

The Role of the Matthew Effect in Science

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ABSTRACT

Robert Merton observed that better-known scientists tend to get more credit than less well-known scientists for the same achievements; he called this the *Matthew effect*. Scientists themselves, even those eminent researchers who enjoy its benefits, regard the effect as a pathology: it results, they believe, in a misallocation of credit. If so, why do scientists continue to bestow credit in the manner described by the effect? This paper advocates an explanation of the effect on which it turns out to allocate credit fairly after all, while at the same time making sense of scientists' opinions to the contrary.

For unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath (Matthew 25:29).

1. INTRODUCTION

Can the true significance of a scientific practice be found hidden in a biblical interstice?

Thirty-five years ago Robert Merton observed that, for equally good scientific work, renowned scientists tend to get more credit than unknown scientists. He called this phenomenon the *Matthew effect* (Merton 1968, 1988), after the Gospel of Matthew 25:29, my compulsory epigraph.¹

The workings of the Matthew effect are especially obvious when a more and a less well-known scientist independently and simultaneously make the same discovery, or when a more and a less well-known scientist together make a discovery while collaborating as equals. In such cases, Merton reports, the better-known scientist receives a larger share of the credit for the discovery. More generally,

Eminent scientists get proportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions (Merton 1968, 443).

Merton gives his evidence for the effect on p. 440—all references are to Merton (1968) unless otherwise noted—and Merton (1988), pp. 609–613; also informative are Harriet Zuckerman’s interviews with Nobel laureates (Zuckerman 1977), from which Merton draws heavily.

This essay will consider two possible explanations of the Matthew effect, paying special attention to the following questions. First, is the effect’s im-

1. The same teaching can be found in Matthew 13:12 and in the other synoptic gospels.

pact on the scientific enterprise predominantly good or bad?—Merton says it goes both ways. Second, does the effect violate science’s norms for distributing credit?—Merton says it does.

As Merton points out, those eminent scientists who already enjoy the fruits of success in abundance—Nobel prize winners, in particular—regard the effect as perfidious. That a practice’s main beneficiaries disdain it would hardly seem to count in its favor. Nevertheless, I will advocate an explanation of the effect that makes it look good on both counts above, while at the same time explaining why it is regarded by its beneficiaries and victims alike as a pathology.

2. ASPECTS OF THE MATTHEW EFFECT

The form and scope of the Matthew effect is not especially well specified in Merton’s work. Because certain questions that Merton leaves open are critical to assessing competing explanations of the effect, I will attempt to specify the effect more precisely, or where this is not possible, to identify some important areas of uncertainty.

2.1 Uncertainty about Contribution Let me begin by excluding from the Matthew effect’s realm a phenomenon that Merton appears to include, characterized in the following remark made by a Nobel laureate:

If someone is being considered for a job by people who have not had much experience with him, if he has published only together with some known names—well, it detracts. It naturally makes people ask: “How much is really his own contribution, how much [the senior author’s?]” (444).

The scenario sketched here is not a case of an equal contribution’s being undervalued due to the contributor’s low status; rather, the speaker is asking whether the contribution is in fact equal.

This is likely a common phenomenon. Two scientists collaborate on a piece of work; observers guess that one has contributed more than the other; as a result, they credit one for the discovery more than the other.

Could it be that this is all there is to the Matthew effect—that whenever a senior researcher is given more credit, it is because other scientists infer, though perhaps wrongly, a degree of contribution proportional to seniority? That the Matthew effect is found in cases of independent as well as collaborative discovery suggests not. When the same discovery is made simultaneously and independently, there is no question of who made how much contribution. Yet according to Merton, better-known scientists tend to monopolize the credit just as much when discovery is independent as when it is achieved through collaboration.

