitely close players should be identical in all confounding aspects. In practice, all RD-designs use data from a (narrow) data window around the threshold to generate separate predictions from each side of the threshold. The differences between these predictions provide estimates of the causal impact of passing the threshold regardless of the nature and functional forms of other confounding factors, as long as these are smoothly distributed. Hence, the RD model is identified even if (e.g.) A and Z are not linearly separable (see e.g. Hahn et al, 2001). In our empirical analysis, we follow common practice and identify the effect of success on performance by running a pooled regression with separate linear terms on each side of the threshold within a narrow data window which we vary across specifications. Formally, we estimate:

$$Z_{i2} = \beta_0 + \beta_1 I[Z_{i1} \leq T_1] + \beta_2 (Z_{i1} - T_1) + \beta_3 I[Z_{i1} \leq T_1] * (Z_{i1} - T_1) + u_{i2} \quad (6)$$

where $\beta_1$ denotes the impact of past successes. Note that the practice of running local (i.e. within a narrow data window) linear regressions does not mean that we assume linearity of the underlying model. Instead, it is a straightforward way of approximating the ideal conceptual RD-comparison between observations infinitely close to the threshold on the two sides. Thus, while estimation of Equation (5) would rely on the additive separability between ability and confidence imposed in equation (1), estimates from Equation (6) can be given a causal interpretation under any continuous functional form assumption. The key assumption is continuity, which requires that there is no perfect sorting around this threshold (see e.g. Lee and Lemieux, 2010). In the empirical section below, we show a number of tests of this assumption.
3 Institutions and Data

3.1 European professional golf tournaments

Our paper relies on data from professional golf tournaments. These tournaments are well-suited for the RD approach outlined above since they use a cut midway through which effectively sort players into elimination or qualification based on their number of strokes. Players close to the threshold thus performed almost equally well, but some was assigned a perception of failure and some the perception of success. Below follows a more detailed description of the institutions of professional golf tournaments which clarifies our argument.

The two major male professional golf tours in the world are the U.S. PGA Tour and the European PGA Tour. In this paper we employ data from the male European PGA Tour. Every year about 50 tournaments are played on this tour; that is, virtually every week a tournament is being played. Counterintuitive to the name of the Tour, the tournaments are not only played in Europe but all over the world; for example in South Africa, New Zealand, Australia and China. The typical tournament has about 140 participants and is decided over four days of play, starting on a Thursday and ending on a Sunday, with 18 holes played each day. Based on the players’ score after two days (36 holes) a qualification score called the “cut” is determined. From the season of 2006, all players that tie the 65th place (70th place during earlier seasons) or have a better position are qualified to play over the weekend whereas the other players are eliminated from the tournament. Players failing to make the cut will not earn any prize money whereas all players that do make the cut will receive a prize come Sunday afternoon, the amount being determined by their final position.

We find it reasonable to assume that making the cut can be described as a success, and failing to make the cut as a failure. There will only be a one stroke difference between players just making the 36-hole cut and players just failing to make the cut; hence one stroke separates success from failure. The existence of the cut thus gives us a situation where individuals performing almost equally well will record the tournament differently in terms of success and failure.

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9 www.europeantour.com
3.2 Data

Our data are drawn from tournaments between the start of the 2000 season and the 1:st of April 2012. Using these data we define empirical counterparts to the theoretical model outlined in section 2. The number of strokes after two days of playing serves as the empirical measure of the number of mistakes \((Z)\). The cut, separating players after two days, corresponds to the threshold \(T\). Thus, success \(S\), for a player, is defined as the case when the number of strokes is at least as low as the cut in the relevant tournament.

To mimic the two-period structure of the model, we use data on the number of strokes (after two days) in a tournament which we use to measure period 1 performance. We then use data from the tournament played the week after as our measure of the outcome in period 2. An observation in our data thus pertains to a tournament pair consisting of the number of strokes in a “treatment” (period 1) tournament and the number of strokes in an outcome (period 2) tournament.

For each treatment tournament we have collected data on players within a six stroke window (on either side) of the cut. Throughout, we use the normalized scores (corresponding to \(Z_{i\tau} - T_{\tau}\)) which therefore range from -5 to 6 at most. In addition to the number of strokes/mistakes \((Z)\), we use the probability of passing the cut in the outcome tournament as an alternative measure of performance.

