

# Book review: Next Generation Performance Management. The Triumph of Science over Myth and Superstition. By Alan L. Colquitt.

Book review by Patrick Vermeren

Someone pointed out to me that this book would be very worthwhile reading, as it corroborated my views in my earlier books (e.g., *Around Leadership. Bridging the Scientist-Practitioner Gap*). Indeed, the title sounds promising and is in line with the new book I am preparing myself (it is an elaboration of my TEDx talk, which you can find here: <https://t.co/9EhQOmbDsC>).

That's why I started reading the book with more than average interest and curiosity. As a member of the worldwide skeptic community, I consider myself to be quite good at critical thinking, but I was also aware that I might fall into the trap of confirmation bias, as at first glance, Alan Colquitt holds the same views about performance management as I do. A warned (wo)man counts as two.

The book starts with an historical overview of the birth of what Colquitt calls Performance Management 1.0. As an American, he interprets the rise of PM 1.0 as the result of :

- the need for more objective criteria to hire and promote people;
- the employment laws that changed during the 1960s and 1970s - requiring legal defense of human resources decisions;
- the popularity of Management By Objectives (MBO) that introduced goals and their measurement, and;
- the rise of more future oriented PM practices, such as outputs, results, rewards (e.g., pay-for-performance) and punishments, as well as employee development (e.g., personal development plans).

I agree with Colquitt that PM 1.0 has become too much of a one-size-fits-all approach: there are too many objectives and too many features and processes involved and what it surely does not do is motivate employees. He also rightly points out that (legal) documentation is only useful for the 1% (a handful) of employees who underperform or should be fired for other reasons. He shares my view that we should manage these 1% outside of PM.

I am already twenty pages further in this book and I finally see some names of researchers that sound familiar to me: Edwin Locke (goal-setting), Richard Thaler (the *endowment effect*), Herbert Simon (*bounded rationality*), Amos Tversky and Daniel Kahneman (*framing effects* - in terms of gains or losses). The author states that these researchers and their research pointed out the flaws in the central ideas in psychology and economics at the time: contrary to what was thought until then, people are not rational agents, they make bad decisions and their behavior cannot simply be changed by rational appeals, punishment or incentives. He also claims that *agency theory* assumes that the relationship between employee and employer is adversarial because the two parties have a conflict of interest. He also gives a nasty blow to *tournament theory*, which propagated the idea that organizations should create large pay gaps between the top performers and the rest, with the lower scoring people giving up a large proportion of their salaries, in order to be given to the top employees. The idea was that in such tournaments, people would compete to rise to the top

by working harder. He challenges the idea that money motivates and that financial rewards need to be contingent upon individual performance. He finally maintains that because of this inherent mistrust from employers in employees, the emphasis is too much on control and delivering negative feedback to correct employees who go “off course.” He also rightfully points out that the practice of benchmarking (which Jeffrey Pfeffer once labeled as a practice that can only lead to mediocrity) led to a homogenization of business and HR practices across companies. Instead of “best practices,” he says, we should speak of mediocrity, common practices and imitation of popular companies. The consulting firms are to blame too, because they give the same advice to all companies, selling it as “best-in-class” practices. Finally, as less than 1% of HR practitioners read academic papers (Rynes, Brown & Colbert, 2002), scientific research is overlooked and people instead rely on beliefs, hearsay, intuition, and so-called personal experience.

In chapter 4, Colquitt gives an overview of the many problems with **performance ratings**: People hate getting feedback from others; comparing people to each other (relative feedback) can negatively affect performance; there are too many documented biases (anchoring, confirmation bias, recency, halo effects, leniency, etc.). These problems make boss-only performance ratings extremely untrustworthy. In this chapter, I raised my eyebrows for the first time: he refers to Dan Pink, the popular writer who made some of the central ideas of Self-Determination Theory known to the business public (in his book *Drive*). Pink is no scientist and without rigorous scientific research, he changed the three basic needs as defined in Self-Determination Theory (ABC or autonomy, belongingness and competence) into autonomy and mastery and added a third one – purpose – based on a discussion with Mihaly Csikszentmihalyi (one of the gurus of the Positive Psychology movement). Pink’s book is full of cherry picking and references to very, very questionable research (e.g., the controversial notion of grit as described by Angela Duckworth; the myth of the left versus right hemisphere lateralization; Carol Dweck’s theory about a growth mindset; and the *Good to Great* book by Jim Collins, which is stifled with halo effects because of the poor research methodology, etc.). Not very good references.