Let me therefore distinguish those cases where an unequal distribution of credit is a result of contributions believed to be unequal from those where contributions are presumed to be equal. It is the latter phenomenon that is, I suggest, especially interesting, and that deserves the gloss from the Gospel of Matthew and so the epithet “Matthew effect”. (That said, I should concede that, however it is defined, the Matthew effect may be a heterogeneous affair, of which the effect described and putatively explained in this paper is a large and important part but not the whole.)

2.2 Absoluteness Consider two formulations of the Matthew effect, the relative formulation:

In determining credit, the objective value of your contribution is weighted by your eminence relative to that of your co-discoverers,

and the absolute formulation:

In determining credit, the objective value of your contribution is weighted by your eminence.

(The co-discovery may be either collaborative or independent. You may think of eminence in the absolute sense as eminence relative, not to your co-discoverers, but to the scientific community as a whole.)

The two formulations correspond to observably different versions of the effect. Most important, on the first formulation contributions are weighted by eminence only in cases of co-discovery, whereas in the second formulation they are always weighted by eminence, even when there is only a single discoverer. On the absolute formulation, then, the more famous you are, the more credit you get for *any* discovery; the Matthew effect as described by Merton would therefore be just one aspect of a more general phenomenon, an aspect that stands out because of the proximity, in cases of co-discovery, of a highly salient comparison class.

Merton's characterization does not commit him explicitly to either an absolute or a relative construal of the phenomenon. Of the sort of situation that would decide between the two, he writes "systematic information about such cases is too sparse for detailed study" (443 n. 16). As I will show in section 4, his explanation of the effect implies that it lies nearer the absolute end of the spectrum. If the Matthew effect were purely a result of uncertainty about relative contribution—that is, if there were nothing but the "pseudo-Matthew effect" described in section 2.1—then it would be an entirely relative matter. My own approach will, even more strongly than Merton's, suggest an absolute formulation.

2.3 Retroaction The Matthew effect is *retroactive* if the assignment of credit to a scientist for past discoveries may change as the scientist becomes better known. Merton himself is unclear on the question whether the effect is retroactive. At one point he writes

Should the younger scientist move ahead to do autonomous and significant work, this work *retroactively* affects the appraisals of his role in earlier collaboration. In the words of [a Nobel laureate in medicine] "People who have been identified with such

joint work and who then go on to do good work later on, [do] get the proper amount of recognition” (444).

There is a suggestion, however, that this is true only of the pseudo-effect distinguished in section 2.1. And a page later, Merton says of a Nobel laureate in physics

In the end, then, a sort of rough-hewn justice has been done by the compounding of two compensating injustices. His earlier accomplishments have been underestimated; his later ones, overestimated (445).

This passage implies that the original assessment of the subject’s “earlier accomplishments” is not reconsidered: it remains, as Merton says, “unjust”. Further, a case in which a distinguished scientist—sociologist Dorothy Swaine Thomas—did not have her early work retroactively reassessed in the light of later achievements is cited a page later (446–7).

The evidence for the Matthew effect’s retroactivity is therefore mixed. I will later argue that the effect is not retroactive, while explaining why it may appear otherwise.

3. NORMATIVE NEGATIVITY

A large part of Merton’s original paper on the Matthew effect is given over to recording practicing scientists’ misgivings about the effect. They regard the effect as normatively negative: it is seen not only as holding back the scientific enterprise, but also as perverting generally accepted scientific mores.

Merton’s assertion of normative negativity is based in part on interviews with Nobel laureates conducted by Harriet Zuckerman (1977), in which the laureates express frustration and disapproval when their junior collaborators receive less credit than they themselves receive for work done jointly. A large part of the evidence for their negative attitude is constituted by their efforts to neutralize the effect:

[Eminent scientists] tend to give first place in jointly authored papers to one of their collaborators (446).

At the extreme, they sometimes refuse to coauthor a paper reporting research on which they have collaborated in order not to diminish the recognition accorded their less-well-known associates (446).

