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Personal Best (PB) goal-setting enhances arithmetical problem-solving

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Abstract Personal Best (PB) goals are defined as specific, challenging, and competitively self-referenced goals involving a level of performance or effort that meets or exceeds an individual's previous best. Much of the available research underpinning arguments for PB goal-setting is self-report-based; thus, the causal effect of PB goals on learning outcomes remains in question. The present experiment examined the impact of PB goal-setting (against a no-goal condition) on 68 Year 5 and 6 schoolchildren's problem-solving during an arithmetic fluency-building activity, SuperSpeed Math. Equivalence of the two conditions was established across a range of prior ability and self-report motivational variables, including prior mathematical ability; Personal Best, Mastery, and Performance goal orientations at the individual and classroom level; mathematics self-concept; and valuing of and interest in mathematics. Controlling for initial problem-solving performance, students who set PB goals in subsequent rounds showed a small but reliable advantage over students in the control condition. These results suggest PB goals may provide a way for students to experience both challenge and success in a range of classroom activities. Suggestions for future research based on these initial findings are made.

Keywords Goal setting · Personal Best (PB) goals · Mathematics · Problem-solving

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Introduction

The goals students set are among the most important influences on their self-regulation of learning and subsequent motivation. Goals direct attention and mobilise effort towards desired learning outcomes (Locke and Latham 2002). Following recent emphasis on growth-oriented goals (Anderman et al. 2015; Dweck 2012; Martin 2015), we focus on Personal Best (PB) goals. PB goals are defined as specific, challenging, and competitively self-referenced goals that involve a level of performance or effort that meets or exceeds an individual's previous best on a particular task (Martin 2006). PB goals may take two different forms: 'process' and 'product' PB goals. Examples of process PB goals include dedicating two hours more to completing an assignment than one's previous effort, or asking a parent for assistance with homework where no assistance had been asked for beforehand. Examples of product PB goals include getting more homework sums correct than at any previous time, or achieving a higher exam mark at the end of term than the beginning of term (Martin 2011a). The present study examines whether goal-setting based on PBs enhances problem-solving accuracy across a short, classroom-based activity for building arithmetical fluency.

Theoretical Perspectives on Goals and Goal Setting

Goal-setting theory

Scholarship based on goals and goal setting has burgeoned over the past four decades across a range of applied fields, including industrial and organisational psychology (e.g. Locke and Latham 2002; Schmidt 2013), sport psychology (Weinberg 2002; Williams 2013), and educational psychology (Seijts et al. 2013). Goal-setting theory (Latham and Locke 2013; Locke and Latham 2002) has driven much of the experimental research on the impact of goals on achievement. Reviewing the core findings of empirical research underpinning the theory, Latham and Locke (2013) posited that (a) a linear relationship exists between the degree of goal difficulty and performance, and (b) specific, difficult goals lead to higher performance than no goals or ambiguous goals such as 'do your best'. While much of the research on the effects of goal setting on learning has been conducted with adult participants in colleges (for a recent review, see Seijts et al. 2013), the effectiveness of goal setting in schools has also been established experimentally in early studies by Schunk and colleagues. Working with elementary school students with learning disabilities over a school week on subtraction worksheets, Schunk (1985) gave students in a self-set goals condition the following instructions:

> While working problems, it helps to have something in mind that you're trying to do. For example, you could try to work a certain number of pages today. Why don't you decide how many pages you think you could work today? (p. 311).

Students in an assigned goals condition were invited to aim to complete at least seven pages, and those in the no-goals condition simply completed as much work as they could. Students in the self-set goals condition had higher levels of subtraction skill and self-efficacy than either the set goals or no-goals condition at the end of the study. Similarly, Schunk and Rice (1989) compared effects of different goal setting instructions on children's self-efficacy and reading comprehension. Children in the process goal condition (focusing on "trying to learn how to use the steps to answer questions about what you've read"; p. 286) had higher levels of performance and self-efficacy on a reading test than participants in a general goal condition ("while you're working, try to do your best"; p. 287). Taken together, this research suggests that specific self-set goals, such as PB goals, have a positive impact on student outcomes.

Achievement goal theory

Achievement goal theory provides another important contemporary perspective that informs the present study. The theory makes a core distinction between mastery-approach goal orientations, focusing on the development of competence and task mastery, and performance-approach goal orientations, focusing on outperforming others or establishing competence through comparisons (Elliot 2005). Beyond approach goals, Elliot and McGregor (2001) argued students might also adopt mastery-avoidance (avoiding misunderstanding and/or a decline in competence) and performance-avoidance goal orientations (avoiding demonstrating incompetence to others, and/or weak performance in competitive or comparative settings). The latest 3×2 iteration of this model (Elliot et al. 2011) further distinguishes between goal orientations focusing on tasks, the self, and others along both approach and avoidance dimensions. Self-based-approach goals (e.g. "to do better on the exams in this class than I typically do in this type of situation") emphasise growth in a similar manner to PB goals. As such, the present study focuses specifically on approach goals.