Two potential concerns arise because of the institutional set-up. The first is that we need to handle the fact that thresholds are tournament specific. In order to address this, we enrich Equation (6) by including tournament specific fixed effects \((\delta_{\tau})\) for each pair \((\tau)\) of a treatment and an outcome tournament. Thus, using \(Y\) to denote outcomes (number of strokes, or a dummy for passing the cut, in the outcome tournament) our final empirical model can be written as:\(^{10}\)

\[
Y_{i\tau} = \beta_0 + \beta_1 I[Z_{i\tau} \leq T_{\tau}] + \beta_2 (Z_{i\tau} - T_{\tau}) + \beta_3 I[Z_{i\tau} \leq T_{\tau}] * (Z_{i\tau} - T_{\tau}) + \delta_{\tau} + u_{i\tau}
\]  

The second possible concern is that not all players from the treatment tournament participate in the outcome tournament. To minimize our exposure to selective dropouts, we focus the analysis on tournament pairs where the participation rate in the outcome

\(^{10}\) We also present results from models where we allow tournament specific slopes above and below the thresholds.
tournament is at least 60%. We further address the issue of potential systematic selection into outcome tournaments at length below.

3.3 Descriptive statistics
Table 1 provides descriptive statistics for the sample used in our later analysis. Our used sample includes 16,515 observations. Note that players with a result equal to the cut also pass the cut, which explain why the normalized strokes on average have a one stroke lower absolute value amongst those passing the cut. A crude comparison between successful (made the cut) and unsuccessful (did not make the cut) players reveals that successful players have fewer strokes (and make the cut to a greater extent) also in the outcome tournaments. This is natural even without a confidence effect since we expect the successful players to be inherently better than their unsuccessful counterparts, i.e. good players persistently perform well and bad players persistently perform badly ($A$ in the model). The difference in ability between the two groups is also reflected in the fact that successful players had a lower stroke average in the previous year.

4 Validity and Results
In this section we will first assess the robustness of our empirical strategy. After discussing the validity of the identification we present the main results.

4.1 Validity of the empirical strategy
We are primarily concerned with two issues which potentially could confound the causal interpretation of our final estimates. Firstly, since it is important for players to make the cut the assignment to success and failure at the threshold might not be entirely exogenous. The identification of the effect of interest will be confounded if players can push themselves to exactly match the threshold in order to end up on the right side of the cut (as this would mean that players no longer will have equal abilities on each side of the threshold). Secondly, since not all players participate in an outcome tournament, we also worry that the passing the cut in the treatment tournament influences the decision to participate in the outcome tournament.

Figure 1 addresses the first concern. The left-side panel shows the distribution of players around the cut in the treatment tournaments. Naturally, there are a lot of

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11 Our data are collected manually from the tour web page. A detailed protocol for the tournament selection process is available in Appendix A. All the micro data and a complete list of tournaments are available on demand.
observations close to the threshold since this is defined by the median score. This implies that many players barely made the cut. As shown in the right-side panel, however, the relative difference in the number of observations between scores 1 and 0 fall well within the normal range.\textsuperscript{12} The figures thus imply that there are roughly as many players that just miss the cut as there are players that just miss the cut.

Figure 2 addresses the second concern. The figure clearly shows that there is systematic selection into the outcome tournament amongst the poor performing players, but this selection is only related to the average score, and not related to whether or not the player succeeded in making the treatment tournament cut or not. The gradually lower participation rate among relatively poorly performing players is most likely due to the fact that some tournaments invite a few local players to attend through special invitations. On average these players are likely to perform worse and they do not, in general, have the right to attend the outcome tournament. Leaving the general trend aside we conclude that the participation rate seems to evolve smoothly over the cutoff.\textsuperscript{13} This indicates that attrition is not a problem in our case.

As a further specification test, we expect predetermined individual characteristics to, on average, be identical on the respective sides of the threshold if our empirical strategy is valid. Differences in characteristics between treated and untreated players at the cut in the treatment tournament constitute a violation of our main identifying assumption of equal abilities on either side of the threshold and could therefore potentially be a threat to causal interpretations of the treatment effect. Figure 3 and Figure 4 below investigate this concern using data on the average scores-per-round during the preceding year and the number of years as a pro (experience) within the sample of players actually participating in an outcome tournament (i.e. the used sample). Figure 3 shows that poorly performing players had a higher stroke average in the preceding year. Reassuringly, however, marginal players on either side of the threshold had almost exactly the same stroke average the preceding year. Interpreting the stroke average during the preceding year as an indicator of ability, the picture is very much in line with our stylized model which presumes that ability matters linearly, but also that ability is equally distributed at the threshold. Figure 4 shows that the variance in performance

\textsuperscript{12} This impression also holds if we focus on absolute rather than relative differences, despite the obvious bias towards larger numbers in the center of the distribution in that case.