There is less that famous scientists can do about the uneven allocation of credit for simultaneous, independent discoveries. But they clearly regard such allocations as unfair:

I'm probably getting credit now, if I don't watch myself, for things other people figured out. Because I'm notorious . . . Well, I may just be saying things that other people have thought out before (445).

Unsurprisingly, the losers, too, disapprove of the effect:

When you're not recognized . . . it's a little bit irritating to have somebody come along and figure out the obvious which you've also figured out, and everybody gives him credit just because he's a famous physicist or a famous man in his field (445).²

If, as it seems, the Matthew effect is regarded not merely as having unfortunate consequences, but as unfair, then there must be some particular scientific norm that is violated by the effect. It is quite clear from Merton's discussion that the norm in question concerns the allocation of credit itself: the Matthew effect is an insidious thing because it infringes the rules laid down by science for determining who gets credited by how much for what, or in other words, because it deranges science's reward system.

2. This and the previous quotation are from the same physics Nobelist, reflecting on different stages of his own career.

Yet, although the scientists surveyed by Zuckerman think of the Matthew effect as a pathology, they are surely at the same time its perpetrators. After all, who allocates credit, whether evenly or unevenly, but scientists themselves? The Matthew effect exists only because the scientists who denounce it nevertheless distribute credit in accordance with the dictates of Matthew 25:29.

What sort of false consciousness is this? An answer will bring considerable insight into the Matthew effect. There are, I suggest, two kinds of responses to choose between. First, it may be that the Matthew effect indeed violates the norms of the scientific reward system, and that scientists recognize and lament this fact, but that they nevertheless persist in the violation. Second, it may be that the Matthew effect does not violate the reward norms—indeed, perhaps it is dictated by the norms—but that scientists fail to recognize this fact and so disparage the effect.

Merton opts for an account of the first sort, albeit one on which the Matthew effect is an unintended consequence of a practice that also does science some good. The principal aim of this paper is to explore an explanation of the second sort, and so to argue that the effect not only directly does good, but does good by following, rather than by contravening, the relevant social norms of science.

4. MERTON'S EXPLANATION

The best scientists, writes Merton, “search out risky but important problems” (459). Attention is a limited resource; it therefore makes sense for individual scientists seeking out important work in their field to bestow relatively more on their more eminent colleagues.

This selective attention has, Merton proposes, an effect that is at odds with science's reward system. If a more and a less famous scientist produce the same piece of work, a reader using fame as a cue for scientific significance

is more likely to have encountered the work by attending to the more famous scientist's name, and so is more likely to remember that name in connection with the work. This in turn creates a tendency to credit the bearer of the famous name more readily for the work. Hence the Matthew effect. Another Nobel laureate on co-authored papers: "You usually notice the name that you're familiar with. Even if it's last, it will be the one that sticks" (444).

On Merton's story, the Matthew effect is a pathology. True, the effect has its roots in a practice that aids the scientific enterprise: it does make good sense to pay more attention to the work of the famous, because such work is more likely to be fruitful. Merton even goes so far as to identify this as the "function" of the Matthew effect in the system of scientific communication (447–8). But it is one thing to differentially attend to the work of the famous; it is quite another to give the famous more credit than the obscure for work of the same quality. Insofar as it is the differential attention, rather than the differential allocation of credit, that makes communication more efficient—and this is Merton's view—the Matthew effect itself must be seen as a mere side effect, and a bad side effect at that. Ideally, science would institute differential attention without differential allocation of credit, and in this way enhance its system of communication without compromising its system of rewards.

Merton himself has emphasized the enormous stock that scientists put on the correct allocation of credit for a discovery, resulting in, for example, fierce priority disputes in which the larger scientific community displays an intense interest in seeing that justice is done (Merton 1957). Is it really so plausible, then, to think that scientists allow all of this to come unraveled because they cannot put out of their mind the way in which they first encountered someone else's work? Does the entire scientific reward scheme founder on the not-so-difficult problem of distinguishing a piece of evidence for a work's importance—the name on the paper—from the importance of the work itself?