Beyond the complexity of goals motivating a student, achievement goal theorists have also posited that goal structures operating at the classroom level can affect motivation, engagement, and achievement. For example, Ames' (1992) TARGET (task, authority, recognition, grouping, evaluation, time) framework identified three classroom structures—Task, Authority, and Evaluation/Recognition—that act to promote different achievement goals, with attendant effects on motivational patterns (e.g. levels of intrinsic interest, effort attributions, belongingness, and use of effective learning strategies). Of particular interest, the various instructional strategies that Ames argues are supportive of desirable motivational patterns are the Task structure-aligned strategies of "help students establish short-term, self-referenced goals" and "design tasks that offer reasonable challenge to students", as well as the Evaluation/Recognition-aligned strategy of "provide opportunities for improvement" (see p. 267, Fig. 1). By definition, PB goals are self-referenced, and their incorporation into brief classroom-based activities may provide a straightforward means of experiencing both challenge and improvement (since current performance must be bested) across a short time frame.

Prior Research on Growth/Personal Best Goals

Survey-based investigations over the past decade have established an extended nomological network around PB goals. An initial cross-sectional study by Martin (2006) found high school students' self-reported PB goals predicted persistence, class participation, educational aspirations, and school enjoyment. In a subsequent longitudinal study (Martin and Liem 2010) using a cross-lagged design, high school students' PB goals predicted subsequent literacy and numeracy achievement, test effort, persistence, school enjoyment, homework completion, educational aspirations, and engagement across a one-year interval. Liem et al. (2012) tested a longitudinal structural equation model using the same variates measured at the beginning and end of a school year, revealing PB goals as substantial predictors of deep learning strategy use, experience of academic flow, and teacher–student relationship quality when prior variance in all latent variables was controlled.

Contrasting PB goals with mastery and achievement goals, Yu and Martin (2014) found that, while mastery and PB goals were correlated (r = .74), both predicted substantial variance in Chinese middle school students' motivation, engagement , and academic buoyancy outcomes, with mastery goals more clearly linked to motivation factors and PB goals more clearly associated with engagement and buoyancy factors. Martin's (2015) longitudinal study that used a cross-lagged panel design found PB goals predicted students' implicit theories of intelligence one year later, but the reverse was not the case. Together, these correlational studies establish PB goals as predictive of a range of academically salient variables. Most recently, Martin and Elliot (2016a) found both PB and mastery goals predicted Australian high school students' motivation and engagement 1 year later, while the relation of performance goals with motivation and engagement outcomes was either neutral or negative. As with the results of Yu and Martin (2014), Martin and Elliot found that mastery goals and PB goals were substantially correlated (r = .78), but differential correlations of mastery versus PB goals with motivation and engagement outcomes was interpreted as evidence of the distinctiveness of these constructs. Across both of these studies, relative to mastery goals PB goals were more strongly associated with academic engagement, while mastery goals were more strongly associated with academic motivation.

In contrast, there is less research demonstrating a causal relationship between PB goals and subsequent learning outcomes using experimental designs. The earliest extant experimental investigation of a PB goal-like experimental manipulation appears to have been conducted by Alschuler (1969). This quasi-experimental study was framed as a comparison of power-oriented games, in which a player who earns a point necessarily deprives another player of a point, versus achievement-oriented games, which are not zero-sum and "which place greatest value on independent, self-reliant accomplishment" (Alschuler 1969, p. 20). Working with the teacher, students in restructured typing classes agreed on the standards of words per minute that would earn different grades, and in addition, students recorded their growth in typing speed daily, using these data to set personally challenging short-term and long-term goals. Compared to a comparison class from the previous year taught by the same teacher with the same materials, the restructured class typing accuracy was 54% higher by the end of the study than the comparison class.

A later field experiment (16 language arts classes, $n = 389$) by Slavin (1980) investigated the impact of using 'individual learning expectations' (ILEs) to reward students in a given week compared to their own past performance. Students in the ILE classes achieved at a higher level on an end-of-unit standardised test of language mechanics (e.g. punctuation, word usage) than students in control classes, controlling for pre-test scores on the same test. In a subsequent study, Beady et al. (1981) investigated learning in ten mathematics classes ($n = 307$) where students received certificates of recognition following quizzes based on ILEs, certificates of recognition based on relative standing in the class (with the top seven students receiving certificates), or no certificates. While students in the ILE classes outperformed those in the control condition on the end-of-unit test, contrary to expectations, the ILE condition did not outperform the relative standing condition. The authors noted a range of topic and school differences between their study and Slavin's (1980), including the relatively poor attendance rate in the 1981 study whereby certificates were given out only six times in 11 weeks. This may have meant that by the study's conclusion, students in the ILE condition were only just beginning to understand the nature of these rewards; thus, a longer implementation period might have been necessary under that school's context.