\textsuperscript{13} As should be evident from the figure, the jump at the threshold is not statistically significant.
seems to diminish with experience. For our purposes, however, the crucial element is that experience is equal on either side of the threshold. In summary, these validity checks are fully consistent with the notion of a smooth distribution of confounding factors around the threshold.

4.2 Main results

Turning to the main results, we start by showing the results in terms of figures, and then move on to a more formal analysis. We start by analyzing the effect on the probability of making the cut in the outcome tournament. This effect is displayed in Figure 5. As a contrast, the figure also shows the probability of having made the cut in the tournament before (thus, in effect an additional specification test). If the assignment to treatment is truly exogenous we should not see any effect on the rate of lagged successes whereas if the treatment (success) really matters we are to see an effect on the success rate in the outcome tournament. Clearly, Figure 5 supports both the empirical strategy and the theoretical prediction since players around the threshold are similar with respect to previous performance (left-side panel), but differ with respect to subsequent performance (right-side panel). In essence Figure 1 tells us that the treatment effect observed in the right panel is not due to the fact that some players consistently (marginally) make the cut while some players consistently (marginally) fail to make the cut. Instead, marginally making the cut appears to generate a genuine performance improvement as compared to marginally failing to make the same cut. The right-side panel suggests that the event of making the cut in the treatment tournament increases the probability of making the cut by about 3 percentage points (corresponding regression estimates are found in the table below). Considering a baseline probability of around 50 percent, we interpret this as a fairly substantial effect.

As an alternative outcome, which is closer in spirit to our model, we use the number of shots after two rounds in the outcome tournament normalized by the subtraction of the cut in the relevant tournament. (i.e. when the cut is set). The results are shown in Figure 6. Clearly, the more shots a player had in the treatment tournament the more shots he had in the outcome tournament (again, roughly consistent with a linear, time constant, impact of ability A). However, there is also a visible performance gap at the threshold. Those marginally making the cut outperformed those marginally failing by about 0.25 shots after two rounds in the outcome tournament after accounting for the
linear terms. We interpret this result as suggesting that players marginally making the
cut gained confidence from this relative success, and that this additional confidence
allowed these marginally successful players to outperform the marginally unsuccessful
players in the outcome tournament. Note also that a one stroke decrease in the number
of strokes after two rounds in the treatment tournament is associated with a drop of only
about 0.2 strokes after two rounds in the outcome tournament. This indicates that there
is substantial randomness in the number of strokes, i.e. it is hard to predict the
individual results in a tournament given the number of strokes in the preceding
tournament. Unlike many other assignment variables used in the RD literature (e.g. test
scores) it therefore seems that the number of strokes has a large random component
which attenuates its relationship to underlying ability, making it difficult for players to
perfectly manipulate their score relative to threshold.

In Table 2 we provide a more formal analysis. The top panel shows the impact on the
number of strokes in the outcome tournament (after two rounds), and the lower panel
the impact on the probability of passing the cut in the outcome tournament. We focus
our discussion on the former but the results are qualitatively very similar. Throughout,
tournament fixed effects are included and all standard errors are clustered at the strokes-
times-tournament level and robust to heteroscedasticity. In the first column, we account
for the normalized assignment variable in a linear form that is constrained to be the
same on the two sides of the cut (corresponding to a simplified version of Equation 7).

As is evident from the table, making the cut in the treatment tournament decreases
the number of shots after two rounds in the outcome tournament by 0.246 shots and
increases the probability of passing the cut by 3.23 percent. Both of these effects are
statistically significant on the 5 % significance level. In column (2) we allow the
normalized assignment variable to affect the outcome differently on the two sides of the
cut (as in the figures and as in Equation 7). The effect then gets slightly larger in
absolute terms. In the third column we also add control variables. These include number
years as professional player, the stroke average per round the previous year and a
dummy for making the cut in the immediately preceding tournament alongside a set of
dummy variables capturing cases with missing information on each of these variables.\footnote{\textsuperscript{14}}

\footnote{\textsuperscript{14} See Table 1 above for frequencies of missing data.}
As expected from our specification tests, the inclusion of the control variables does not affect the estimates in any substantial way.