I have my doubts. Merton is able to account for the disdain with which the Matthew effect is regarded only too well: the effect is produced by a mechanism that manifestly works against one of the central norms of science. But because the violation of the reward norm is so apparent to all, and the violation in itself brings no benefit either to the violators or to science as a whole, how to explain the persistence of the effect?

5. A NEW EXPLANATION

The Matthew effect is universally derided. Could this derision be an error? Could it be that the effect not only makes a positive contribution to the scientific enterprise, but is mandated by the reward system itself? I will propose an explanation of the effect on which the answer to both questions is *yes*.

The explanation rests on three premises. First is the posit that the reward system in science bestows credit in proportion to a scientist's contribution to society. Second is the notion, not much remarked on in the science studies literature, that the value of a scientific contribution may depend in part on its epistemic standing—on the degree of trust that society can place in a set of scientific results. Third is the observation that, all other things being equal, the credibility of a scientist's results will increase with the scientist's eminence. Let me say something more about each of these.

5.1 Reward as Proportional to Contribution to Society The rewards of science—principally fame, respect, and the material benefits that they bring, such as research grants, laboratory space, and eager graduate students—are distributed, for the most part by scientists themselves, in proportion to the contribution thought to have been made by a scientist's work. The larger the contribution, the more credit the scientist receives from their colleagues, hence the more fame, respect, and material goods.

There is much more to say about the reward system in science, and in particular there is more to say as to what counts as a contribution (Merton

1957; Kitcher 1993; Strevens 2003). For the purposes of engaging with the Matthew effect it will be enough to point out that, when determining the credit to be bestowed on a discoverer, the discovery's contribution to society at large counts for something. The greatness of a discovery depends, that is, not only on the pleasures found in the contemplation of profound knowledge, but in the good that it does for a population not often in a position to fathom its depths.

Medical science provides many examples of this dictum. Pasteur's supreme standing is surely in part due to the many lives saved as a consequence, direct or indirect, of his discoveries. The status of a cure for cancer as a *ne plus ultra* of medical research is due not only to the understanding of the mechanisms of cellular reproduction that it would no doubt involve, but also to the extent of the misery alleviated by such a cure.

An earlier paper on the reward system (Strevens 2003) provides a basis for a more theoretical argument that (and explanation why) credit is indexed to overall benefit to society rather than to some particular benefit. In that paper, I argue that the scientific reward system works, by fine-tuning the allocation of labor among available research programs, to maximize whatever contribution is used to apportion credit. If contribution to society determines credit, then, contribution to society is maximized. Given the further premise that the reward system is responsive to the needs of society as a whole (Strevens 2003, §5.2), you have some reason to expect the allocation of credit to reflect overall good.

5.2 Contribution as Proportional to Epistemic Security That a discovery's value depends on its trustworthiness has been suggested in a different context by Turner (2002, 374):

The total value of the "product" in question, the science, is not only the ideas, the intrinsic value, but the guarantees that come along with it.

This is surely true for some notion of *value*, but does it apply to the particular notion that determines the amount of credit a scientist receives for a discovery?

Further consideration of the medical case suggests that it does. The credit due a researcher for a medical discovery depends in part, I have observed, on the suffering alleviated. But the suffering alleviated depends on the trustworthiness of the results. Suppose that a new treatment has predictable and serious side effects, but that, were the treatment to be effective, it would save the lives of many patients. How widely should the treatment be administered? That will depend on how confident the medical community can be that the treatment does its job. If there is little reason to trust the treatment, then in the light of the serious side effects, it should not be administered at all until further tests are conducted. If there is every reason to trust the treatment, then it can be administered in every case where the positive effects of a cure outweigh the negative side effects. If there is some, but not overwhelming, reason to trust the treatment, it may only be administered in the more desperate cases. Even in cases where the treatment is genuinely effective, its social contribution, measured in patients cured (less side effects suffered), will depend on the degree to which it can be trusted, a purely epistemic property.