Two recent experimental studies in substantially different settings have investigated how PB goals might affect learning processes and outcomes. Martin et al. (2014) investigated PB goal-setting in the context of a museum-based, self-paced science education program. On entering the program, participants ($n = 71$ schoolchildren) completed a pre-test quiz on their knowledge of infectious disease facts (the program's focus), as well as a questionnaire about science valuing and aspirations. Participants who set a subsequent PB goal based on their quiz score showed significant growth in science valuing (but not aspirations), but due to technical issues, pre-test to post-test gains in knowledge could not be assessed. In the context of an annual mathematics contest, Martin and Elliot (2016b) invited some students to set a PB goal based on their previous year's performance, while others were simply reminded of their previous year's performance. Students in the PB goal condition substantially outperformed the control condition ($d = .52$, equivalent to a 20% percentile gain).

The Present Study

Research on goal-setting, especially within academic settings, provides a general foundation for an expectation that suitable targeted goal-setting will enhance learning. Considering the various forms of goals students might develop, correlational and experimental studies of PB goals indicate adopting specific, challenging, and competitively self-referenced goals exceeding a student's previous level of performance are associated with a range of desirable processes and outcomes.

Compared to survey-based studies, however, there are relatively few experimental studies of PB goal impacts for learning outcomes, and among extant studies, there are some noteworthy gaps. At present, the experimental evidence base lacks a clear consideration of how PB goals might be incorporated into a range of classroom activities. While teacher-directed instruction may focus on developing conceptual understanding, such activities often aim to build procedural fluency and automaticity in specific skills (e.g. arithmetical problem-solving) as a foundation for further learning (for a review, see Bratina and Krudwig 2003). Given that such activities necessarily generate data on the student's current level of achievement, classrooms serve as suitable sites for investigating the impact of PB goal-setting. In addition, while prior experimental studies of PB goals have typically incorporated prior knowledge as a control variable, there has been little consideration of the impact of students' prior motivations outside of the work by Martin et al. (2014). In the context of mathematics, motivation may vary, *inter alia*, as a result of achievement and PB goals at the classroom and individual level, self-concept, and valuing and interest of mathematics. The present study considers these potential confounds in testing the core hypothesis that students who set PB goals as part of an arithmetic fluency activity will solve more problems accurately than students who do not set PB goals.

Method

Participants

Participants were recruited from Years 5 and 6 (age range: 10–12 years; M = 10.78, SD = .67) in a government primary school located in Sydney, Australia. Of the overall sample, 68% were female (Year 5: 54%; Year 6: 70%). The research study was carried out following ethical approval from The University of Sydney, with written consent obtained from 72 students and their parents/caregivers. However, four students' responses were excluded from analysis for several reasons, such as declining to complete the provided activities (three students) and refusing to set PB goals (one student). The final sample thus consisted of 68 students randomly assigned into either the PB condition (35 students) or the no-goal comparison condition (33 students).

Materials

Participants were first asked to answer demographic questions (year group, gender, and age). We used the Brief Math Achievement test and self-report measures related to mathematics motivation as control variables, to test for group equivalence on variables potentially confounded with the processes under investigation (namely PB goals and arithmetic problem-solving). All materials were paper-based.

Group equivalence test measures

The measures used to test for group equivalence prior to the experimental manipulation traversed a range of arithmetical ability and mathematical goal and motivation constructs. The Brief Math Assessment test (Steiner and Ashcraft 2012) is a brief, wide-range measure of general mathematical problem-solving ability. Comprising ten items, the items range from simple 2-digit subtraction (Question 1) through to a college-level factoring problem with exponents (Question 10). Questions are scored 0 for an incorrect response, and 1 for a correct response. Cronbach's alpha for this scale was .71.