Colquitt says that although many companies are now moving away from rating employees and using the incredibly flawed concept of forced ranking (in a Gaussian distribution curve), they basically are replacing the old rating with new rating. He doesn’t believe these new ratings are more accurate than the old ratings. He radically proposes a shift to PM 2.0. He proposes three solutions to ratings:

1. Instead of being based on individual performance ratings, pay should be market-based and bonuses should be based on group and firm performance;
2. Use more objective ratings such as “*360 degree feedback surveys, personality tests, interest inventories, ability tests, skill assessments and assessment centers*” (p. 61);
3. If companies really want to further rely on performance input for their decisions, they should use performance evaluations ... without sharing them with the employees. He just assumes they “*will be more accurate because they have no far-reaching consequences for the employee*” (p. 62). I don’t see how, and Colquitt fails to give convincing arguments.

A lot can be said about these proposed solutions, but I will only deal with the second one a bit more extensively. First of all, a lot of 360 degree feedback instruments are not

scientifically validated. Of those that are, many are not very well designed, with typical problems such as huge differences in feedback scores between raters, often due to the quality of the feedback tool itself. There are only a few personality tests that are really very good (reliable and valid, and offering predictive validity such as measures based on the Five Factor Model of personality -e.g., the NEO-PI-3 by Costa & McCrae, or the Six Factor Model of personality - e.g., the HEXACO by Ashton & Lee), but they have limited predictive powers for job performance for example. If one looks at the latest update by Frank Schmidt and his colleagues, assessment centers generally have a poor predictive validity for job performance when compared to a simple but well-designed intelligence test (Schmidt et al., in press).

The next chapter is about **money** and is even more problematic, with airy titles such as “People Are Not Motivated by Money.” It is full of contradictions. For example, despite the title, he writes that money is important too. He agrees that people value money, but that it sometimes has adverse effects. For example, raising the toll on express lanes actually resulted in more people wanting to take the express lane. However, this example does not at all prove that people are not motivated to work for money, only that they are sometimes willing to pay for status or for “premium services.” The examples of volunteers working for free also do not imply that these people don’t have regular jobs to earn their money. Who can live well and flourish without money? It is clear that not everyone is driven by greed, but that does not mean that money would not motivate people at all. Money is an important motive for many people: they use money for intrinsic reasons -- to become more independent and thus experience more freedom and self-determination, to provide a good future and education for their children or other family members, to be able to do some charity, etc.

Then I notice the next serious problem in the book. On page 69, Colquitt refers to *adaptation* when in fact he should have referred to *habituation*. Adaptation is the term that is scientifically reserved for the biological process known in the theory of evolution: organisms reproduce, and their offspring often has mutations that can lead to adaptations by natural selection (I will not go further into detail of these processes). Habituation is the term used to describe the extinction of an emotional response when people are exposed to the stimulus that led to the emotion multiple times. He is also confused about “*returning to a ‘set point’ for happiness*”: this is not an adaptation or habituation, but a simple falling back to the basic, innate set point. He also refers to the popular claim that money does not make one happy. This is very questionable – having (or winning) a lot of money beyond a minimum threshold does not make one happy, but not having enough makes one unhappy and even aggressive and violent. There is more peace and happiness in countries that have reached a certain level of wealth for their inhabitants. However, he is right when he points to the findings that relative income (compared to others) is twice as important as actual income in driving happiness (but didn’t he just write money was *not* important for happiness?).