In the more urgent medical scenarios, the medical community will assume the burden of testing at once, quickly rendering irrelevant a treatment's initial epistemic status. But in many other cases, including most cases of pure research, it may take many years for a discovery to be either confirmed as genuine or exposed as illusory (among other reasons because the cost of testing itself may be very high, even far from world of clinical trials, as when communities of mathematicians devote their time to checking very long proofs). All decisions based on the discovery in the intervening period will be clouded by uncertainty; the surer the discovery, the less the uncertainty, so the better the decisions, and hence the greater the contribution to

science and society.

5.3 Epistemic Security as Correlated with Scientific Eminence I do not need to argue, I think, that a discovery produced by a scientist with a demonstrated record of success has more initial credibility than a discovery produced by an unknown. I perhaps do need to remark that credibility is affected by much else as well, such as the overt quality of the experimental design. No scientist relies in their judgment of research exclusively on the reputation of the researcher; still, so much in an experiment or other empirical work depends on details that are not or cannot be reported in the journals, that reputation will always be an important factor in determining epistemic security.

5.4 Explaining the Matthew Effect When an eminent scientist puts their name to a paper, they add value to the research reported therein, by making it more trustworthy and therefore more usable. Because credit in science is allocated in proportion to the total value of research, eminent scientists receive more credit for their research by the very token of their eminence—so becoming disproportionately yet more eminent. Thus the Matthew effect, in its very strongest and most biblical formulation.

This explanation suggests a reinterpretation of a phenomenon cited by Merton as evidence for his own explanation of the Matthew effect, namely, researchers' tendency to remember only the reputationally most prominent names on a piece of published research (see section 4). To refresh your memory, another Nobel laureate on this topic:

When people see my name on a paper, they are apt to remember *it* and not to remember the other names (443).

Merton interprets this as a perseverance effect: it is the famous name that first draws your attention to the paper, as evidence for its importance, and so it is the famous name that you remember later. Your mnemonic favoritism,

according to Merton, is entirely unintentional. I suggest that you deliberately focus on the famous name even after you have assessed the importance of the topic because it serves as a kind of epistemic guarantee.

What would Merton have said of this explanation of the Matthew effect? He comes close to invoking the credibility bestowed on work by a famous name as an important factor in understanding the effect (e.g., p. 455 n. 38). But he regards this phenomenon as a kind of appeal to authority of the sort that science should, and does, roundly reject, and drops the subject altogether (457). In response, I submit that what science rejects is appeal to authority *as a final recourse*. Appeal to an authority whose reliability has been empirically demonstrated and is treated as defeasible is not only permissible, but is essential to the everyday workings of science (Hull 1988). Such an appeal is all that is required in the present context.

5.5 Good or Bad? How to evaluate the Matthew effect? My answer comes in two parts. The first part is an answer to the following question: is the Matthew effect good or bad by the lights of scientific norms? The answer is that it is good, because it conforms to the rule laid down by science's reward system: allocate credit in proportion to contribution. Indeed, it is a necessary consequence of this rule, given the relation between reputation and reliability.

The second part is an answer to a somewhat different question: is the Matthew effect good or bad by the lights of society as a whole? In this case the answer must be provisional. The provision is this: suppose that a contribution-based reward system—a reward system that allocates rewards in proportion to contributions made—attracts different numbers and kinds of researchers to different kinds of problems so as to maximize the sum total of contributions made, as argued in Strevens (2003). Then the Matthew effect may be seen as helping to maximize science's contribution to society. The outsize rewards reaped by eminent scientists help to attract those scientists to problems where (among other things) their ability to guarantee

a discovery will do the most good; for society at large, then, the Matthew effect is a good thing.