All self-report items were responded to on a 5-point Likert scale, where 1 = strongly disagree, and 5 = strongly agree. In total, we included six reliable scales (as indicated by Cronbach's alpha): the Personal Best (PB) goal orientation scale (alpha = .85), adapted from Martin (2006) to be appropriate to mathematics, comprising four items (e.g. when I do my mathematics schoolwork I try to do it better than I've done before"); the Mastery (approach) goal orientation scale (alpha = .75), adapted from Elliot and McGregor (2001) to the mathematics domain, comprising three items (e.g. "I want to learn as much as possible in mathematics classes"); and the Performance (approach) goal orientation scale (alpha = .85), adapted from Elliot and McGregor (2001) to the mathematics domain, comprising three items (e.g. "it is important for me to do better than other students in mathematics"). Single-item measures[1] of Personal Best, Mastery-approach, and Performance-approach classroom goal structures were adapted further by focusing on the student's perception of the typical goals of students in their mathematics classroom: "in my mathematics class, students really try to improve on how they've done before" (PB goal structure); "in my mathematics class, students want to learn and understand as much as possible" (Mastery-Approach goal structure); and "in my mathematics class, students really try to do better than each other" (Performance-Approach goal structure). Lastly, we included the Mathematics Self-Concept scale (alpha = .85) from the Self-Description Questionnaire-I (Marsh 1990), comprising five positively valenced items (e.g. "work in mathematics is easy for me") and the Valuing Of and Interest in Mathematics scale (Martin et al. 2012; alpha = .70), adapted from the 'Valuing' scale of the Motivation and Engagement Scale (Martin 2011b) to the mathematics domain, and comprising four items (e.g. "I'm able to use some of the things I learn in mathematics in other parts of my life").

Lesson materials

The materials used in the experimental phase were based on the SuperSpeed Math activity (Biffle 2007a). The activity is a paper-based resource used to build arithmetical problem-solving fluency, and is available through the Whole Brain Teachers of America organization (www.wholebrainteaching.com). The activity, typically completed by pairs of students, requires one student to mentally solve

[1] Reliability coefficients are not provided as these were single-item constructs.

provided arithmetic problems (i.e. no calculators, no written work) with the other student tracking performance within a set time limit. The student's test result from the initial test (e.g. 65/121 on the first addition problem set) is then used as the basis for a target to be beaten in a subsequent post-feedback test, using the same set of questions. In the present study, the research assistant took the role of tracking performance. Problem sets for addition (two sets), subtraction (two sets), and multiplication (two sets) rounds consist of 121 items each; the two problem sets for division rounds consist of 100 items each; and the two problem sets for what are called 'Gnarlies' (interleaved addition, subtraction, multiplication, and division items in a round) consist of 110 items each. The total number of practice questions that might be attempted was thus 1650 questions, across the initial test and post-feedback test rounds of 20 problem sets. Presentation of math problems was not counterbalanced. The order of presentation of problems followed the order given in the SuperSpeed Math manual, based on a progression from simpler to more complex problems.

For the purposes of estimating reliability of problem-solving performance on the SuperSpeed Math activity for both the initial test and post-feedback test phases of the experiment, we treated the number of correctly solved questions in each problem set (i.e. addition, subtraction, multiplication, and Gnarlies) as an indicator of a general capacity in arithmetic problem-solving. Students' arithmetic problem-solving performance (assessed by the number of problems solved) across the initial test set of rounds evinced Cronbach's alpha of .93; student performance (assessed by the number of problems solved) across the post-feedback test set of rounds evinced Cronbach's alpha of .94.

Procedure

Two research assistants arranged separate working areas in quiet spaces outside of the classroom. Prior to inviting a consenting student to meet outside of their classroom, the two participants randomly assigned the participant to one of the two conditions (PB goal and no-goal). Each assistant met individually with consenting student participants for 30–40 min to carry out the experiment. Once seated with a research assistant, a student recorded answers to questions on demographics and different sources of mathematics motivation. Next, the student worked through the SuperSpeed Math activity. The activity was conducted in 60-s time periods, with the research assistant timing the student. The research assistant introduced the activity individually to the participating student by reading aloud the following script:

> We're now going to do some mental maths questions. This is how we'll work together. I'm going to give you some sheets of paper with mental maths questions on them. For each sheet of questions, your task is to answer as many questions as you can in 60 s. I'll give you some feedback on how many you got correct, then we'll go through that sheet of questions again. Please start with the questions on the top row, going from left to right, then the second row from left to right, and so on.

For the *PB condition*, after each initial 1-minute attempt at a set of initial test questions (e.g. addition), the researcher gave the following instructions:

> OK, your score on these addition questions was [X]. This is your Personal Best score. Now we're going to do these questions again, and I would like you to set a goal where you aim to do better on these questions than you did before. Now before we start, can you explain to me what your goal is in this task?

This latter question was used as a manipulation check; if the student did not appear to understand the instructions, the instructions were provided again until the student indicated comprehension. For the students in the *comparison condition*, the only difference with the experimental (PB) condition was such that students in the condition were informed, "OK, your score on these [addition] questions was [X]. Now we're going to do these questions again".

