Promoting Integrity as an Integral Dimension of Excellence in Research

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Deviance in science: a criminological analysis

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Table of Contents

1. Introduction

This deliverable is part of Work Package II of the Promoting Integrity as an Integral Dimension of Excellence in Research (PRINTEGER) research project. Titled *What is integrity? Multidisciplinary Reconnaissance*, Work Package II is devoted to the analytic reconnaissance of research integrity and scientific misconduct. This report contributes to this reconnaissance by conceptualizing deviance in science from a criminological perspective. In this chapter we aim at discussing deviance in science or scientific misconduct from a criminological perspective. A criminological approach focusses on the complexity of deviant behavior, as well as on the problematization of this behavior as deviance, and how this is part of the social reaction to it. In order to understand deviance in science we need to deconstruct the several dimensions that shape this paradoxical object (Pires, 1993).

As criminologists we cannot look at misconduct in science as if it was a naturally given or ontological entity. On the contrary, we need to take into consideration the social processes that problematize scientific practices (behaviors) as not acceptable or deviant: it is precisely through these social processes not only that the figure of "scientific deviance or misconduct" is constructed, but also and at the same time, that practices of social reaction and control, and possibilities for (early-) intervention, emerge. Therefore, we will address both the phenomenon of so-called misconduct as well as the social reaction it calls into being. Scientific misconduct indeed refers to its classical forms, well known as FFP, meaning Fabrication, Falsification and Plagiarism (FFP). But as we will see in this contribution, scientific misconduct refers to a much broader category of researchers' behavior when *doing science*.

There is a clear and increasing attention for the phenomenon of misconduct in scientific practices today. This growing attention can be noted especially in the different developing policies and reactions to misconduct coming from funding bodies, research institutions, media, policy makers and government as well as civil society. Criminology strives to challenge mainstream perceptions and "questions what we believe to be true about crime, why we might believe this and how crime is shaped by wider social factors" (Carrabine et al., 2014, p. 5). As stated by Becker in his work *Outsiders*: "All social groups make rules and attempt, at some times and under some circumstances, to enforce them. Social rules define situations and the kinds of behavior appropriate to them, specifying some actions as 'right' and forbidding others as 'wrong'" (Becker, 1963, p. 1). To state it boldly, the emerging definition of the phenomenon of scientific misconduct is intimately intertwined with the rise of a discourse on social reaction and intervention with regard to the problem defined. Hence, we will try to grasp precisely the dynamic and double character what is being coined as "scientific misconduct".

In order to do so, we need first to understand the specificity of *doing science*. So, before addressing deviance in science as a criminological object, we need to describe in short which constraints define (the different) scientific practices in a generic way (2. What is science). At least two aspects are relevant in respect of scientific misconduct as a problem: the supposed norm consensus in defining scientific practice; and the diversity of scientific practices. Only subsequently, will we address the question *What is scientific deviance?* (3.). Today, scientific misconduct refers to a much broader category of transgressions in comparison with the classical FFP. Deviance, as a criminological concept, refers to a much wider and looser category of behavior, than punishable offenses qualified in a penal code for example, since it includes a wide range of behaviors that deviate from a dominant culture's normativity, being "the aggregate of social behaviors, practices, acts, demeanors, attitudes, beliefs, styles or statuses which are culturally believed to deviate significantly from the norms, ethics, standards and expectations" (McLaughlin & Muncie, 2006, p.

126). Scientific misconduct operates as a container concept, blurring the boundaries of acceptable and nonacceptable behavior, and in doing so fostering the net widening of social reactions; "widening the net, thinning the mesh" (Cohen, 1979).

Consequently, deviant scientists can be regarded as committing acts in conflict with the norms assigned to their social and organizational roles. Criminological studies are characterized by a multidisciplinary hybridity, implicating that when studying crime (and the derivations of it, such as social deviance), micro-meso-and macro levels intersect (applied to scientific misconduct, see Rita Faria, 2013). A criminological perspective will therefore take into account, on a micro level, psychosocial approaches focusing on models of personal performance, (neuro) psychological explanatory models, individual perspectives and personal motivations. These individual features and processes take place in a complex context, in this case the organizational setting, requiring a broader understanding that goes beyond a sole focus on the individual.

After that we need to consider (4. Doing science in a rapidly changing environment) the contemporary context of *doing science.* The evolution of science towards a knowledge economy has brought along relentless pressures on the generic constraints of science, and the publish or perish requirement is often considered to be an important factor in explaining scientific misconduct. It is clear that the knowledge economy and notions of New Public Management influence and condition the ways in which science must be done, and is therefore often considered to be part of the constructing dynamics of scientific misconduct as a problem and the emerging intervention strategies of social control connected to this.

Therefore, in Section 5 we address how the social reaction to scientific fraud and misconduct was problematized and how this can be understood from a criminological perspective. Besides problematizing internal mechanisms of social control within scientific practices, a formalization of external regulations of scientific practices (ethical committees, bodies of integrity, prevention, early-intervention, etc.) can be noticed. So, as a counterpart, we do witness the rapid growth and expansion of institutional intervention policies driven by a strong ethical mindset in science. This *ethical turn*, as we would like to call it (6. An ethical turn in science), reflects nicely how the introduction of integrity- and ethical intervention programs in contemporary science policies are closely intertwined with the definition scientific misconduct as a problem of integrity.

2. What is science?

Science can be generically characterized as a collective practice aimed at producing robust, rectified and reliable knowledge. Two interwoven aspects play a constitutive role in this. On the one hand, the produced knowledge should comply with the requirement of *objectity* as defined by Latour (contrary to the distant attitude that is called objectivity and that is characteristic for judges and jurists, Latour, 2002). Scientific knowledge should correspond accurately to that which its object allows to postulate about it. In other words, that which is postulated by scientists may not be provoked, imposed, manipulated, suggested or enforced by them. The object of scientific attention thus validates or invalidates the claims scientists make about it (see e.g. Gutwirth, 2010; Latour, 2002; Stengers & Bensaude-Vincent, 2003). *Ideally,* scientists succeed in granting the investigated object the power to determine the way it *must* be described. In this context, the scientific experiment can be considered an emblematic situation. However, in any other than the natural sciences, this will raise many complex questions (cf. Stengers, 1993).