Based on research findings, I have been a severe critic of **pay-for-performance** (P4P) programs, so I had to take care not to fall into the pitfall of confirmation bias once again when I read his chapter 6. I think he really comes up with the right arguments and criticisms on P4P, such as that much research was conducted in laboratory settings with students, and that these findings can hardly be extrapolated to working adults earning a lot more than a student gets paid (for experiments). He also refers to Jeffrey Pfeffer and Robert Sutton who

took down the classic research by economist Edward Lazear at Safelite Autoglass: the job is indeed very fit for P4P with employees who work independently, with an easy job, easy to measure and monitor, and with a good monitoring program already in place -- P4P would indeed make employees work faster. Again, this can't be extrapolated to the majority of jobs, as they are often much, much more complex and challenging. But, then he engages in cherry picking – selecting the research that confirms his views without referring to the critics or research that contradicts their findings, or rather, their opinions. He refers to research by Edward Deci – one of the founding fathers of Self-Determination Theory (SDT). He concludes that “*research shows extrinsic rewards can negatively affect intrinsic motivation, especially when the work is complex and interesting to begin with, when it requires creativity and innovation, when incentives/rewards are explicit and expected, and when they come with extensive, intrusive monitoring*” (p. 87). I reviewed the literature in 2017, and this conclusion is far from certain. First of all, intrinsic motivation shows high correlations between two types of extrinsic motivation as defined in SDT: integrated motivation correlates between .69 and .75 and identified motivation between .64 and .80 with intrinsic motivation (Gerhart & Fang, 2015). Gerhart and Fang convincingly argue that we should rule out the idea that one type (e.g., controlled motivation using rewards) will always undermine the “higher” quality motivation types. Most of the time, people will be motivated by several types to varying degrees; although we must promote autonomous motivation over controlled motivation (Kuvaas et al., 2016) because if the emphasis is on controlled motivation, several unwanted negative effects might occur. For example, Yam et al., 2017 found that if people show good organizational citizenship behavior (OCB) because of extrinsic reasons, they get a sense of entitlement<sup>1</sup> and engage in deviant behaviors (p. 389). A recent study with students (alas) also points in the direction that people have several types of motivation at the same time. The study found that a “general factor of motivation” was a better longitudinal predictor of physical activity than specific types of motivation as described within SDT (Gunnell & Gaudreau, 2015). This view was already advanced by Weibel, Rost, and Osterloh (2007). Today, a more complex and nuanced picture arises: for example, some workers that are described as *highly motivated* show not only high levels of intrinsic motivation (e.g., pleasure at the task), identified regulation (e.g., outcomes that are personally relevant or important), and partially internalized motivation (external social pressure: shame, guilt or pride), but also higher than average levels of external regulation in comparison to other workers – even in comparison with workers who experienced low levels of external and partially internalized motivation and above average levels of fully internalized (identified regulation) and intrinsic motivation (Graves et al., 2015; Howard et al., 2016). I still agree with the central tenet of Colquitt's conclusion however: it is very tricky to use complex pay schedules because a lot can go wrong very quickly.

Then Colquitt's reasoning becomes cloudy: after having argued that companies should promote collaboration over competition, he considers creating more opportunities for promotion as a means to motivate employees in other ways. Especially for top talent, organizations should promote them more frequently. He says that nothing is more discouraging than discovering you only have one or two promotion opportunities in your

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<sup>1</sup> This is called a moral licensing effect in psychology: if people have done good deeds it often leads to subsequent bad behavior, like a kind of compensation or feeling of entitlement (“now that I have done a good deed, I can now compensate by doing a bad deed.”)

career. OK, but how do you align this with the plea for collaboration and for removing all competitive elements?