Students in both conditions completed ten sets of two rounds of arithmetic problem-solving, i.e. 20 rounds in total. As noted above, the first set of two rounds (1 min per round) consisted of addition questions; after the first initial test round, students would complete questions from the same set again as a post-feedback test. The second set (third and fourth rounds) consisted of different addition questions. The third set (fifth and sixth rounds) and the fourth set (seventh and eighth rounds) used two different sets of subtraction problems. The fifth set (ninth and tenth rounds) and the sixth set (11th and 12th rounds) used two different sets of multiplication problems. The seventh set (13th and 14th rounds) and the eighth set (15th and 16th rounds) used two different sets of division problems. The ninth set (17th and 18th rounds) and the tenth set (19th and 20th rounds) consisted of two different sets of Gnarlies consisting of intermixed addition, subtraction, multiplication, and division questions incorporating the numbers 6, 7, 8, and 9. Following the SuperSpeed Maths activity, students were thanked for their participation and returned to class. Each research assistant recorded any field notes prior to beginning the process with the next consenting student.

Data Analysis and Tests of Statistical Assumptions

Data analysis commenced with checking for completeness of responses. For two of the goal and motivation scales (Personal Best orientation and mathematics self-concept), a participant did not complete one of the items in the scale. Missing data were imputed through SPSS 21 using the Expectation Maximisation (EM) algorithm. Graham and Hoffer (2000) note this method provides good recovery of population parameters when the percentage of missing data is less than 5%, as was the case in this study. Missing responses were imputed based on responses to other items in the scale along with demographic covariates (age and gender).

Data analysis proceeded with a complete dataset. To establish the construct validity of the multi-item self-reports of mathematics motivation, we applied confirmatory factor analysis to these data using M*plus* 6.12 (Muthén and Muthén 1998–2010). We followed recommendations on establishing model fit provided by Marsh et al. (2004), focusing on the Comparative Fit Index (CFI), the Root Mean

Square Error of Approximation (RMSEA), and an evaluation of parameter estimates to assess model fit. For the RMSEA index, values at or less than .08 and .05 are taken to reflect acceptable and excellent fit, respectively (see Marsh et al. 1996; Yuan 2005). For the CFI, which varies along a 0-to-1 continuum, values at or greater than .90 and .95 are typically taken to reflect acceptable and excellent fit to the data, respectively (McDonald and Marsh 1990). To address the negative bias to fit statistics associated with estimating complex models with small samples, we used the Swain (1975) small sample correction function (Boomsma and Herzog 2013; Herzog and Boomsma 2009).

Subsequent analyses of the control variables aimed to evaluate whether the two conditions could be assumed equivalent across a range of potentially confounding variables. These analyses thus consisted of two-group comparisons. Assumptions of the independent group's *t* test include random assignment to conditions; independence of scores; normality of distributions; and homogeneity of variances. The first and second of these assumptions was achieved through the experiment's design. The third assumption was tested using the Anderson–Darling test of normality, based on Keselman et al.'s (2013) recommended type 1 error rate of .20 (see p. 17). Across the control (Brief Math Assessment and mathematics goal and motivation) variates, the assumption of normality was not tenable in one or both conditions (see A-D columns in Table 1), but was met for the experimental phase variates (Problems solved in Round 1 and Problems solved in Round 2).

The accuracy of the Levene test, commonly used to test the fourth assumption of homogeneity of variances across experimental conditions, relies on the above assumption of normality. Under violations of the assumption of normality, the non-parametric Levene test (Nordstokke and Zumbo 2010; Nordstokke et al. 2011),

Table 1 Means (M), Standard Deviations (SD), and Anderson–Darling Test *p* value (A–D) for Brief Math Assessment Scores, Prior Motivation Self-Reports, and Number of Correct Solutions

	Comparison Group			PB Goal-setting Group		
	M	*SD*	*A-D*	*M*	*SD*	*A-D*
Variables to test for pre-existing group differences						
Brief Math Assessment (/10)	5.09	1.84	.067	4.91	1.87	.001
PB goal orientation (/5)	4.29	.68	.012	4.30	.71	.002
Mastery goal orientation (/5)	4.43	.65	< .001	4.23	.67	.004
Performance goal orientation (/5)	3.30	1.19	.081	3.30	1.08	.156
PB classroom goal structure (/5)	3.61	.56	< .001	3.60	.88	< .001
Mastery classroom goal structure (/5)	4.03	.88	< .001	4.03	.92	< .001
Performance classroom goal structure (/5)	3.06	1.14	.001	3.09	1.15	.001
Mathematics self-concept (/5)	3.93	.62	.529	3.88	.63	.001
Mathematics valuing and interest (/5)	4.50	.55	< .001	4.37	.69	< .001
Problems solved—Round 1	378.36	118.47	.458	399.63	136.20	.300
Problems solved—Round 2	433.88	119.05	.268	465.43	135.96	.229

based on a rank transformation of all scores followed by Levene's test on these scores, provides a suitable alternative. The results of the non-parametric Levene tests were non-significant ($p > .05$), with the exception of PB classroom goal structure ($p = .002$).