On the other hand, no robust knowledge can be developed outside the collective of scientists working on the same objects and questions. It are those *peers* who will test, improve and validate the proposed scientific knowledge. This process of mutual testing grants robustness, reliability and validity to scientific claims, and can eventually, when the controversy has died down, result in stabilization. This stabilization is, however, only temporary: a scientist can always get the ball rolling again with a new claim, starting the process of research, *claims* and controversy all over again (Latour, 1988, 2001; Stengers, 1993, 1997; Stengers & Drumm, 2013). The proposals from an individual scientist should therefore always be understood as a contribution to the dynamics and production of the collective (Stengers & Drumm, 2013), while the next step in the research also depends on the reaction of competent *peers* (Polanyi, 1962)[1]. Such a concept of science implies and demands that scientists make their actions completely transparent and controllable in order to allow the *peers* to do their job thoroughly, which, in turn, suggests a fundamental mutual level of trust, or inversely, the absence of distrust between scientists. This type of fundamental *gentle(wo)men's agreement* is characteristic for the collective practice of scientists. From this point of view, science is strongly linked with concepts such as *commons* and *commonality*: the production of scientific knowledge is a collective responsibility of all those who are united by this practice (Freeland Judson, 2004, referring to Merton's "communism", R.K. Merton, 1942; see also Gutwirth & Stengers, 2016). For sure, individual scientists can claim their personal role and merits in a specific development, but the knowledge that is produced always remains cumulative and shared. No scientist starts from scratch.

Ultimately, science is always associated with undertaking a specific risk., the questions raised by scientists are always "open" and they always take on a double challenge: they have to be pertinent and interesting from a scientific point of view (for the *peers* in the field *and* with regards to the object); and success or triumph are never a certainty beforehand. This is what is called the "production of knowledge": producing knowledge that did not exist before. On that basis, an important distinction should be made between the "modern sciences", which are aimed at the production of new knowledge and the "cameral sciences" aimed at informing and supporting public or private decisions (cf. Stengers & Drumm, 2013).[2] The distinction is important in the light of the discussion on scientific misconduct today, in at least two ways. First, the so-called "internal" mechanisms of social control (peer review and replicability) are intimately rooted in what defines in a generic way the production of scientific knowledge, and therefore modern scientific practice. Without this intrinsic social control there would be no modern sciences. Second, because "cameral sciences" are confronted with other constraints beyond, and sometimes even conflicting with the generic constraints described above. With this cameral model problematic scientific practices and behavior became part of another regime or "dispositif". Moreover, as we will see further on, institutionalized scientific practices are more and more pushed towards this cameral or *fast* model of doing science.

But there is more, again complicating the picture. Even though, generically speaking, science can be described as the collective production of reliable, rectified and robust knowledge, the scientific *practices* take on various and distinct shapes. In light of the diversity of subjects (the universe, nature, the earth, matter, numbers, legal documents, concepts, animals, communities, brains,

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[1] It is important to keep in mind that today the significance of the concept of *peer* generally refers to an evaluator or expert assessing the work of others from a position of power and whose assessment has consequences for the further evolution of their research (evaluating publications and fund requests, giving advice on appointments and promotions, ...). The confusion between both meanings of the term *peers* is far from innocent. After all, the evaluator is usually unapproachable and "unaccountable" because he or she is anonymous. In that sense, he/she is in direct opposition of the peer as a co-producer of knowledge (cf. Judson 2004, (p.p. 244-286) who is openly destructive for peer review from a position of power, and who, from that perspective, defends *open access* publications).

[2] In those cases, regardless of whether we are dealing with bioengineers, engineers or criminologists, the *experts* (who are often recruited among scientists) are confronted with questions with a political or economic scope (e.g. "is this product a danger to human health?"). In that sense, the experts participate in policy making *outside of* science and provide information by appointment, as a result of a contract which is not "open ended" at all. All practices aimed at "technology transfer", innovation, spin-off, patent acquisition, consultancy and policy support fall under this definition, and as such they fall outside of our present outline of what is science. In these cases, after all, the collective production of reliable and robust knowledge gives way completely to the commissioned realization (in a manner only similar to science) of extra-scientific objectives like supporting the economic and/or innovation policy of a country or region, legitimizing social measures, determining limitations in the fields of human health, the environment, food quality, ionizing radiation, noise pollution, etc.

emotions, etc.) and the associated diversity in methods, this is actually self-evident. However, it is usually not incorporated in the broad debate about fraud and the reactions to said fraud (the KNAW 2012 report, however, does this).

In light of the discussion on scientific misconduct it is important to stress that only experimental sciences can provide *evidence*, and this to the extent where they relate to "recalcitrant" or indifferent objects (Stengers & Bensaude-Vincent, 2003). This kind of proof is only possible by way of the complete control of the parameters and the elimination of "noise" and contextual influences. In consequence, anything that goes beyond the experimental setting, loses reliability and robustness due to the complexity of the world (Callon, Barthe, & Lascoumes, 2014; Stengers & Bensaude-Vincent, 2003; Stengers & Drumm, 2013). Hence, this type of conclusive experiments is exceptional. A lot of objects do not behave like marbles (not even marbles)[3]. Therefore, they cannot be transposed unproblematically in an experimental setting or lab, precisely because they *cannot* be indifferent to the *dispositif* in which they end up, e.g. pigeons and rats, let alone people[4]. In those cases, the experiment degenerates into the exercise of power. After all, it creates a "straitjacket" influencing, nudging, channeling and therefore modifying or even mutilating the studied object. Besides this, there are many complex phenomena (like a healing process, the danger of tobacco, the effect of GMO's on the environment and agricultural organization, climate change) that can never be reproduced in their totality in a laboratory environment. When doing so, the experimental setting would be so reductionist, that it would be unable to provide any *evidence* (Callon et al., 2014).[5]

Consequently, no *proof* is provided in the majority of sciences: the produced knowledge is corrected and verified in other ways, with bigger differences between experimental, terrain, empirical social and interpretative sciences. However, even in these different scientific practices, the generic *constraints* remain double: the studied object (forests, animals, people, communities, historical documents, formal legal sources or literary texts) obligates the scientist ("objectity") and the competent colleagues (peers) to closely examine, check, refine or refute the proposed knowledge or insights, whereby it will gain in robustness and reliability ("mutual control"). These practices will not deliver any conclusive proof, but rather insistency and consistency of observations (see Gutwirth & Christiaens, 2014). This plurality of scientific practices further complexifies the issue of scientific misconduct, especially when trying to define or even codify behaviors into reproachable facts of misconduct.

3. What's in a name: deviance in science?

When analyzing the literature on scientific misconduct it appears that authors often hastily browse over conceptual debates of misconduct in science, after which they move towards the core arguments of the article, leaving behind definitional ambiguity, as if the phenomenon or concept of scientific misconduct is evident and clearly delimited. Nothing, however, could be further from the truth. Despite multiple attempts at defining the norms of science (e.g. R.K. Merton, 1973),

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[3] The question we can ask ourselves is if in practice we can truly provide proof or evidence, even when dealing with indifferent objects like, let's say marbles? Do we know everything there is to know in for example quantum physics, where scientists are developing theories working with the smallest particles known today?

[4] Following the infamous experiments by Milgram (obedience research) and Zimbardo (Stanford prison experiment), it has become obvious that no human, like a marble, can make it through an experimental setting untouched and indifferent, and maybe a fortiori when people participate with the best intentions (Stengers & Bensaude Vincent 2003, 152-154; Stengers 2013, 65) – albeit for a fee.