In my view, the problem is that he based himself on a much too narrow framework about motivation. The ABC of SDT (autonomy, belongingness and competence) is much too narrow and dramatically lacks full explanatory power for many phenomena at work. Adding purpose (as proposed by Dan Pink) out of the blue won't help either. We need to take a broader, meta-theoretical look at human motives. First of all, we are a social species. The survival and reproductive fitness of our ancestors depended highly on their capacity to collaborate. Natural selection favored *both* 'selfish' or agentic *and* 'altruistic' or communal goals, drives and behaviors. The human mind constantly tries to calibrate these two motives.<sup>2</sup> Calibration is done by calculating Welfare Trade-Off Ratios (WTRs), where an organism (or in our case a human being) quickly (often unconsciously) calculates the costs and benefits for one's own welfare versus the welfare of others. Evolutionary Psychologists thus look at motivation as "calculus programs" in our brain that calculate the different weights in the factors that should enable us to make choices that direct our behavior. If the specialized module in our brain calculates there is more at stake for ourselves than for the others, for example, we might become angry, an emotion that makes us behave more aggressively to defend our interests. Autonomy and competence (two of the three needs from SDT) are only submotives of the larger agentic or selfish metamotive – other submotives of agency are self-protection, status-seeking, reputation management, kin-protection, achievement, (inflated) self-esteem, impression management, etc. The communal or cooperative metamotive also encompasses several submotives such as our innate desire to belong to (different) in-group(s), exclusion avoidance, the need for reciprocity, free-rider avoidance, social support, etc. Making meaning is considered the third meta-motive, encompassing our need for order, structure, predictability, and meaning (e.g., Hogan, 2006). These meta-motives have evolved over millions of years, and although our modern (mainly Western) society can no longer be compared to the environment in which our savanna-dwelling ancestors lived, it is highly unlikely that these should no longer play a role in our daily lives, including our professional lives. People are not only motivated by ABC to go to work. People want to earn money to take care of themselves and their families, and many also want to move up the social ladder, at least to a level where they can compare themselves with others without feeling like a *loser*. For example, the relative pay or one's own pay level in comparison to others in our environment is what can make us feel happy, not the absolute amount of money (e.g., Harris et al., 2008). Under uncertain situations, people become less cooperative, less agreeable and more selfish too.

The book goes on to argue we should **use goals in a different way**. But the author accepts that "*effectiveness of goal setting is among the most well-established findings from the social sciences*" (p. 99). He backs up his claim by referring to three meta-analyses, which he considers the gold standard of research reviews. He is right, but only to the point when

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<sup>2</sup> There is great consensus on this among biologists as well as anthropologists and a large number of psychologists. In biology, 'selfishness' and 'competition' are the most often used terms to describe this drive or goal, whereas in psychology, 'agency', 'power' and 'autonomy' are often used. In biology 'cooperation' and 'altruism' are the most often used terms for the second drive, whereas in psychology 'communion', 'affiliation', 'belongingness' or 'relatedness' are often used. In psychology these 'Big Two' are labeled meta-motives – motives that overarch submotives, or – in other words – motives of a higher order.

these meta-analyses used the right methodology. I have reviewed four (not three!) meta-analyses myself, so here I come: The first attempt at meta-analyzing the previous studies included only 21 studies to test the goal difficulty hypothesis and another 22 to test the goal specificity hypothesis (Chidester & Grigsby, 1984). The first study (goal difficulty) not only mixed ten lab experiments (with undergraduate students) with 11 field studies, several of these studies should not have been included if we look at current standards for inclusion: for example three studies had less than 50 people included. Still then, goal difficulty showed only correlations with performance of 0.19 to 0.21, meaning a 4% increase in performance. The second study (goal specificity) included 11 studies including children, high school and undergraduate students, and 11 field studies. Upon closer inspection, five (5) studies used less than 50 people. Because of the methodology used and the inclusion criteria, this meta-analysis cannot be considered to be reliable. Another early meta-analysis was conducted by Mark Tubbs (1986) even *before* the formal Goal Setting Theory was built. He found strong effect sizes of goal difficulty and goal specificity, but it should be noted that lab studies obtained larger effects than field studies. One explanation the Tubbs offers is that subjects are perhaps willing to work hard for a difficult goal because “they know that they will not have to do so for any extended period” (p. 80). This makes a lot of sense. Only one year later, Wood, Mento & Locke (1987) published a second meta-analysis in the same journal, but this time they studied the effects of task complexity. They found that goal setting had stronger correlations with easy tasks ( $d = 0.76$ ) and weaker effects with more complex tasks ( $d = 0.42$ ). So these two meta-analyses by Tubbs and Wood et al. offer far from conclusive evidence for the effectiveness of goal setting as a whole. The first points to the weakness of using lab experiments with students and the second points to the weak effects for complex tasks. We had to wait until 2011 for Kleingeld and his colleagues (2011) to conduct a new meta-analysis, including still only 38 studies that they considered to be valid. The researchers were surprised to find that individual (egocentric) goals aimed to maximize individual performance reduced group performance and that group (groupcentric) goals increased performance.