In column (4) we use the same model as in column (3) but with a more limited (narrower) sample. Instead of using players ranging from -5 to 6, we use players ranging from -4 to 5. Hence, we compressed the sample towards the threshold. The purpose of this exercise is to test the robustness of the result and to rule out the risk of the result being driven by non-linear effects of the assignment variable. As expected from the smooth patterns shown in the figures, the result in column (4) is virtually identical to the previous columns. Column (5) pushes this exercise even further along these lines, now using a sample of players ranging from -3 to 4. Here, we see that the estimate gets marginally smaller in absolute terms although the main difference is that the standard error gets larger due to the reduced sample. However, it still remains significant at the 10 % level. Finally, in column (6) we estimate the model using tournament-specific slopes above and below the thresholds. Again, the estimates remain largely unchanged. Our overall interpretation from these exercises is that the results are quite robust to fairly large variations in the estimated model. Players that marginally succeed to make the cut thus appear to substantially outperform players that marginally fail to make the cut in the subsequent tournament.

4.3   The role of tournament stakes

Above, we showed evidence suggesting that confidence matters for performance. A potential concern regarding the interpretation of the estimates is that passing the cut also entails prize money which possibly could have an independent effect on the outcome. In addition, passing the cut allows the player to continue playing for two additional days which, potentially, could have an additional effect on the performance in the subsequent tournament. Although it is not clear whether these mechanisms should entail a positive or a negative bias to the estimates, they remain as a source of uncertainty regarding the exact interpretation of the estimates. In order to investigate their potential importance, we use data on the prize sums in the studied tournaments.

We separate between high and low stakes tournaments by defining a high stakes (HS) tournament as a tournament in which the prize money is larger than the median prize money for that season. We repeat this categorization separately for treatment and
outcome tournaments. Our first conjecture is that we should see larger effects for high stakes treatment tournaments if prize money had an independent effect on the outcomes.

A complication is that the stakes in the treatment and outcome tournaments are correlated. Thus, we need to analyze the impact of treatments within a joint framework. To this end, we estimate a separate regression (corresponding to equation 7) for each tournament pair, and then use these tournament specific estimates as the outcomes in regressions where we explain the estimated effects with two indicator variables, one capturing high treatment-stakes and one capturing high outcome-stakes.\textsuperscript{15} The results are shown in Table 3. The results first show that the stakes in the treatment tournament is unrelated to the effect of interest, which suggest prize money is an unlikely mechanism behind the main results (in terms of Equation (3), this implies that $\mu$ is independent of the stakes in the treatment tournament).

However, the effect is found to be substantially larger for outcome tournaments with high stakes. This seems to suggest that confidence is crucial in high-pressure situations (i.e. that $\gamma$ in Equation (1) is an increasing function of the involved stakes). We also find this result reassuring since we conjecture that the impact of additional practice during the continued treatment tournament should be relatively independent of the stakes in the outcome tournament, whereas it seems more likely that the role of confidence is more pronounced when stakes are high.

\textsuperscript{15} Notably, the average of the underlying estimates of this analysis is closely related to column 6 of table 2 since we use tournament-specific assignment-variable effects in both cases.
4.4 Confidence effects for different types of players

As a final exercise, we have explored the extent to which the effects differ depending on the characteristics of the players. We characterize players in three dimensions using a dummy for experienced players (years as pro above the median), a dummy for stroke average above median in the previous year and a dummy indicating if they managed to pass the cut in the last tournament they played. For each of these dummy variables, we display estimates for players having the values one and zero respectively, and also display the difference with standard errors. These estimates are derived from expanded versions of the model shown in Equation 7 (as in column 3 of Table 2). The expansions build on interactions between the main variables and the dummy for the relevant characteristic ($D_{i}^{\text{Char}}$). Formally:

\[
Y_{it} = \beta_0 + D_{i}^{\text{Char}} \{ \beta_1^1 I[Z_{it} \leq T_1] + \beta_2^1 (Z_{it} - T_1) + \beta_3^1 I[Z_{it} \leq T_1] * (Z_{it} - T_1) \} + \\
+ (1 - D_{i}^{\text{Char}}) \{ \beta_1^0 I[Z_{it} \leq T_1] + \beta_2^0 (Z_{it} - T_1) + \beta_3^0 I[Z_{it} \leq T_1] * (Z_{it} - T_1) \} + \\
\delta_t + u_{it}. \quad (8)
\]