[5] Of course, this does not mean that our scientific knowledge on these complex processes does not become more robust and reliable along the way. On the contrary: the accumulation of an increasing amount of consistent data allowing for the supposition of every greater likelihoods. But, that is different from the certainty of evidence and proof (Stengers 2009, i.t.b. on p. 86).

there remains no general agreement on the fundamental norms of science (Kalichman, Sweet, & Plemmons, 2014; Mcfarlane, Zhang, & A., 2014).

Further, it could be said that attempts to define deviance in science and/or scientific misconduct have often been too perfunctory, denying social constructionist influences by those defining problematic scientific behavior. As Brian Martin wrote in 1992: "The social definition of fraud is one which is convenient to most of the powerful groups associated with science. This includes government and corporate sponsors of scientific research, and the scientific community itself, especially scientific elites" (Martin, 1992, p. 84).

When scientific fraud is detected or comes to the surface, a dominant view is being represented, one that addresses consensus when it comes to integrity and fraud in science. It is what a Dutch criminologists once described as the consensus model (Bianchi, 1980). The presumption of (a universal) consensus about scientific norms or values is not clear in itself in our contemporary science practices, contexts or even disciplines (Gutwirth & Christiaens, 2014). As we have pointed out in the previous section, there are some fundamental constraints to the production of scientific knowledge, which can be seen as the basic rules when doing science. Hence, scientific misconduct can be understood as an infringement of these constraints. Therefore, the context of knowledge production is very important, especially at the epoch of fast and cameral science practices.

3.1 A definition of scientific misconduct beyond FFP

The phenomenon of deviance or misconduct in science is complex and it encompasses a wide range of "improper" behaviors and activities, leading to very different possible interpretations. Due to ambiguous and controversial definitions, distinctions between fraud, misconduct and errors cannot always be made systematically, consequently these concepts are often only superficially explained and deconstructed. Due to the fact that there is no consensus on what can be called questionable, unethical, objectionable or deviant, one must not simplify the complexity of the system in which the production of knowledge takes place. As Stankovic argues: "the complexity and the confusion of defining misconduct are illustrated through the existence of various types of behavior that might be defined as some type of scientific misconduct, arguably with different degrees of culpability" (Stankovic, 2004, p. 982). Science, but also concepts such as fraud, misconduct and deviance can be understood in a way that goes beyond the assumption of consensus, taking into account the plurality that is inherently part of science.

On a general scientific level, there seems to be consensus on Falsification, Fabrication and Plagiarism (FFP) as clear forms of violations of the norms of science. These rather classical forms of scientific fraud can indeed all be closely linked directly to the generic constraints mentioned above. There is no confusion about the fact that presenting results that are completely fictive and imaginary is appropriately labeled as fraud (fabrication). Idem ditto for faking and massaging research data (falsification). Plagiarism is the appropriation of another person's ideas, processes, results or words without giving appropriate credit (Resnik, 2010; Schuyt, 2014a, 2014b). Plagiarism in science does not result in much social harm, yet those who plagiarize fail to contribute to a scientific practice that is inherently based on mutual transparency. Schuyt sees plagiarism as an infringement of the rules of science – *credit is given where credit is due* – yet it is not considered exactly the same as a transgression of intellectual property rights (Schuyt, 2014b, p. 100). Where fabricated and falsified data could result in serious social harm and undermine trust in science, plagiarism has consequences for the prestige, output or recognition of researchers and their work. In other words, plagiarism erodes the informal reward system of science (Schuyt, 2014b). Plagiarism contradicts the basic constraint of science, which is the advancement of scientific knowledge. Giving credit where it is due and correctly acknowledging where the scientific knowledge is coming from is an absolute necessity of everyday scientific practice. Therefore, reproducing texts, words or citations without giving credit is considered to be unquestionably wrong.

But, to a certain extent Questionable Research Practices (QRP) have been added to the discussion on deviant scientific behaviors and are often located on a scale somewhere between FFP and Responsible Conduct of Research (RCR) (Steneck, 2006). Whereas people agree on classifying the FFP as forms of high profile misconduct, much less consensus exists about the allocation of QRP as misconduct in science. Some authors consider misconduct as deliberate violations of methodological procedures through forgery and fabrication of data or, dishonest behavior committed in a professional context characterized by the intent to deceive (Ben-Yehuda, 1986; Fanelli, 2009; Fang, Steen, & Casadevall, 2012; Goodstein, 2010; R.K. Merton, 1973). Others prefer to use slightly broader narratives, including forms of data-fabrication, such as the selective reporting of results (*cooking*) and directing data so that the results do not reject the hypothesis (*trimming*) (Babbage, 1975 cited by Ben-Yehuda, 1986). Thomson (2002) interestingly conceptualizes deviance in science very broadly, and adds behaviors that deviate from the normative framework of academia, such as: "inefficiency; incompetence; negligence; lack of respect for properly constituted institutional authority; abuse of office; arbitrariness; patronage; nepotism; favoritism; corruption; partisanship; factionalism; discrimination; conflict of interest; circumvention or violation of properly established institutional principles, policies, procedures, and practices; disregard for the truth and the free spirit of inquiry; stifling academic freedom; abusing academic freedom; fraudulent research; engaging in any other conduct not consistent with the formal and informal missions and goals of the university; systematic replacing or supplanting of formally declared goals and objectives by fundamentally inconsistent informal ones; basing key personnel or related decisions on factors extraneous to the principle of merit; or performing other unsanctioned or prohibited institutional activity" (Thomson, 2002, p. 76).

The more or less subtle forms of misconduct such as for example the removal of unwelcomed data in order to report positive results parallel with the hypothesis – or authorship related practices such as *ghostwriting*, *editorial ghostwriting*, *honorary authorship* and *self-plagiarism* - could be considered misconduct in some disciplines, but then again they are sometimes accepted in other disciplines and contexts. Not recognizing an author when there has been a significant contribution, or giving credit to authors whose contribution was very limited or nihil, can be considered forms of misappropriation of credit (Schuyt, 2014a). Further, reporting data and results of a single study into multiple articles (salami-slicing), and recycling parts of a publication in several journals without proper citations (self-plagiarism) can be seen as inappropriate conduct and deviance in science (Schuyt, 2014b). Other behaviors such as the exploitation of other people's work (Martin, 1992), bias in the peer review process (Becher & Towler, 2001) and conflicts of interest can also be added to this list (Bird & Spier, 2005; R. Faria, 2015; Lipton, 2004). The *grey zone* situated between misconduct and responsible conduct in research remains the subject of intense debate.

All the above mentioned practices or behaviors (beyond FFP) are not necessarily listed under a common denominator of 'scientific fraud'. Some do cause an erosion of the core principles and constraints of doing science and are therefore good examples on which behaviors can be considered as violations of scientific integrity (Gutwirth & Christiaens, 2014; Schuyt, 2014a, 2014b). Others refer not so much to *doing science* but rather to being an academic (which is not necessarily the same) and taking up roles that are at a certain distance of *doing science*.