Scientists' individual efforts to counteract the effect are, if this is correct, quite possibly an impediment to the scientific enterprise. The Nobel laureate who withholds her name from a list of authors in order to shield her younger collaborators from the Matthew effect thereby fails to augment the credibility of the research in a way that she might, and so makes the research less valuable, at least in the short term, to the rest of the world. (For evidence that scientists appreciate this problem, see Merton, p. 448.)

6. ASPECTS OF THE MATTHEW EFFECT REVISITED

On the supposition that my explanation of the Matthew effect is correct, let me consider and attempt to explain three features of the effect: its normative negativity, its absoluteness, and its retroactive aspect.

6.1 Normative Negativity Revisited Why is the Matthew effect regarded by science's practitioners as a pathology? Merton can give a straightforward answer: scientists correctly see that the effect is a perversion of the scientific reward system. On my explanation, by contrast, the Matthew effect is not a perversion of, but rather a consequence of, the normal operation of the reward system. How then to explain the suspicion with which it is regarded?

There are two elements to the explanation. First, consider the perspective of one of Zuckerman's Nobel laureates co-authoring a paper with a junior colleague and seeing the colleague receive well under half the credit for the work. The laureate subscribes to the fundamental tenet of the scientific reward system: allocate credit in proportion to contribution. He also recognizes that a discovery's contribution depends in part on its epistemic security. From his point of view, however, the epistemic security of the work is not increased by his own advocacy, or for that matter, by his co-author's advocacy, but only by the nature and execution of the work itself. The sole

apparent contributions to the work are made in virtue of the two contributors' talent and effort, and these are by assumption equal. From the interior of the research program it may seem, then, that the unequal allocation of credit is unjust.

Second, consider the perspective of the historian or sociologist of science looking back at a case of co-discovery. The epistemic status of the discovery has long since been secured. Any epistemic guarantees conferred in virtue of a discoverer's reputation have been superseded, and the positive effects of those guarantees may be lost to history. *Sub specie aeternitatis*, or *scientiae* at least, the effect of co-discoverers' differing levels of fame are invisible, and so they appear to have made an equal contribution (and in a certain sense, this may be true; see the discussion of retroaction below). An earlier unequal allocation of credit by the discoverers' contemporaries will, again, seem unjust.

6.2 Absoluteness Revisited If the Matthew effect is generated as I have conjectured, then it is absolute in the sense specified in section 2.2: a researcher's scientific contributions are always weighted by their absolute level of eminence, whether or not there are any co-discoverers with whom to share the credit for the discovery. This prediction is testable, though as already remarked, in 1968 Merton thought that insufficient evidence had been uncovered by sociologists of science to perform the test.

6.3 Retroaction Revisited Merton tentatively suggests that the Matthew effect is retroactive: as a scientist's reputation grows, their early scientific contributions are reevaluated and reweighted by their newfound eminence, so that the credit they receive for their early discoveries increases as they become more famous (section 2.3). If the explanation of the Matthew effect advocated in this paper is correct, the effect ought not to be retroactive, at

least not in the typical case.³

Why not? As I remarked earlier, any genuine discovery is eventually shown to be such by experimentation and by related theoretical advances. Over time, the epistemic security of the discovery will come to rest entirely, or almost entirely, on these factors, rather than on the status of the original discoverer. The period of time over which a discovery makes a social contribution may, then, for expository simplicity be crudely divided into two parts: an initial period, when the researcher's reputation makes a difference to the discovery's credibility, and so to the contribution made, and the rest, after which the discovery is confirmed and reputation no longer matters.

By the time an unknown discoverer becomes famous, the initial period is—precisely because independent confirmation and fame are linked—usually over. The discoverer's reputation, then, is no longer in a position to affect the credibility of, hence the usefulness of, their discovery. Thus whatever contribution was made by their reputation, was made when it was low. That it is now high does not change the size of this past contribution, so does not change the credit allocated for the contribution. In other words, once your brilliant discovery is independently verified and you join the scientific elite, it is too late for your new reputation to enhance the contribution made by your discovery. Thus, because credit is proportioned to contribution, there ought to be no reassessment of the credit due to you for your discovery in the light of your new status.