The results are displayed in Table 4 with indicators for weak players (inexperienced, high stroke average and missed the previous cut) on the top row. As is shown, the point estimates suggest that the effects are more pronounced for the relatively stronger players. Precision is, however, clearly an issue for this exercise and only the differences between players with a high and a low stroke average is statistically significant.

Although the lack of precision clearly suggests that we should interpret these estimates with great care, they do seem to suggest that the effect of confidence, if anything, is larger when the initial endowment of ability (and confidence) also is large. If this indeed is the case, then it is reinforces the distributional impact of confidence effects that we derived in the stylized theoretical model (where endowments and effects were assumed to be additively separable).
5 Conclusions
In this paper we have used data from professional golf tournaments to analyze the joint conjecture that success breeds confidence and that confidence in turn influences future performance. Using an RD design, we are able to isolate the causal impact of present success on future performance, and our results indicate that these effects are indeed substantial. Passing the cut in one tournament is found to causally increase the probability of doing so in the next tournament as well by 3 percentage points from a baseline of about 50 percent. We also show that confidence matters the most when a player is competing in a high stakes tournament.

As we show in a very stylized model the propagating role of confidence has far-reaching consequences since both initial confidence (which has been suggested to be positively correlated with social background, see e.g. Twenge and Cambell, 2002) and early luck will affect future performance. Furthermore, confidence provides a mechanism through which the importance of ability will across time, thus providing a rationale for growing returns to ability over the life course (see e.g. Altonji and Pierret, 2001).
References


Appendix A: Protocol for selection of tournaments

1. We first collect data from a given “treatment” tournament. We then follow the players and record their results in the subsequent, “outcome” tournament. The subsequent tournament is the tournament played during the weekend directly following the treatment tournament.

2. We have collected data from tournaments during the time period between the start of the 2000 season to the 1:st of april 2012.

3. All players participating in the treatment tournament will not participate in the outcome tournament. To minimize selection problems, we place 5 restrictions on our used data (labeled A to E below). Data for tournaments excluded through restrictions A to D have not been collected at all.

A) The treatment tournament and the outcome tournament should be played in the same geographical region. The relevant regions are Europe (including Morocco), South Africa, Australia and New Zealand, United Arab Emirates Qatar and Bahrain, India, China and Hong Kong, Thailand Malaysia Indonesia and Singapore, Russia, South Korea.

B) The treatment tournament and the outcome tournament should both be “Normal” tournaments. This excludes Majors, “World tournaments”, Match tournaments, Team tournaments, Low status tournaments, tournaments without cuts and Alfred Dunhill Links Championship (played every fall in Scotland). Tournaments that are Match, Team and No cut type of tournaments cannot be used since they do not produce results in the normal sense. Tournaments that are Major, World and Low status type of tournaments typically contain different players than a normal tournament. We have also chosen to exclude the Alfred Dunhill Links Championship since it has a cut after three rounds and contains a Pro-Am element. By choosing two subsequent normal tournaments the chance of obtaining two similar entry lists in the tournaments is reasonably high.

C) On some occasions two tournaments are played in the same week. These data are excluded.

D) The outcome tournament should be played in the week directly following the treatment tournament.

E) We exclude tournaments where less than 60 percent of players appear in the outcome tournament.
### Tables and Figures

**Table 1:** Descriptive statistics for the used sample

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>By treatment</th>
<th>tournament results</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Treatment tournaments</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average cut ((T))</td>
<td>144.0</td>
<td>144.0</td>
<td>144.0</td>
</tr>
<tr>
<td>Normalized strokes ((Z-T))</td>
<td>0.249</td>
<td>-2.053</td>
<td>3.023</td>
</tr>
<tr>
<td>Made the cut ([I(Z \leq T)])</td>
<td>0.546</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td><strong>Outcome tournaments</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average cut</td>
<td>143.6</td>
<td>143.6</td>
<td>143.6</td>
</tr>
<tr>
<td>Strokes relative to the cut</td>
<td>0.323</td>
<td>-0.189</td>
<td>0.941</td>
</tr>
<tr>
<td>Made the cut</td>
<td>0.549</td>
<td>0.594</td>
<td>0.495</td>
</tr>
</tbody>
</table>