3.2 Misconduct and mistakes

When discussing scientific misconduct an important distinction is made between actions intended to deceive and actions that unintentionally deceived. The latter is captured in available literature as honest errors, reputable errors or *sloppy science* (La Follette, 1992; Resnik, 2010; Resnik & Steward, 2012; Zuckerman, 1977). When authors intentionally cheat, they want to achieve their ambitious goals and create a successful image of themselves, obtain more power, grants, career advancements and sometimes even monetary gain. According to La Follette, the *intention* to deceive forms a fundamental element to label an act as misconduct. Mistakes or errors, though threatening to one's career and reputation, must be distinguished from fraud and should therefore not provoke institutional investigation (La Follette, 1992). Sloppiness could creep into all stages of research. It could even be said that mistakes are unavoidable, although research result are usually being presented in a purified way, not representing the chaotic practice that science can be (Latour, 1988).

The difference made between cases of misconduct and cases of sloppy science is then based on the intention of the involved researchers. Such intention refers also to an important dimension of what in a penal context would be culpability, as distinct from the failure or fault in civil liability law. Since, science does not have a formal special criminal code, nor a crime detecting unit, neither a judge that may decide upon the presence of an "intent to deceive", the use of such criterion for distinguishing between scientific misconduct and sloppy science and errors.

Not only a scientist's motivation but also accepted norms in one's discipline play an important role in deciding over misconduct or honest errors. Questionable practices - either with or without the intention to deceive - do inevitably conflict with the constraints of science and form a threat for the reliability of science as a practice and as a base of knowledge. An even greater danger occurs when these practices become necessary for survival in the scientific world, thereby creating an environment where flirting with certain behaviors on the edge of acceptability has become the norm rather than the exception. When it comes to the consequences for science and its generic constraints, whether acts are committed with the intent to deceive, resulting from sloppy science, or simply part of common suboptimal practices is of less importance, as all such acts damage the sciences and the reliability and quality of the knowledge produced (Gutwirth & Christiaens, 2015).

3.3 Scientific misconduct and white collar crime

As a form of 'elite' deviance, fraud in science requests a study of upper world deviance similar to domains of nonconformity in politics, in the corporate context, in governmental institutions or in the medical world. Should we consider the evolutions in the academic institution as so distinguishable from the wider societal evolutions in which competition and rivalry have become emblematic of individual advancement (personal, career, education, …)? Or should we recognize the fact that science is experiencing similar evolutions and pressures as other domains of society where (*professional*) integrity has experienced tension?

Deviance in science takes place within a professional framework, where organizational culture, organizational structure, the power structure of an organization, and the specificity of this organization plays a significant role in understanding the structure of science, and the context in which certain behaviors take place. Scientists can and have to take up different roles (task, responsibilities, etc.) in being part of a university or research institution. Looking at organizational dynamics thus means going beyond the individual him/herself. From this perspective we can say that misconducting scientists "benefit from and/or abuse of a professional and/or trusted position of some seniority or status – following from being an employee in an organization and-or recognition of qualifications by professional or other bodies" (Carrabine et al., 2014, p. 238).

In this context we can point out the possibilities for exploring white-collar crime with regard to deviance in science. When speaking of white collar crime, one cannot go on without mentioning Edwin Sutherland, the founder of the concept (Sutherland, 1949). He initially defined white collar crime "as a crime committed by a person of respectability and high social status in the course of his occupation" [6] (Sutherland, 1949, p. 9). Some characteristics of white collar crime focus on the individual status of the offender whilst others take into account offences committed within

[6] We must, however, address the many definitional issues with the concept of white-collar crime, for example discussions of whether or not white collar crime can actually be considered as 'crime' (see for example the discussions mentioned in: Croall, 2001)

occupational roles (Croall, 2001). To a growing extent it is the latter that gained more popularity in criminology, due to growing criticisms when it comes to defining crimes starting from the characteristics of the offender (Braithwaite, 1985b).

Deviance in science can, in some ways, fit within descriptions of white collar crime for several reasons. First, the administrative procedures (Sutherland, 1949) – a sort of "infra" law - in which 'fraud' in science is dealt with (and not by way of penal law, for example) is little different from other occupations. Further, we can ascertain difficulties in assessing the prevalence due to a lack of official statistics[7] (Croall, 2001). Power structures do also play an important role. Brian Martin, for instance, argues that scientific elites[8] are able to dominantly steer and influence not only science policy making, but also a social definition of fraud, a definition that is the most convenient for the powerful groups associated with science (Martin, 1992). Additionally, Ben-Yehuda also mentions some similarities, arguing that – just as in white collar deviance – fraudulent research can result in a considerable amount of social harm and damage victims. He goes on by mentioning the weakness of science in detecting fraud, and that punishments are not very harsh (Ben-Yehuda, 1986). Furthermore, in both white collar crime and deviance in science we can speak of conceptual ambiguity and dissensus when it comes to defining and labelling behaviors as misconduct.

The importance of the notion of trust is yet another key feature and similarity between scientific deviance and white collar crime. The violation and manipulation of the norms of trust – of disclosure, disinterestedness, and role competence – represent the modus operandi of white collar crime (Shapiro, 2001, p. 25). It is precisely this trust that is "the most important pillar on which science rests" (Drenth, 2005, p. 9). Scientific knowledge is trustworthy precisely because it is produced in accordance with its generic constraints. The work of scientists is based on mutual trust, it is part of the institution of science. But, the increased focus on (hyper) competition is inducing rivalry among scientists, eroding necessary trust and introducing distrust. Contemporary science has become part of a context where the focus lies on "growth" in an environment where resources are scarce. The strong competition between academics and researchers is a threat to the collective character of scientific knowledge production.

When reflecting upon the above mentioned characteristics, deviance in science can be associated with occupational deviance. Misconducting scientists, in their academic roles, do not act conform the norms of the institutionalized profession. These norms can be either written down formally in regulations by employers, or can be unwritten and 'commonly accepted' between employees and employers (Friedrichs, 2002). Considering institutionalized scientific practices (in universities for example), both set of norms (formal and informal) are not necessarily universal, nor complementary, nor even clear and consistent, since they can differ according to the university or discipline the researcher is connected to.

4. Doing science in a rapidly changing environment

The increased scientific focus on nonconformity in science indicates the fact that it is considered a social problem, something that can be observed through emerging initiatives and developments in the social reaction towards misconduct in science. When it comes to the available literature, most explanations of deviance in science tend toward individualistic (etiological) interpretations

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[7] the frequency of fraud or misconduct in science has been a subject of controversy in the "bad apple" versus "iceberg" debate. The prevalence of FFP and/or irresponsible behavior in science increasingly became a subject of interest (see for example Fanelli, 2009; John, Loewenstein, & Prelec, 2012; Martinson, Anderson, & De Vries, 2005). We can however argue that we are dealing with a serious dark number when it comes to the prevalence of misconduct in science (issues in registration, no clear-cut definitions, fuzzy boundary between tolerated and non-tolerated behaviors in science, the desire to keep cases of misconduct low profile, …).