The primary focus of the present study was the impact of PB goal-setting on arithmetic problem-solving performance, based on prior performance. The data generated by the study's design thus lend itself to the analysis of covariance (ANCOVA), where performance on initial rounds of problem-solving serves as the covariate, and performance on subsequent rounds of problem-solving serves as the dependent variable. While data from pre-test/post-test control group experiments might be analysed in several ways, including analysis of change scores or 2 × 2 mixed design factorial analysis of variance, Huitema (2011) argues ANCOVA has distinct advantages over these alternatives for analysis in adjusting for chance differences on the pre-test as well as generally higher experimental power. ANCOVA assumptions include use of continuous variates (as was the case in the present study); a statistically reliable (preferably substantial) correlation of the covariate with the dependent variable across both conditions; homogeneity of regression slopes; and homogeneity of error variances for the independent and dependent variable. The correlation between pre- and post-scores was substantial and statistically reliable in the PB goal-setting condition ($r = .98$, $p < .001$) as well as the comparison condition ($r = .98$, $p < .001$). The assumption of the homogeneity of regression slopes was tenable, $F(1, 67) = .06$, $p = .809$; however, Levene's test for the equality of error variances indicated that the error variances were not equal across conditions for the dependent variable, $F(1, 66) = 5.70$, $p = .020$.

Given these tests of statistical assumptions and some evident violations, it was necessary to conduct analyses that would most appropriately accommodate these violations while also validly test our hypotheses. We did so through a sensitivity analysis comparing results based on ordinary least squares with those based on random permutation of the data. Randomisation-based analyses do not rely on any assumption (e.g. normal distributions, random sampling of units from a population) apart from random assignment to conditions (Edgington and Onghena 2007). In such analyses, the results of the actual mean difference between conditions are compared with a large number of mean differences based on random allocations of the same data. The probability of the mean difference—or adjusted mean difference in the case of ANCOVA—is calculated as the proportion of randomly permuted mean differences that is extremer than the observed statistic. We adopt a Type 1 error rate of .05, and for each analysis, we also present the standardised mean difference effect size (d). There have been a range of suggestions for judging the magnitude of d in educational research. Tallmadge's (1977) review of best practice in educational evaluation noted "one widely applied rule is that the effect must equal or exceed some proportion of a standard deviation—usually one-third (i.e. 0.33), but at times as small as one-fourth (i.e. 0.25)—to be considered educationally significant" (p. 34). More recently, Hattie's (2009) review of over 800 meta-analyses of educational research suggested the following benchmarks for effect size magnitude: small $d = 0.20$, medium $d = 0.40$, and large $d = 0.60$ and above.

However, authors including Breaugh (2003) and Prentice and Miller (1992) have argued that in situations in which the independent variable is diffuse and/or in which the dependent variable is difficult to influence, even small effect sizes ranging from 0.1 to 0.2 may be considered educationally significant.

Results

Results for the PB goal-setting experimental condition and the comparison condition are given in Table 1. Probability values from permutation-based analyses are given after the p values derived from ordinary least squares-based analysis, and are marked p_{perm}.

Control Variables and Tests for Pre-existing Group Differences

A confirmatory factor analysis of multi-item self-report scales showed acceptable fit of the model to the data, CFI = .91, RMSEA = .07. All factor loadings were significant at $p < .001$, ranging from .52 to .94. Latent factor correlations ranged from − .05 (PB orientation with self-concept) to .74 (PB orientation with Mastery); this last result is highly similar in magnitude to findings described above (cf. Martin and Elliot 2016a; Yu and Martin 2014). Taken together, the results of the CFA provide evidence of the construct validity of the multi-item scales, and for creating scale scores for statistical analyses addressing the substantive questions of the study.

There were no statistically reliable differences between conditions on (a) their prior ability in mathematics as measured by the BMA, $t(66) = .39$, $p = .696$, $p_{\text{perm}} = .735$, $d = -.10$; (b) their PB ($t(66) = -.06$, $p = .952$, $p_{\text{perm}} = .952$, $d = .00$), Mastery ($t(66) = 1.29$, $p = .201$, $p_{\text{perm}} = .217$, $d = -.31$), or Performance goal ($t(66) = -.01$, $p = .995$, $p_{\text{perm}} = .993$, $d = .00$) orientations in learning mathematics; (c) their perceptions of PB ($t(66) = .03$, $p = .973$, $p_{\text{perm}} = .963$, $d = -.01$), Mastery ($t(66) = .01$, $p = .994$, $p_{\text{perm}} = .999$, $d = .00$), and Performance ($t(66) = -.09$, $p = .998$, $d = .02$) goal structures at the classroom level; (d) their mathematics self-concept using the short form of the *SDQ-I*, $t(66) = .28$, $p = .780$, $p_{\text{perm}} = .741$, $d = -.07$; or (e) students' valuing of and interest in mathematics, $t(66) = .40$, $p = .399$, $p_{\text{perm}} = .402$, $d = -.20$. Together, these results confirm that the two experimental conditions formed by random assignment can be considered equivalent across a range of variables likely to play a role in mathematical learning and problem-solving.