As I said, I reviewed the literature on the different theories on goal settings. I contacted Robert Pritchard by e-mail in November 2017, because I knew he had been invited to write about the long-term effects of goal setting in a book edited by Locke and Latham (2013). He immediately pointed to the obvious lack of longitudinal research on the effectiveness of goals. There is indeed *very* little extant literature on the longitudinal effects of goal setting. Latham and Baldes (1975) found a positive effect of setting specific, high goals for truck drivers in a logging company that lasted for nine months. In private communication between Gary Latham and Robert Pritchard, Latham reported that this lasted for several years. But we have to take Latham’s word for it, as this claim was never described in a peer-reviewed article. Upon closer inspection, their research article consisted of only three pages, which is very unusual. They studied 36 trucks in one company: would setting a specific high goal (reaching a 94% truck net weight) result in an increase? Of course, I read the original study and I find the conclusion that the mere fact of goal setting resulted in higher performance is unwarranted. As it turns out, after the goal setting, the truck drivers started to keep track of their truck weight on their trip sheets. So it might simply have been the awareness of the results (the actual weight of their trucks) that made people load their trucks heavier (from about 65% to about 94%). Also, the company only provided feedback about the results to the entire group, and no individual performance information was provided.

Ann Howard (2013) was the second researcher to study long-term effects, but she used longitudinal data from only one company, AT&T. She found that people who set themselves a goal to get promoted to a specific management level, often achieved this job advancement when she looked at them 25 years later. But it needs to be noted that this is not a goal about performance, of course, only about career advancement. Also, her research is not peer-reviewed (it appeared in Locke and Latham's book). So, this actually leaves us with only one study, the truck driver study showing effects during nine months. This leaves one wondering whether setting specific high goals really will result in sustained effort and sustained increased performance. We simply do not know.

I am not going to repeat my whole review (I reserve that for my own book) but suffice it to say that goal commitment probably is the thing that leads to higher productivity, not goal setting in itself. Luckily for Colquitt, the evidence from different research traditions on goal setting converges into one of the conclusions he draws himself: team goals are better than individual goals.

All right then, let's move to chapter 8 about feedback and progress. Logically, Colquitt cites the meta-analysis by Avi Kluger and Angelo DeNisi (1996), that demonstrated that feedback had highly variable effects on performance: roughly one-third (1/3) of the feedback interventions resulted in positive effects, one-third had no effect, and one-third had a negative effect. But Colquitt omits two important findings that are relevant for the rest of his contentions. First, he fails to mention that such variable effects were found for both positive and negative feedback. Indeed, also positive feedback does not always produce the desired outcomes! Second, on average, feedback interventions had a slight positive effect if they concerned only feedback on tasks. Feedback on personality aspects such as behavior, generally resulted in negative effects. But Colquitt refers to this research to assert that positive feedback is better than negative feedback (p. 119). Then he loses me entirely as he totally goes astray: he cites bad research from the gurus of Positive Psychology (PP). Although he mentions in a footnote at the end of the book (p. 197) that Losada and Fredrickson partially retracted an article after they were debunked by three other researchers, he fails to acknowledge that their research really is biased and has a problematic methodology. The founders of Positive Psychology believe that PP can change the social world towards "*more complexity*" and that this will ultimately *create* a God (I am not making this up!). Some even believe that a collective effort by humans could change the electromagnetic field of the earth. Sonja Lyubomirsky and Barbara Fredrickson have elaborated the ideas of ever expanding positivity, which they called the 'broad and build' theory of positive emotions of the "upward spiral dynamics," labeled by others as the "positivity spiral theory" (e.g., Pérez-Alvarez, 2016). Fredrickson firmly believes that positive emotions improve physical health (she refers to her own Lab experiments) and she also believes that if people experience more social connection with others (she defines love as "*a form of social connection marked by positivity resonance*"), this will do good within society (Fredrickson, 2013a). Drawing on "fluid dynamics," a subfield of physics, Fredrickson and Losada (2005, p. 679) even came up with a "minimum positivity ratio": the proportion of at least 2.9 positive experiences to one negative would have "*repercussions on growth and resilience*," or people would "*flourish*" if they exceeded this ratio. They even considered this a "breakthrough" finding that would change "your life." Seligman enthusiastically spread this 'discovery' (2011, pp. 66 – 68). In their paper with the funny title "*The Complex Dynamics of*