[8] "A relatively small number of scientists and bureaucrats make the crucial decisions about research [...] they have the dominant influence on priorities in science" (Martin, 1992, p. 89)

or models. However, as we have already pointed out, even within a mere individualistic approach, it is necessary to understand how the context or situational conditions shape daily scientific practices. On a micro level, this entails that personal incentives and the character of the misbehaving scientist are taken into account. Van Onna (2014) argues that criminogenic factors alone do not form viable explanations in the criminal development of frauds, since not all peers commit fraud in similar contexts (Van Onna, J. H. R. 2014). Taking into consideration the personal developments someone goes through in the course of his or her career in interaction with external social influences, could offer interesting knowledge on the development and evolution of fraudulent behavior in science (cf. Piquero & Benson, 2004).

Criticisms to individualistic approaches arose with the general acceptance of the notion that the threat does not lie with the few bad apples, but rather within the barrel, the research system as such. As indicated by Ben-Yehuda: "The pressures to deviate have very little to do with the scientist's personality, but more with the structure of science, its goals, the way it functions and the specific positions scientists hold" (Ben-Yehuda, 1986, p. 15). A "bad person" approach, being the least threatening to the status quo, has successfully diverted attention away from possible deficiencies in the scientific practices and their organization today. Therefore greater merit lies in looking at scientific misconduct in the shadow of the subjugation of science to a system characterized by competitiveness and mass production of knowledge (fast science) on a large scale in an environment that shows a lot of similarities with a corporate context. Within this perspective one can explore today's scientific practice as an economy of knowledge, and examine the elements that create a fertile environment for misconduct combined with a study on the interaction of deviant behavior in science and the social reaction to it. Scientists are individuals acting within their organizational roles in an academic institution. Not only academic culture, but also organizational dynamics of institutions of higher education deserve attention from a criminological point of view.

The presumed rise or increase[9] in scientific misconduct over the years has often been attributed to the pressures of the present transitionary period, and the *publish or perish* idiom is mentioned in almost every scientific contribution or opinion paper on scientific integrity and misconduct. Yet, not all scientists are convinced that the urges and coercions to publish can be used as explanations for deviance in science since empirical research on the causality between the urge to publish and fraud in science is lacking (Schuyt, 2014a, 2014b). Nevertheless we are convinced that exploring the ecology of today's science as a knowledge economy can offer interesting insights about the context in which deviant behavior takes place. In accordance with this idea we want to shed light on the expanding ethical discourse in science as a response to the designation of a wide range of behaviors as incorrect (as will be discussed under point 6), resulting in intervention mechanisms in science (e.g. policy and prevention, ethical committees, commissions of integrity).

As noted in much of the available literature, the commodification of science evolved rapidly, bringing along a considerable set of output measures. The fittest are able to survive because they are granted with financial awards, more projects and funding. Prominent scientists manage to acquire extended credit due to the many top publications in highly ranked journals, almost exclusively in English irrespectively of the specific context in which the article was produced. These publications bring along more prestige, resulting in more and bigger projects. Renowned scientists are able to accumulate symbolic and material rewards, a process referred to as the

[9] Although it is often claimed to be the case in the literature, we can ask ourselves the question if we can at all justifiably speak of a rise in cases of QRP and misconduct in science? The increased attention towards scientific integrity and misconduct in science has opened up the discussion on these matters and has raised awareness, resulting in an increased focus and consequently an intensified discovery and registration of conjectures of misconduct. Further, as it is discussed in this text, there is a growing body of behaviors considered to be improper, largely transcending the narrow focus on falsification, fabrication and plagiarism, resulting in a 'perceived' rise in scientific misconduct.

Matthew effect in science (R.K. Merton, 1973; R.K. Merton, 1988). The unknown scientists suffer from "scientometric" evaluation systems and languish whenever they have not met the right output requirements, published fast enough or with enough impact. Not the quality of content as such is the primary concern, but rather the volume of publications a year, causing the perverse effect that papers are published even before reaching the completion they deserve, resulting in an enormous amount of scientific publications a year (some of these remain as good as unread). The quality assessment of these publications is done by competent peers, other scientists, who are ought to be skeptical and critical about the work of their colleagues. Collective dynamics as such have been eroded due to the expanding knowledge economy. Scientists are suffering from the enormous workload and do, justly, not feel like spending too much time nitpicking on textual, compositional or bibliographical qualities of the paper laying in front of them (Ben-Yehuda, 1986; Broad & Wade, 1982; Gutwirth & Christiaens, 2014). The quantity is more important than the actual quality of the scientific paper. The significant amount of paper retractions from high impact journals due to errors or misconduct, is effecting cracks in the self-correcting ability of science (Fang et al., 2012; Stroebe, 2012). Just as in other professions career advancement has become an important academic target that can be obtained mainly through publications. The institutionalization of doing science has brought on the scientific or academic career (Bauer, 2013), which is as such not problematic. However, strain occurs when the emphasis on profit (for science in the form of grants, acknowledgment, funds) overshadows the goal of knowledge production. Therefore, one needs to take into account the organizational dynamics that can drive academics into a behavior that is considered deviant or problematic.

In order to cope with the pressures coming from a peculiar publication system, surviving scientists strategically cut off the edges on a regular basis. To a larger extent, scientists are playing the game by segregating their data and results into different publications, submitted to numerous journals in different languages. Others strategically cite their colleagues, or have their name automatically listed as co-author, without having contributed in a significant way. The permanent and fierce competition between colleagues, research groups and institutions is further instigated by a spectacular growth in the number of researchers and students, not equally accompanied with a similar expansion in funding and financial opportunities (Halffman & Radder, 2013). Yet scientists, convulsively and perhaps nostalgically, need and try to hold on to fundamental principles or generic *constraints* that guarantee the production of robust and verified knowledge, because this is at the core of being recognized as a scientist. However, these traditional features and constraints such as the pursuit of certified knowledge, disinterestedness and organized skepticism as characteristics of the enlightenment period, became overshadowed by powerful external vested interests, political and commercial profits, and the urge to feed the media and the public with *fast* scientific results in order to advance as quickly as possible in one's personal career. The modern researcher needs to have managerial skills in order to survive the competitive game called science, produced in a big enterprise: the university (Bauer, 2013; Lorenz, 2006). The question is then *how* this has an impact on scientific practices and its mechanisms of social control., which brings us to our next point.