**Player characteristics**

<p>| | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Years as pro</td>
<td>11.2</td>
<td>11.2</td>
<td>11.2</td>
</tr>
<tr>
<td>...nonmissing</td>
<td>0.954</td>
<td>0.962</td>
<td>0.944</td>
</tr>
<tr>
<td>Stroke average in previous year</td>
<td>72.0</td>
<td>71.9</td>
<td>72.1</td>
</tr>
<tr>
<td>...nonmissing</td>
<td>0.978</td>
<td>0.982</td>
<td>0.973</td>
</tr>
<tr>
<td>Made the cut in previous tournament</td>
<td>0.554</td>
<td>0.578</td>
<td>0.525</td>
</tr>
<tr>
<td>...nonmissing</td>
<td>0.876</td>
<td>0.881</td>
<td>0.870</td>
</tr>
<tr>
<td>Number of tournaments</td>
<td>189</td>
<td>189</td>
<td>189</td>
</tr>
<tr>
<td>Number of clusters (tournament/strokes)</td>
<td>2,257</td>
<td>1,132</td>
<td>1,125</td>
</tr>
<tr>
<td>Number of observations</td>
<td>16,515</td>
<td>9,025</td>
<td>7,490</td>
</tr>
</tbody>
</table>

Note. Sample includes players within 6 strokes of the cut, i.e. with strokes relative to the cut in the \([-5,6]\) interval. The cut is defined by the maximum number of strokes allowed for players to proceed in the tournament. This implies that values of strokes relative to the cut will be one stroke higher on average (in absolute values) among those not passing the cut if the stroke distribution is symmetric around the cut.
<table>
<thead>
<tr>
<th>Sample:</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcomes</td>
<td>-0.246**</td>
<td>-0.265**</td>
<td>-0.254**</td>
<td>-0.268**</td>
<td>-0.231*</td>
<td>-0.255**</td>
</tr>
<tr>
<td>Strokes</td>
<td>0.118</td>
<td>0.121</td>
<td>0.119</td>
<td>0.125</td>
<td>0.139</td>
<td>0.111</td>
</tr>
<tr>
<td>N</td>
<td>16,515</td>
<td>16,515</td>
<td>16,515</td>
<td>14,841</td>
<td>12,743</td>
<td>16,515</td>
</tr>
<tr>
<td>Outcome:</td>
<td>0.0323**</td>
<td>0.0347**</td>
<td>0.0338**</td>
<td>0.0339**</td>
<td>0.0283*</td>
<td>0.0351***</td>
</tr>
<tr>
<td>Making the cut</td>
<td>0.0139</td>
<td>0.0143</td>
<td>0.0141</td>
<td>0.0151</td>
<td>0.0167</td>
<td>0.0132</td>
</tr>
<tr>
<td>N</td>
<td>16,515</td>
<td>16,515</td>
<td>16,515</td>
<td>14,841</td>
<td>12,743</td>
<td>16,515</td>
</tr>
</tbody>
</table>

*Controls for assignment variable (strokes from cut):*
- Linear: Yes, Yes, Yes, Yes, Yes, Yes
- By treatment status: No, Yes, Yes, Yes, Yes, Yes
- By tournament: No, No, No, No, No, Yes
- Covariates: No, No, Yes, Yes, Yes, Yes

Note: Covariates are: Years as pro-player, Stroke average during the previous year and Passing the cut in the previous tournament, as well as a set of indicator variables for missing values on each of these variables. Tournament specific controls for the assignment variable are interacted with treatment status by tournament. Standard errors are clustered at the strokes times tournament level. */**/*** significant at the 10 /5/1 percent level.
Table 3: Confidence effects and tournament stakes