*Wishful Thinking: The Critical Positivity Ratio,*” Brown, Sokal,<sup>3</sup> and Friedman (2013) debunked this ratio. They concluded that there was no theoretical nor empirical justification and moreover, the “*purported application of these equations contains numerous fundamental conceptual and mathematical errors*” (p.1). They demonstrated that the three articles published by Marcial Losada lack any justification for why the “Lorenz equations” from fluid dynamics would apply to human emotions transmitted through “speech acts.” I can highly recommend the article for those readers who are interested in fluid dynamics, nonlinear dynamics (or chaos theory) and a pedagogical explanation of the mathematical equations. In short, in order to use the Lorenz equations, at least five criteria need to be respected, but Losada’s research (1999) met none of these criteria. In a bantering tone, the authors conclude:

*“One can only marvel at the astonishing coincidence that human emotions should turn out to be governed by exactly the same set of equations that were derived in a celebrated article several decades ago as a deliberately simplified model of convection in fluids, and whose solutions happen to have visually appealing properties.”* (p. 8)

Two other articles written by Losada and Heaphy (2004) and Fredrickson and Losada (2005) continued to take this ratio for granted. It is in the latter paper that the critical minimum positivity ratio value of 2.9013 was published. Not only did they build entirely on the flaws in the 1999 Losada paper, they have based their conclusion on “*a series of erroneous, and, for the most part, completely illusory ‘applications’ of mathematics*” (Brown et al., 2013, p. 12).

Immediately following the debunking paper by Brown et al., *Discover* blogger NeuroSkeptic called for a retraction of the Fredrickson-Losada paper and the *Chronicle of Higher Education* covered the story in August 2013. Notwithstanding an initial defensive article by Fredrickson (2013b) and a letter in which she said that the Lorenz equation in fact was only a metaphor, on September 16, 2013, the Fredrickson-Losada paper was “partially” corrected<sup>4</sup> (Fredrickson & Losada, 2013). Other researchers such as Carol Nickerson (2014) expressed their frustration with the lack of scientific rigor such as longitudinal within-person and across-time research, which Fredrickson acknowledged herself (2013b). Nickerson rightly concluded: “*Doing research the wrong way, while delaying doing it the right way ‘until later’, is not acceptable after so many years of discussions of this issue.*” (p. 627).

This did not stop Fredrickson from continuing to make claims based on complex mathematics and difficult words. To understand the terminology used below: *hedonistic* well-being is a term used by only a few scientists-- it means that you are mainly focused on having fun and having pleasant feelings in order to feel happy. *Eudaimonic* well-being means, however, that you experience well-being because you feel that you live a purposeful

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<sup>3</sup> Alan Sokal, a physics professor, has become famous for his publishing of a hoax (“The Sokal Affair”) in an article in the magazine *Social Text*. The article was totally fake, using impenetrable language and flattering statements about the ideological preconceptions Sokal suspected of the editorial board. The article was called *Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity*.

<sup>4</sup> For more information, you can also read the entry on Retraction Watch (<http://retractionwatch.com/2013/09/19/fredrickson-losada-positivity-ratio-paper-partially-withdrawn/>) or the funny article on Nick Brown’s quest: <http://narrative.ly/nick-brown-smelled-bull/>

and meaningful life. In a 2013 paper, Fredrickson and her colleagues claimed that they had found that *“hedonic and eudaimonic well-being engage distinct gene regulatory programs”* that would be associated with distinct forms of immune response (Fredrickson et al., 2013b, p. 13684). This sounds spectacular: could this really be found in our genes or the programs that turn our genes on and off? And would eudaimonic well-being improve our immune response? This claim was quickly debunked in two blogs by health scientist James Coyne who pointed to the simple fact that the two concepts of well-being were so highly correlated that they should be considered to be measuring the same characteristic. He concluded that again, Fredrickson had based her claims on statistical nonsense<sup>5</sup>. The five (!) items in the self-report (!) scale used to measure eudaimonic well-being were also very problematic, for example probing for attitudes such as *“During the past month, how often did you feel that people are basically good?”* or *“...that our society is a good place, or is becoming a better place, for all people.”* To me, it sounds like a naive questionnaire rather than that one that measures how people transcend their own situations or says something about their eudaimonic well-being.