5. Social control and deviance in science

Misconduct in academia had not been part of (criminological) studies for years, potentially because "deviance in one's own family is the last to be recognized or acknowledged" (Bechtel & Pearson, 1990, p. 667). It should be said, however, that a growing body of studies are currently investigating deviant research behavior in a more exhaustive and critical way. Interest in the topic grew, mainly after some heavily publicized cases (Cf. Baltimore case, see for example Freeland Judson, 2004), and a considerable amount of publications about scientific misconduct started to show up in the academic literature. Prior works on deviant demeanors in science are arbitrarily distributed across diverse scientific disciplines and addressing an array of actions considered to be problematic. Situational and individual stimulus have been explored in a substantial amount of literature, reporting individual cases of scientific fraud. Next to the extent, causes and consequences of misconduct in science, the social reaction to it increasingly became a subject of interest to academics. Furthermore, there is a significant amount of research directed at codes, procedures, guidelines and institutional policies developed to deal with allegations of scientific misconduct. Further, to a growing extent, research attention is directed at methods meant to prevent undesirable behaviors in science and to promote integrity. Several scientific disciplines, ranging from economics to the medical sciences, have claimed their expertise on the causes of scientific misconduct, on what regulations, policies and prevention strategies are the most effective to counter this behavior. Criminological literature on this subject, however, is scarce. This is astonishing, because it is precisely the criminological sciences that have over 200 years of expertise when it comes to the complex object of deviant behavior considered as a social problem.

Going beyond a limited focus on the perpetrators, the causes and consequences of deviance itself, criminology takes into consideration social processes as well, as these processes construct deviance and shape the way in which society deals with them. Within a social reaction perspective, "crime (or deviance) appears as a reality constructed by how we define some situations" (Pires, 1993, p. 131). There cannot be a sole focus on deviant behavior as a single entity, since the social reaction and criminalization processes are inevitably part of a social construction that needs to be studied in a totality of social relations (Pires, 1993, pp. 129-131). The next section aims to discover some possibilities from theoretical criminology that can serve in understanding deviance in science.

5.1 Social control

Over thirty years ago, the first impulse towards a criminology of scientific misconduct was initially made by Zuckerman (1977), followed by Ben-Yehuda (1986). The latter indicated that "by exposing a limited number of deviant cases, the scientific community indicates to the public, and to its own members, that it has adequate controls to detect and punish deviants, and that it is a cohesive community knowing its moral-ethical limits and obligations" (Ben-Yehuda, 1986, p. 19). Ben-Yehuda was also one of the first authors to propose the possibility to apply sociological and criminological theories of deviance as conceptual tools to understand deviance in science, such as for example the concept social control. Social control is an ambiguous concept, which has been integrated in several theoretical disguises, such as for example labelling theory, control theory, critical criminology, feminism, etc. A standard definition written down in the sage dictionary of criminology describes social control as: 'all the means and processes through which social conformity is achieved, ranging from primary socialization, through informal mechanisms (such as peer group pressure) to formal methods associated with the police and the legal system' (McLaughlin & Muncie, 2006).

When reflecting upon social control in the institution of science, Ben-Yehuda found the best support in Matza's control theory. Matza explains the process of becoming deviant in terms of a drift situation (Matza, 1964). Although this theory was mainly formulated to address traditional 'universally agreed upon' forms of deviance, it could be applied on deviance in science as well. A scientist can enter a drift situation when he or she becomes disconnected from the morals of science. This does not mean the scientist in question will choose a deviant pathway over a right one. Ben-Yehuda believes cynicism forms an important step towards deviance in science: "a lonely scholar, hungry for publications and recognition, in a stiff competition and in a desperate subjective need for tenure, may easily become cynical of science" – a process that might enhance a drift situation (Ben-Yehuda, 1986, p. 20). In line with this philosophy, Ben Yehuda states that one might be deterred from committing deviant acts in an environment where social control is strong and where there is a high probability of being caught (Ben-Yehuda, 1986).

We can expect that through years of education and intensive training a scientist has internalized the constraints of science that characterize scientific practices. When reflecting upon Merton's norms, often considered to be central to the pursuit of scientific knowledge, one could argue that science is also characterized by strong external forms of control. Not only do the fundamentals of good and responsible science lie in organized skepticism, a principle which entails that a scientist's' work is open to inspection and that peers are suspicious and willing to actively criticize one another, organized skepticism also forms an important external control mechanism (R.K. Merton, 1973). This communality is crucial for the development of reliable knowledge. Both the internalization of constraints through years of education, and the externalized mechanisms of control such as the peer review process specific to science, suggest that social control in science is high. However, rather on the contrary, social control mechanisms in contemporary science seem to be not as strong as generally accepted (Ben-Yehuda, 1986; Broad & Wade, 1982).

The self-policing nature of science is often used as an argument to underline the ability of science to detect deviance and to deal with it adequately, as was pointed out by a director of the National Institutes of Health in 1981; "we deliberately have a very small police force because we know that poor currency will automatically be discovered and cast out"[10] (D. Fredrickson quoted in Broad & Wade, 1982, p. 79). The peer review system, the referee system and replication are the principles of scientific success or felicity. Yet, our contemporary knowledge economy has eroded the principles of organized skepticism and communality. After all, the credit that can be obtained through the replication of research, is small to nonexistent. Replication is useful in investigating existing accusations of misconduct, but is not an instrument for discovering it. The same can be said about peer review. Hence, it can be stated that dispersed forms of organizational control, such as replication and the peer review process, result in a low probability of getting caught (Ben-Yehuda, 1986). The latter has become a powerful argument in the discussion on the necessity of external forms of social control and consequently the need of external intervention possibilities. It becomes clear with this argument that it is precisely the pressures on the constraints of *doing science* that weaken and corrode generic internal mechanisms of social control (cf. replication and peer review).

5.2 Anomie and strain

Whereas Ben-Yehuda introduces control theory as a model of looking at scientific deviance, Bechtel and Pearson (1990) reflect upon Merton's anomie theory as an obvious choice. Anomie theory explains how deviant behaviors occur when the cultural goals cannot be achieved due to limitations in legitimate means for reaching these goals. As in Merton's words*:* "nonconforming behavior is a symptom of dissociation between culturally prescribed aspirations (goals) and socially constructed avenues for realizing these aspirations (means)" (R.K. Merton, 1968, p. 188). The ambitions of scientists are not financial by itself, although career advancement, promotion, prestige and success are targeted achievements. When dealing with these ambitious goals, scientists experience hefty pressures when the institutional resources are scarce, producing strain to anomie (R.K. Merton, 1968). "By placing the primary motivation toward deviance on the frustrations encountered by those who are expected to achieve, even told to achieve, but lack the legitimate resources to be successful, anomie may prove a useful concept in situations like modern science" (Bechtel & Pearson, 1990, pp. 677-678).