Effect of PB Goal-setting on Arithmetical Problem-solving Performance

To investigate the impact of PB goal-setting, we first summed students' performance across the ten 'first rounds' (conducted prior to the PB goal-setting group or comparison group instructions), and across the ten 'second rounds' (conducted immediately after the PB goal-setting group or comparison group instructions). We used analysis of covariance to analyse the data, treating students' first round performance as a 'pre-test' covariate. The difference between conditions in first round performances was not statistically reliable, $t(66) = .69$, $p = .496$, $p_{\text{perm}} = .498$, $d = .17$. Taking pre-test performances into account as a covariate, permutation-based analysis indicated that there was a small but statistically reliable effect of PB goal-setting, $F(1, 65) = 3.02$, $p = .087$, $p_{\text{perm}} = .041$, $d = .08$, favouring the PB goal-setting condition.

Discussion

The present study adds to the largely correlational body of research on PB goal-setting by experimentally testing the effect of setting such goals on arithmetical problem-solving performance. Students who set a PB goal for arithmetic problem-solving based on performance in initial rounds solved more problems in subsequent rounds than students who were simply informed of their performance in the initial round. Our study adopted a classroom-based exercise, SuperSpeed Math, in which goal-setting based on PBs has a central role. Describing the exercise's design, Biffle (2007a) argues:

> Students love to play SuperSpeed Math because they love to strive for goals and to set and break personal records. Players are never competing against each other, but against their own previous best effort. Thus, the learning objective is set at exactly the right level, no matter a player's ability. (p. 14)

While these arguments appear to be based on anecdotal testing and usage of SuperSpeed Math, they are nonetheless consistent with theorising on the motivating effects of goals which are well-aligned with a student's current ability level, and which the student him/herself sets.

Performance in mathematical problem-solving, including arithmetic, is a function of diverse cognitive and motivational factors. While not exhaustive, the present study included a range of potential measures of such factors, including a wide-range test of mathematical problem-solving ability; Mastery-approach, Performance-approach, and PB-approach goal orientations at the individual level; students' perceptions of classroom Mastery, Performance, and PB goal structures; mathematics self-concept; and interest and valuing of mathematics. None of the differences between experimental conditions on the above variables were statistically reliable, indicating random assignment of participants to conditions served to create equivalent groups. The statistically reliable difference between conditions can therefore be attributed to the effect of PB goal-setting; the lack of reliable differences between the groups on both general mathematical ability and a range of motivational variables measured prior to the experimental manipulation strengthen this conclusion.

While statistically reliable, the effect of PB goal-setting was small ($d = .08$). Discussions of standardised mean difference effect sizes in experimental educational research have suggested a range of benchmarks for educational significance, ranging from .25 (Tallmadge 1977) to Hattie's (2009) suggested benchmarks for small ($d = 0.20$), medium ($d = 0.40$), and large ($d = 0.60$) effects. While the effect of PB goal-setting in the present study would be considered small under these guidelines, we believe this result is nonetheless educationally significant for several reasons. First, the result was obtained based on a very simple experimental manipulation (asking students to set a PB goal based on their own performance). Recent experimental investigations of school-based interventions based on principles from social psychology have demonstrated that very brief exercises (e.g. attributional retraining, affirming important values) targeting students' school-related thoughts, emotions, and beliefs can have reliable effects on subsequent academic achievement (for a review, see Yeager and Walton 2011). The present study is distinct from the above body of research in targeting students' goal-setting, yet shares a focus on ease of implementation. Second, and related to the first point, Hattie (2009) notes small effects may nonetheless be practically valuable when considered in context. The context of the present study involves short-term improvement in arithmetical problem-solving fluency, across a range of arithmetical skills, under time pressure to build fluency. Substantial improvements to such a complex and variegated cognitive skill will be unlikely through a 'one-shot' intervention; instead, students would undertake such activities repeatedly over the course of weeks or even months as a means of building fluency. In this sense, then, the small effect obtained in the present study becomes a starting point for a virtuous cycle of enhanced achievement. Third, the experimental manipulation was incorporated into a classroom activity (Biffle 2007a) already used by a substantial number of teachers; thus, we argue the results have substantial external validity.