Coyne and his colleagues also criticized this division into hedonic and eudaimonic in two peer-reviewed articles (Brown et al., 2014 and 2016). In their first article they explain how they reanalyzed the dataset using a variety of exploratory and confirmatory factor analysis techniques. These analyses showed no support for the two dimensions of well-being (eudaimonic versus hedonic) but many two-factor solutions could be found. Their regression analysis of all possible two-factor solutions revealed that *“69.2% of these gave statistically significant results,”* but *“only 0.25% would be expected to do so if the regression process was really able to identify independent gene expression effects”* (p. 12705). Their article concluded that there were *“myriad problems with the study of Fredrickson et al, which range from theory and conceptualization, to measurement, and to statistics and interpretation of findings.”* (p. 12709). In 2016 the same authors responded to renewed claims by Fredrickson et al. (2015), again demonstrating that there is no justification for labeling hedonic and eudaimonic wellbeing as two factors. But moreover, this time Fredrickson used another technique that Brown et al. had recommended in 2014, but she did not re-examine the dataset from 2013. So as you might expect, Brown and his colleagues decided to do it themselves and they found precisely opposite effects of hedonic and eudaimonic well-being on gene expression between the 2013 and 2015 datasets. We can be glad that there are serious researchers who act like real Sherlock Holmes to investigate hyperbolic claims by others.

One can only conclude that the minimum positivity ratio is maximum bullshit. Nevertheless, Colquitt explicitly refers to this nonsense.

By now it was clear to me that Colquitt had set out on a mission to promote Positive Psychology. To me, he is one of the members of the school of Platonic Utopians: people adhering to this school *want* to believe that everyone has talent, everyone is intrinsically good, etc. It is just a matter of discovering it, fostering and stimulating it. So that’s why the emphasis should be on positive feedback. If something goes wrong, it is because teachers

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<sup>5</sup> <http://blogs.plos.org/mindthebrain/2013/08/05/improving-your-health-by-pursuing-meaning-versus-happiness/> and a second blog <http://blogs.plos.org/mindthebrain/2013/08/21/positive-psychology-is-mainly-for-rich-white-people/>

don't treat children with respect and don't believe in their talents, it is because leaders are bad leaders, etc. The employee has all the talent and good intentions one can imagine. Only teachers and leaders often are mistaken. Hallelujah, release all criminals from prison.

I am not alone in my criticism of PP. Far from it. In a conversation about the role of positive emotions with June Gruber on Edge.org, we can simply read that Daniel Kahneman opposes the idea of promoting happiness. Daniel Dennett says he finds it troublesome if we try *"to cocoon our children in a world of positive emotions, and shield them from ever really experiencing fear, or loneliness, or boredom."*<sup>6</sup>

Michael Shermer considers the "happiness equation" to be *"nothing more than a slogan gussied up in math."*<sup>7</sup> He considers PP as a movement that resembles much "shallow bafflegab" of the old positive-thinking school that has disguised itself as science. He prefers realism instead of optimism or pessimism.

June Gruber, herself a PP-psychologist (from the University of Colorado) sums up the risk of this kind of PP like this:

*"First, from an evolutionary perspective, negative emotions aid in our survival—they provide important clues to threats or problems that need our attention (such as an unhealthy relationship or dangerous situation). Second, negative emotions help us focus: they facilitate more detailed and analytic thinking, reduce stereotypic thinking, enhance eyewitness memory, and promote persistence on challenging cognitive tasks. Third, attempting to thwart or suppress negative emotions—rather than accept and appreciate them—paradoxically backfires and increases feelings of distress and intensifies clinical symptoms of substance abuse, overeating, and even suicidal ideation. Counter to these hedonic theories of well-being, negative emotions are hence not inherently bad for us. Moreover, the relative absence of them predicts poorer psychological adjustment."*<sup>8</sup>

She states that the idea that sadness is always bad and happiness is always good, is an idea that is "overdue" for retirement.