5.3 Other theories of crime

We would like to indicate that besides perspectives from social control and anomie, rational choice models can be useful in understanding how individuals operate within a social context. From the readings of Vaughan (Vaughan, 1999a, 1999b), we know that it is in the context of an organization that socially patterned variations of a rational choice model should be taken into account to explain how things go wrong in a socially organized setting. Within an academic/scientific context, this means that researchers must compete with their peers over scarce resources to be able to advance in their careers. Institutions must compete with other institutions in order to be successful. These structural pressures alone, however, cannot serve as the only explanation for deviance in science. There must also be *opportunity* to use illegitimate

[10] Declared by Donald Fredrickson, director of National Institutes of Health (March 1981).

means in order to adhere to the goals of the organization. Within this context a rational choice perspective can offer interesting comprehensions to deviance in science. "When an organization experiences structural strain to achieve its goals, individuals acting in their organization roles weigh the costs and the benefits of their actions, choosing to violate laws and rules to attain organizational goals" (Vaughan, 1998).

What traditional theories of deviance have in common is that they build upon the assumption of universal agreement upon values in society, something that is rejected by modern criminological perspectives such as for example in labelling (symbolic interactionism) or conflict theory. It can thus be argued that traditional theories of crime are well suited to explain falsification, fabrication and plagiarism in an academic context, given the overall acceptance of the fact that these behaviors transgress the generic constraints of science. We should however, remain aware of the fact that there is no consensus in academia about the wide set of behaviors beyond FFP, but rather a plurality across place, space, time and discipline. Therefore, it is interesting to look at some additional theories of crime that can provide some anchor points in the explanation of deviance in science, departing from a non-consensual collectivity.

Conflict theory assumes that there is no value consensus in the group concerned, and no common acceptance of the designation of certain behaviors as right or wrong. It is therefore conflict, rather than stability that forms the basis of the collective order (McLaughlin & Muncie, 2006). "The social reality of crime is constructed by the formulation of and application of criminal definitions by certain social segments, their diffusion within the rest of society, the development of behaviour patterns related to criminal definitions and the construction of criminal conceptions" (McLaughlin & Muncie, 2006, p. 64). Conflict theory addresses the power to criminalize rather than focusing on the deviant behavior itself: it has opened up possibilities in exploring for example white collar crime and state criminality (Carrier, 2006). When it comes to deviance in science, Bechtel and Pearson suggest that it is the study of the structure of science in a capitalist society that can reveal power and class relations within the institutionalized scientific practices (Bechtel & Pearson, 1990, pp. 678-679; see also Martin, 1992).

6. An ethical turn in science

As mentioned in the introduction, when trying to deconstruct and understand the phenomenon of misdemeanor in science, we need to take into account the processes of social reaction that define deviance in science, by setting norms that when transgressed will be considered as an "offense" or as deviant behavior. This approach challenges mainstream approaches of deviant behavior, because it supposes that there is no consensus or universal agreement upon what should or not be labelled as deviance. It would also draw attention on the constitutive role of the social reaction in the setting or determination of the misconduct. It is interesting to note that the tolerance or intolerance to certain behaviors in science has changed over time and space, and from one discipline to another just as happened in the law at large (e.g. decriminalization of abortion and consumption of cannabis, criminalization of racism and discrimination). From a criminological point of view it is this evolution, comparable to (de)criminalization processes in society that is worth a thorough exploration. With Kees Schuyt, we observe that "almost all codes of conduct in science are formulated immediately after a number of serious cases of plagiarism, fraud or fabrication occurred" (Schuyt 2014, p. 170, original italics, our translation). Indeed, the objective of preventing and fighting problematic scientific behavior provides a strong anchor point for the deployment of a regulatory offensive in the field. An understanding of the expanding ethical mindset in science policy as a response to the problematization of incorrect behaviors accompanied by a whole range of institutional measures, can offer interesting insights. In this regard parallels can be drawn with the rise of the integrity concept in other professional contexts such as for example police departments and the private security sector, resulting in a wide range of institutional measures, codes of conduct and prevention strategies (See e.g. Hughes, 2007). It closely fits in what Zedner describes as the pre-crime society (Zedner, 2007).

This evolution cannot simply be attributed to a sudden or clearly established increase in misconduct in science, because there is, until now, no evidence to support this statement. The increased attention for misconduct rather points at the complex relation and cohesion between a phenomenon and the reaction to it. It is after all "the level of social concern about any form of deviance that reveals much about the society that disapproves of the behavior" (Hackett, 1994, p. 251). It shows us the necessity to understand the *ethical turn*[11] in science as part of the rise and expansion of external control and regulation of scientific practices today.

Where science policy in the past dealt with FFP in a rather informal and decentralized way, it has now become a formal practice, exercised outside the actual scientific practices (Hackett, 1994). Hence, it is important to draw attention to the role of social reaction when it comes to behaviors considered as deviant. As Hackett already formulated it in 1994: "Why has scientific misconduct recently received so much public attention and opprobrium? Why have there been calls for increased oversight and regulation? To raise such questions does not deny, excuse, or justify scientific misconduct, but calls into question the changing exercise of social control rather than accepting it as arising naturally from a sea of increasing misconduct in science" (Hackett, 1994, p. 251). The regulatory perspective, dominating today's science policy, reflects managerial thinking presuming consensus and even universal agreement on the norms of science and on the notion of scientific integrity, but does in no way take into consideration the situational characteristics that scientists must comply to (Gutwirth & Christiaens, 2015).

Attention towards cases of fraud in science has increased, as did the objections towards a growing body (grey zone) of violations of scientific integrity, going far beyond the generally accepted FFP. This can be illustrated as a small pot on a stove with fraudulent behaviors that have received an increasing amount of attention, causing the pot to boil over. What used to be limited to the pot itself, has now been spread out across the stove, over the countertop and down to the floor. This *spill-over* towards a broader understanding of misconduct in science has resulted in the creation of an umbrella concept, opening up far reaching possibilities for intervention. A variety of external control mechanisms (either preventive or repressive, such as ethical committees and commissions for scientific integrity), are being imposed on the everyday scientific practice, possibly causing a net widening effect. This means that those processes to prevent crime are expanded, past the initial subjects. Social control mechanisms are being dispersed and penetrate deeper into the social fabric, what was described by Stanley Cohen as 'thinning the mesh' and 'widening the net' (Cohen, 1979; McLaughlin & Muncie, 2006). The fact that we have moved on from dealing with misbehaviors 'after the fact' to trying to prevent deviance in science, points out that the boundaries between acceptable and unacceptable behavior are blurred. New movements in the regulation of scientists' behaviors "supplement the existing system or else expand it by attracting new populations" (Cohen, 1979, p. 347*).* These developments result in a sort of creeping criminalization of scientific behaviors that go far beyond the classical FFP.

The knowledge economy has imposed far-reaching pressures on institutionalized scientific practices, with devastating consequences for the collective dynamics of science. Scientists today are cornered between trying to hold on to the generic constraints of doing science and the science policy that is being imposed (managerialism, competitive spirit, output pressure). With the growing awareness of the limits of self-regulating mechanisms in controlling the contemporary scientific practice, it has been acknowledged that science regulating itself through informal processes might not be adequate enough.