Real and widespread retroactive reevaluation, then, would count against my account of the social mechanics of the Matthew effect. As already noted, Merton himself seems unsure whether the effect is retroactive—he runs together genuine retroaction with the non-retroactive scenario in which the

3. There is a sense of retroaction on which the Matthew effect is never retroactive: new-found fame cannot change the credit assigned for that part of a discovery's social contribution made before the rise to eminence, because it cannot retrospectively change the size of the contribution. The argument advanced in the main text shows that the Matthew effect is usually not retroactive in any other sense, either.

impact of the Matthew effect over a lifetime sums to zero along the trajectory from obscurity to celebrity.

There is enough uncertainty about retroaction, however, that it cannot, you might think, be mere illusion. You might suspect, in other words, that if the Matthew effect is not literally retroactive, it certainly creates, in some circumstances, the appearance of retroactive reassessment.

I propose that my account of the effect indeed predicts the appearance, though not the reality, of retroaction. There will be, I suggest, a kind of “washing out” of credibility effects over the useful life of a discovery. Consider again the two stages in the history of a discovery’s contribution to society, the first in which reputation makes a difference to contribution, the second—following the discovery’s independent verification—in which it does not. Since the former period is often quite short and the latter period is unbounded (unless the discovery is entirely superseded), as time goes on the epistemic security conferred on a discovery by the researcher’s reputation accounts for a smaller and smaller proportion of the total, cumulative social contribution made by the discovery. Since credit is pegged to total social contribution, the Matthew effect will proportionally speaking make less and less of a difference to credit received.

Thus the difference between, for example, the credit allocated to two co-discoverers, one eminent and the other not, will go to zero, as a proportion of the whole, as the years go by. Meanwhile, over the same period of time the less eminent co-discoverer will presumably acquire scientific distinction. This correlation between, on the one hand, the disappearance of the credit differential between famous and not-so-famous co-discoverers, and on the other hand, the upwards trajectory of the junior co-discoverer’s career, may very well be mistaken for retroactive reevaluation.

Washing out is not retroaction, but it holds out much the same hope to scientific hath-nots: it assures even the most marginal figure that they may eventually entirely overcome the negative impact of the Matthew effect

on scientific credit received. An extreme example is Mendel, who conferred almost no epistemic security on his now-famous genetic experiments, and who as a result made almost no contribution to science in the following decades.⁴ Once Mendel's discoveries became known and trusted, however, they began to bear fruit, and Mendel reaped the usual scientific rewards for greatness—posthumously, as befits a posthumous contribution. His initial obscurity, at first a terrible disadvantage, simply became a part of the legend.

Yet Mendel was lucky. In cases of this sort, there is always an open question as to who made the contribution. Was it the original discoverer, or was it the scientists (in Mendel's case, the re-discoverers) who confirmed the discovery and so conferred on it the epistemic security it required in order to begin to do good? The social dynamics are complex: quite often, the original discoverer gets the credit, but sometimes, especially when the discovery is incomplete, things go the other way. Returning to Zuckerman's Nobel laureates one last time:

E., who had the idea, went circling round to try to get an experiment for . . . Nobody would do it and so it was forgotten, practically. Finally, A. and B. and C. did it, became famous, and got the Nobel Prize . . . If things had gone just a little differently, if somebody had been willing to try the experiment when E. suggested it, they probably could have published it jointly and he would have been a famous man. As it is, he's a footnote (445).

Sometimes, science plays the angry god—

And cast ye the unprofitable servant into outer darkness: there shall be weeping and gnashing of teeth (Matthew 25:30).

4. There is more to this story than a lack of epistemic security: Mendel's results were brought to the attention of too few, and the wrong, scientists. But reputation played a part there, as well.

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