Although Colquitt *accidentally* reaches some sound conclusions, the book does not at all live up to the promise it makes in its title: there is no triumph of science over myth and superstition - the PM 2.0 Colquitt offers as an alternative to PM 1.0 is built on Utopian thinking. He should really read the criticisms of Positive Psychology, starting with the blogs by James C. Coyne. The problem with this book is that it is impossible for the uninformed reader to distinguish between the good and the bad research Colquitt refers to. He chiefly selected a biased sample to back up his claims about his PM 2.0. His paradigm shift (p. 148) involves at least three shifts that are unwarranted:

- (almost radically) *Shifting from negative feedback to positive feedback*: not only has a meta-analysis demonstrated that positive feedback has about the same amount of negative effects as negative feedback (Kluger & DeNisi, 1996), recent research suggests that negative feedback is probably the best option to increase safety behavior. In 2010, Avraham Kluger published experimental research with Dina Van

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<sup>6</sup> <https://www.edge.org/panel/june-gruber-the-scientific-study-of-positive-emotion-headcon-13-part-ii>

<sup>7</sup> <http://www.michaelshermer.com/2010/01/kool-aid-psychology/>

<sup>8</sup> Retrieved from <https://www.edge.org/response-detail/25498> on February, 13, 2017.

Dijk showing that negative feedback fits better with prevention tasks (“safety first”) and vigilance for safety.<sup>9</sup> This experimental finding was further tested by other researchers. Beersma and her colleagues conducted experimental research with 50 four-person teams and tested whether there would be different outcomes between prevention-focused teams and promotion-focused teams regarding performance. The results showed that (a) prevention-focused teams that (b) worked for a team reward (and not an individual reward) reported higher work engagement and less error tolerance. They also coordinated more effectively and performed better. So if people share their prevention focus and are supported (by team rewards, the appropriate leadership styles, etc.), then performance does not need to suffer. In such cases, safety and performance go together. Although Colquitt refers to the research by Kluger and Van Dijk, he fails to incorporate this in his recommendation.

- *Focus on strengths instead of weaknesses*: this is a recommendation that is also the result of a biased view of reality. Whereas critics and converts of Positive Psychology agree that *balance is key* (one should balance both positive and negative emotions, as they are both very useful and functional), Colquitt one-sidedly promotes building on strengths. Appreciative Inquiry practices are a biased view of reality. Positivity is not always functional. What shall we do with weaknesses such as managerial greed, authoritarian and abusive leadership, manipulative employees, etc.? Wake up -- balance is key, we need both approaches!
- *Get away from competition and promote collaboration*. I agree. But how should we do this? I think it is quite Utopian to think that we can change our innate drive to compete with others. Competition for social or economic status is one of the three meta-motives<sup>10</sup> we share with other social species. And it is an innate feature of the human mind. Colquitt simply says organizations should focus more on the efforts and contributions of teams and should reward this. He is right, but this is simplistic advice and he never offers a path forward for how we can manage our innate drive to compete and get ahead of others. He even fails to see that his advice to “*consider paying top talent and employees with critical skills more toward the top of the market*” (p. 159) is a practice that will totally contribute to competition. And how is he going to change the capitalistic culture that relies so heavily on competition? (he criticizes capitalistic culture) Competition is often heralded as ‘good for consumers’ because it will keep competition sharp and prices low. Colquitt gives no answers, it is just vague idealism without any chance of realization.

So I have to recommend against reading this book, regardless of the fact that there is some good advice in it too. The book contains too many flaws and it is ideologically biased – it reflects a naïve view of humankind, especially in the last chapters promoting the “next generation” of performance management.

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<sup>9</sup> I also liked the comments on the Positive Psychology movement on page 1101: “*various interventions offered by positive psychology.../... may be irrelevant or even debilitating to the performance of prevention tasks.*”

<sup>10</sup> Our need to belong to a group (leading to collaboration) is the second meta-motive, whereas making meaning and trying to predict and explain the world we live in, is the third meta-motive.

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