Therefore more focus is directed at proactive policy measures, coming from outside the actual scientific practice. The emergence of a wide range of codes and regulations to stimulate scientific integrity and prevent misconduct can be seen as a reaction to these developments. The goal is to create a culture of integrity through the explicitation of what is taken to be the norms of science.

[11] In analogy with what is described as a "*preventative turn*" in criminology, see e.g. Hughes, 2007, p.p. 26-53.

In other words: external control mechanisms were set up in order to intervene even before scientists behave badly, a frame of mind that indicates highly preventive thinking., hereby creating an environment where the social control complex is ever expanding through the policing of early signs, risks or possible crimes, thus intervening when no offence has yet been committed. Zedner (2007) identified this in criminal justice as "a shift from a post- to a pre-crime society, a society in which the possibility of forestalling risks competes with and even takes precedence over responding to wrongs done" (Zedner, 2007, p. 262). From such a perspective, the *ethical turn* can be understood as a new leading discourse in science policy, that is merely concerned with enhancing scientific quality, by promoting good practice on an ethical basis (no longer on the basis of the constraints that underpin science).

An interesting example of this shift from reactive to proactive policies in science is the success of ethical committees in science. Whereas ethical committees were initially directed at medical research (health care and clinical trials), they have been extended to other scientific disciplines. Ethical committees were originally installed to deal with complaints regarding unethical research during the course of the research practices. These committees, however, have evolved into a preventive 'control' measure, assessing the admissibility of submitted research on an ethical ground. A researcher needs to specify the peculiarities of the research, methodology of his/her research and the possible harms that could occur during the research process to an ethical committee, accompanied by ways in which the researcher plans to anticipate those harms (Ross & Athanassoulis, 2014). If it satisfies the committee, the project will receive an ethical warrant, a sort of certificate of ethical conformity. Haggerty uses the concept of "ethics creep" in order to refer to such processes that affect an expansion and transformation of controlling systems (cf. net-widening processes). ""Ethics creep" involves a dual process whereby the regulatory structure of the ethics bureaucracy is expanding outward, colonizing new groups, practices and institutions, while at the same time intensifying the regulation of practices deemed to fall within its official ambit" (Haggerty, 2004, p. 394).

The evolution towards a more organized, institutional and formal regulation of scientific behavior (either through codes of conduct or *controlling* agencies) implicates that a discussion about everyday practices of science has become more important. It does however, contribute to a further bureaucratization and containment of scientific liberty, based on distrust and suspicion. Furthermore, it has caused the perverse effect that scientists alienate from the core elements in which scientific knowledge is produced. Normative frameworks direct attention to the responsibilities and integrity of individual scientists, but does not reflect the importance of science as a collective activity. This gives the impression that it is possible to create excellent science practices, by fostering individual responsibility only.

7. Concluding remarks and recommendations

In this deliverable our aim was to shed a criminological light on deviance in science, hereby deconstructing scientific deviance in its complexity and studying it as a paradoxical object that encompasses several dimensions. Therefore, not the ontological phenomenon in itself, but rather the social processes and constitutive practices of social control formed the object of this work.

Concerns were raised in consideration with regard to the consensus model in conceptualising integrity and fraud in science, since the literature has pointed out precisely a lack of agreement on what scientific integrity is, and more specifically on what should be considered scientific misconduct. The increased attention for problematic scientific practices has produced the use of the much broader concept of scientific deviance, beyond FFP. Normative concerns regarding scientific misconduct arose, creating fertile ground for far reaching social reaction possibilities. This way an object of intervention emerged supported by a discourse on scientific practices distinguishing *bad* from *good*, *right* from *wrong* and *excellent* from *questionable*. Moreover, with the increased attention towards misconduct in science, we observe an overabundance of behaviors labelled as scientific misconduct, going far beyond fabrication, falsification and plagiarism, causing scientific misconduct to operate as an umbrella concept. This evolution has blurred the boundaries between tolerable and intolerable behavior, causing confusion and animosity within the scientific community and within the institution in which scientific practices take place. Therefore, a clear distinction is needed between the generically agreed upon notion of misconduct (FFP) and the large amount of questionable behaviors situated somewhere under the umbrella concept of 'scientific misconduct'. The current disagreement on this fuzzy level of 'possibly deviating' behaviors in science, causes obscurity and uncertainty amongst academics.

Therefore, we have expressed the necessity of understanding the rise of external control and regulation in science as an ethical turn. This ethical turn has to be studied in the background of a knowledge economy, an evolution that has inherently created relentless pressures on the scientific practices and their constraints (communality and objectity). These pressures have eroded or weakened internal (generic) social control mechanisms, since these do not allow to keep up with the model of doing fast science and knowledge production. Given the inadequacy to combine the production the most output, the most influential results at the fastest tempo with being exemplary and excellent, the internal control mechanisms (peer review, replication, collectivity, controversy, …) can, in no way, keep the house of science *clean*. The critique towards these internal mechanisms of social control have created a discourse about the necessity of an external social reaction, thereby evolving in terms of institutionalized norms and regulations. The social control mechanisms were installed to deal with misbehaviors, but imposed far reaching interventions that have supplemented the existing system, hereby widening the net of intervention grounds and consequently endangering the procedural safeguards.

The now dominating regulatory framework reflects emergent managerial thinking again assuming consensus. It focusses almost entirely on the individual responsibility of academics, leaving untouched the context in which scientists are trying to tie the knots between the pressures imposed on them and the constraints that form the basic elements and foundations of "*doing science*". Focusing on the bad apple as a way of scapegoating -by highlighting individual characteristics of the deceiver in question, thereby explaining actions as caused by stress or moral corruption- isolates the case from structural flaws in "the institutionalized scientific practices". The external control and prevention measures (that have become part of the so called integrity debate) disregard the collective context of knowledge production, thereby neglecting the wider structure in which science is embedded (that is the knowledge economy and the collective production of knowledge).

Recommendations

- The boundaries between misconduct (infractions on the generic constraints of science), and questionable practices (the outcome of a more political debate about science), have been blurred. Therefore, we express the need to open up the discussion about a clear codification of a whole range of behaviors that are now mentioned under a container concept of scientific misconduct, but that do not form part of the generically agreed upon constraints of science (referring to FFP).
- The diversity and plurality of scientific practices has a remarkable impact on the concept of deviance in science, a notion that has been important in exploring the multidisciplinary reconnaissance in WPII, but that needs to be taken into consideration for further stages of the Printeger project as well.
- Interventions and integrity policies need to acknowledge the basic constraints of science (objectity and collectivity) and go beyond a sole focus on individual responsibility.
  Preventive practices such as for example educational tools need to assure a collective dimension with regard to knowledge production, otherwise they risk a labelling effect to occur, possibly enhancing distrust in science (a distrust that can, consequently, erode *the collective* of science).

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