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Strokes</td>
<td>Making the cut</td>
<td></td>
<td></td>
</tr>
<tr>
<td>HS Treatment</td>
<td>0.227</td>
<td>0.123</td>
<td>-0.007</td>
<td>0.014</td>
</tr>
<tr>
<td></td>
<td>(0.290)</td>
<td>(0.406)</td>
<td>(0.036)</td>
<td>(0.050)</td>
</tr>
<tr>
<td>HS Outcome</td>
<td>-0.802***</td>
<td>-0.920**</td>
<td>0.113***</td>
<td>0.137**</td>
</tr>
<tr>
<td></td>
<td>(0.289)</td>
<td>(0.432)</td>
<td>(0.036)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>Interaction</td>
<td>0.213</td>
<td></td>
<td></td>
<td>-0.044</td>
</tr>
<tr>
<td></td>
<td>(0.582)</td>
<td></td>
<td></td>
<td>(0.072)</td>
</tr>
<tr>
<td>Mean dep v.</td>
<td>-0.307</td>
<td>-0.307</td>
<td>0.040</td>
<td>0.040</td>
</tr>
<tr>
<td>N</td>
<td>189</td>
<td>189</td>
<td>189</td>
<td>189</td>
</tr>
</tbody>
</table>

Note: HS denotes dummies for “high stakes” tournaments, i.e. tournaments with above-median prize sums. Covariates are: Years as pro-player, Stroke average during the previous year and Passing the cut in the previous tournament, as well as a set of indicator variables for missing values on each of these variables. Tournament specific controls for the assignment variable are interacted with treatment status by tournament. Standard errors are clustered at the strokes times tournament level. */**/*** significant at the 10 /5/1 percent level.
**Table 4. Heterogeneous effects – individual characteristics**

<table>
<thead>
<tr>
<th>Years as pro</th>
<th>Stroke average last year</th>
<th>Previous tournament cut</th>
<th>Outcome: Strokes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Inexperienced</td>
<td>-0.150</td>
<td>High</td>
<td>0.0725</td>
</tr>
<tr>
<td>(0.154)</td>
<td>(0.145)</td>
<td></td>
<td>(0.201)</td>
</tr>
<tr>
<td>Experienced</td>
<td>-0.264*</td>
<td>Low</td>
<td>-0.594***</td>
</tr>
<tr>
<td>(0.147)</td>
<td>(0.146)</td>
<td></td>
<td>(0.176)</td>
</tr>
<tr>
<td>Difference</td>
<td>0.114</td>
<td>Difference</td>
<td>0.667***</td>
</tr>
<tr>
<td>(0.176)</td>
<td>(0.159)</td>
<td></td>
<td>(0.279)</td>
</tr>
<tr>
<td>N</td>
<td>15,751</td>
<td>16,145</td>
<td>14,466</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Outcome: Made the cut</th>
</tr>
</thead>
<tbody>
<tr>
<td>Inexperienced</td>
</tr>
<tr>
<td>(0.018)</td>
</tr>
<tr>
<td>Experienced</td>
</tr>
<tr>
<td>(0.017)</td>
</tr>
<tr>
<td>Difference</td>
</tr>
<tr>
<td>(0.020)</td>
</tr>
<tr>
<td>N</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Controls for assignment variable (strokes from cut):</th>
</tr>
</thead>
<tbody>
<tr>
<td>Linear</td>
</tr>
<tr>
<td>By treatment status</td>
</tr>
<tr>
<td>By characteristic</td>
</tr>
<tr>
<td>Covariates</td>
</tr>
</tbody>
</table>

Note: Model is as Table 2, Column (3) except for interaction terms. Assignment (separately above and below the threshold) and treatment are interacted with the displayed characteristics. Stroke average during the year before and years of pro are split by the median within the sample. Covariates are: Years as pro-player, Stroke average during the previous year and Passing the cut in the previous tournament, as well as a set of indicator variables for missing values on each of these variables. Tournament specific controls for the assignment variable are interacted with treatment status by tournament. Standard errors are clustered at the strokes times tournament level. */**/*** significant at the 10/5/1 percent level.
Figure 1: Score density on either side of the threshold, and the distribution of relative differences relative to one stroke less.
Figure 2: Fraction playing in the outcome tournament.

Note: Full sample. 21912 observations.
Figure 3: Stroke average during the preceding year by strokes relative to the cut.

Note: Statistics are for used sample with nonmissing info on the relevant characteristic.
16145 observations
**Figure 4**: Years as a professional player by strokes relative to the cut.
Figure 5: Probability of passing the cut in the previous tournament (specification test, left-side panel) and in the outcome tournament (right-side panel) by strokes relative to the cut in the treatment tournament.
Figure 6: The impact on strokes in the outcome tournament by strokes relative to the cut in the treatment tournament.