Limitations and future directions

There are inevitable limitations to a single study such as this that provide avenues for future research. We have argued above that the small effect from PB goal-setting could be understood as an initial basis for enhanced performance across a larger number of trials, but further studies will be needed to validate this claim. There are substantial challenges in conducting longitudinal field experiments, including threats to validity such as attrition and diffusion of the treatment; single-case designs involving random inclusion or exclusion of PB goal-setting phases over extended time periods provide an alternative design for testing causal hypotheses (see Kratochwill and Levin 2014). Such designs would support estimating the cumulative effect of PB goal setting on a cognitive skill such as arithmetical problem-solving, complementing the 'single-trial' approach taken in the present study. The present study is also limited in examining the effectiveness of PB goal-setting in only one domain. Future studies should investigate whether PB goal-setting improves problem-solving performance in disparate topics requiring fluency, such as reading. Biffle (2007b) describes a classroom activity similar to SuperSpeed Math for teaching letter recognition that also incorporates a goal-setting component; we speculate PB goal-setting may be straightforwardly incorporated into many activities aiming to build fluency under time pressure, and computer-based activities may play an important role in reducing the resources needed to set and track such goals. Ultimately, it would be desirable to demonstrate experimentally the efficacy of classroom-based goal-setting exercises like SuperSpeed Math where students (rather than adults) run the activity; the present study is a first step towards that goal.

Understanding potential moderators and mediators of PB goal-setting effectiveness would also be a valuable line of investigation, but will require samples substantially larger than the present study for adequate statistical power of interaction effects (Whisman and McClelland 2005). Research on goal-setting suggests two likely moderators: expertise and goal commitment (Locke and Latham 2013). Resource allocation theory (Kanfer and Ackerman 1989) holds that focusing on specific, challenging goals when in the early, declarative stage of learning will consume cognitive resources that would better be directed towards learning to perform the task effectively. Thus, novices in a given domain may derive fewer benefits from setting PB goals than students with higher levels of expertise. Goal commitment refers to "one's attachment to or determination to attain the goal, regardless of its source" (Latham and Locke 2013, p. 7), and acts as a moderator because the relationship of goal difficulty with performance is stronger for people with higher goal commitment than those with lower goal commitment. Students' goal commitment as a moderating variable might be measured using Klein et al.'s (2001) brief goal commitment scale, including items such as "I am strongly committed to pursuing this goal" and "I think this is a good goal to shoot for". Another potential line of research involves the motivational constructs used as control variables to assess group equivalence; larger studies might explore the extent to which such variables mediate the impact of a PB goal setting intervention. Process models with multiple mediators and/or moderators (see Hayes 2017) could enhance the understanding of the mechanisms underpinning the impact of PB goals.

It is possible that the small effect size found in the present study can be explained by students in the comparison group aiming to beat their Round 1 scores, even though they were not explicitly instructed to do so. Future studies might incorporate post-lesson questions to all participants to determine the extent to which students were using PB goals regardless of experimental condition; for example, "during the test did you try and beat your score after you were told what it was? (yes/no)" and "if yes, how hard were you trying? (not at all, a little bit, quite a bit, a lot)". An alternative comparison group receiving no feedback on initial performance would also support understanding of the simple effect of feedback, irrespective of PB goals.

Lastly, the nature of the source of the feedback could play a major role in the efficacy of PB-based goal-setting in school settings. The effect of PB goal-setting in the present study was generated based on interactions with adult research assistants. It is possible that the research assistants' status as 'authority figures' played an important role in generating an effect in this study. This possibility might be investigated in future research by including a comparison group where the feedback from an adult is restricted to "try to perform better on the second round" without giving any specific feedback on amount correct. Studies might also test the effect of PB goal-setting with children tracking problem-solving performance rather than adults (as reflected in the standard use of SuperSpeed Math; Biffle 2007a). Computer-based lessons used to develop fluency such as Mathletics might also be adapted to test the effects of PB goal-setting and a range of feedback options. Using such sites as testbeds would have the advantage of an expanded range of dependent variables, including number of questions attempted, reflecting effort; raw number correct, reflecting performance; and proportion correct, reflecting accuracy. The large number of students accessing such sites both during school time and after school would support longitudinal experimental studies with substantial statistical power, including tests of moderators and mediators as discussed above.

Studies based on self-reports have established that the extent to which students set PB goals predicts a range of learning processes and outcomes. In contrast, there is relatively little experimental evidence for the efficacy of goal-setting based on PBs. This experimental study has demonstrated how PB goal-setting can be incorporated into a typical classroom activity used to build fluency in arithmetical problem-solving, yielding evidence for a causal effect on PB goals on problem-solving performance.

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