

# STAGNANT LAKATOSIAN RESEARCH PROGRAMMES

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ABSTRACT. We propose an extension of the classical dichotomous categorization of research programmes into progress and degeneration according to Lakatos in the form of a neutral third category: the stagnant research programme. First, a critical examination of the primary literature with its often criticized definitional gaps justifies such a category. Through a generic derivation of criteria for stagnant programmes, a clear demarcation from progressive and degenerative ones is achieved. An empirical cross-check is subsequently employed for support: Both a series of examples from fundamental physics and a general analysis of today's research landscape also suggest on an empirical level the need to go beyond the traditional Lakatosian conception. Attributing stagnation is entirely in line with Lakatos' original intentions, which aimed not to hastily discard promising research but to exercise patience until the lifting of certain external constraints potentially enables progress once again.

## 1. INTRODUCTION

In the traditional dichotomy between progressive and degenerative research programmes, as proposed by Imre Lakatos, two shortcomings occur. These binary categories appear to be neither mutually exclusive, nor collectively exhaustive. The first shortcoming became an early target of criticism given the concept's lack of definitional precision. According to Feyerabend, the definition of progressive programmes via "intermittently" empirical problem shifts represents an empty criterion allowing one to wait indefinitely: "why not wait a bit longer?" (1975, p. 77). During long periods of absent experimental success, the boundaries between progress and degeneration seem to blur. This fact is closely intertwined with the second shortcoming, which has received little attention in the existing secondary literature on Lakatos and will be discussed in this article.

Facing the challenge of fully grasping the complexity and dynamics of today's research landscape through the categorization of research programmes, one quickly notices that the classification of programs into black and white - into progressive and degenerative - reaches its limits in various cases, and the space of all research programs is not adequately and fully described by this binary division. We propose to expand the Lakatosian vocabulary by a third, neutral option - which we will call the stagnant programme - and to critically reflect on the structures of contemporary scientific practice. The question of a third category of stagnation encourages taking a nuanced perspective on the state and development of research programmes to precisely capture the space between progress and degeneration: The resulting extension does not treat temporary stagnation as a deficiency but as a natural and non-condemnable phase in the

research process of certain disciplines. Four general criteria need to be fulfilled to demarcate stagnant programmes from all the others. As in the spirit of Lakatos, the diagnosis of stagnation can prevent a premature discard of promising research programmes and enables to bring the threefold category scheme more strongly into the present, rather than solely considering it as a historiographical tool.

We exemplify the category of stagnant programmes by concrete challenges of fundamental physics of this century, where the diagnosis of degeneration through missing empirical progress may be inappropriate and misjudges the state of several research programmes: In principle, research could be conducted according to the best methodology, but progress is hindered not by internal misdemeanors, but by external, uncontrollable circumstances. In the end, the introduction of a neutral stagnation turns the previous categorization into both a mutually exclusive and collectively exhaustive framework for today's research, resolving the first shortcoming as a side effect, too.

After critically examining the primary literature (Chapter 2) and analytically deriving four general criteria of stagnation in an analytical top-down-approach, where the ascription of degeneration is unwarranted (Chapter 3), support is drawn from a bottom-up empirical examination of today's research landscape. Particularly, current fundamental physics exhibits a high degree of alignment with the analytically derived criteria (Chapter 4.1). However, a bird's-eye view of the state of today's research in general reveals gaps in the classical Lakatosian dichotomy, too (Chapter 4.2).

## 2. THE DICHOTOMY OF PROGRESS AND DEGENERATION

Let us recapitulate the Lakatosian idea of progressive and degenerative research programmes in a nutshell. Central concepts that will be explained below are the programme's hard core, the protective belt (or ring) of the hard core, as well as the positive and negative heuristics, each of which influences the programme, and its progressive or degenerative state.

The hard core represents the non-negotiable set of basic theses and existing statements on which future research should be based. This foundation is not easily abandoned; doing so would be equivalent to abandoning the research programme itself. However, ongoing investigations typically bring data and phenomena that initially cannot be reconciled with the basic assumptions of the hard core. The inviolability of the hard core thus requires a flexible protective belt that enriches the theoretical system with supporting hypotheses intended to reconcile emerging anomalies with the hard core. While the hard core enjoys immunity, the shape of the protective ring continually adapts to current threats to the hard core - modifications of the belt constitute the actual research process. The *modus tollens* of the research programme does not affect the hard core but is redirected to the protective belt. Together, the core and ring represent the evolving set of theories and explanations of the research programme over time. Models and theories thus exist not in isolation but are always interwoven into the structure of a research programme.

It is the task of positive heuristics to provide general guidelines on how a complement or extension of the hard core should be carried out to explain the phenomena that have emerged over time coherently. Similarly, a heuristic in the negative sense can be formulated. This directive does not aim at gaining knowledge but rather excludes paths that would jeopardize the hard core. In short, these forms of heuristics are guided by the question how one should (not) proceed.

A. Chalmers (1994, p.84) succinctly states: "Research programmes are either progressive or degenerative, depending on whether they successfully lead to the discovery of novel phenomena or whether they repeatedly fail in doing so." A distinction is made between theoretical and empirical progress: Over the course of its development, the research programme forms a series of theories that, through constant adaptation in line with positive heuristics, successively surpass their predecessors in explanatory power and provide novel empirical findings (theoretically progressive problem shifts). If these predictions are also at least partially confirmed, there is an empirically progressive problem shift. The absence of such shifts indicates the degeneration of the programme.

The primary literature initially clarifies that a definitive attribution of degeneration is not readily possible because there can be no "instantaneous rationality" (Lakatos 1982, p.160), and an actual knowledge about the programme's state should exist only in hindsight. The high "methodological tolerance" (Lakatos 1970, p. 71) exhibited by Lakatos' approach, which we will describe below, seemingly leaves the philosopher of science agnostic regarding the state of degeneration. The generality of the approach implies that no time scales for progress can be specified for the "intermittently progressive empirical shift" (Lakatos 1970, p. 49) that characterizes true progress. Lakatos initially only conveys the central message that in every step of the research programme, there must be a theoretically progressive shift in problems when attempting to eliminate anomalies ("increase in content", *ibid.*). This not only serves the general applicability of the methodology of research programmes, but also reflects Lakatos' deep conviction, agreeing with Kuhn, that theories are born falsified (in his words being in an "ocean of anomalies," Lakatos 1970, p. 48). However, while Kuhn describes the absence of empirical progress by an irrational adherence to an outdated paradigm, Lakatos replaces it with a rational, momentarily justified adherence. The latter wants to prevent the demand for constant empirical progress from overly restricting the space for this adherence, even though the potential of a research programme may not have been fully exploited (Lakatos 1970, p. 49). Despite the temporary lack of empirical success, a successful return of a research programme is conceivable, which ultimately outperforms other programmes in a sort of evolutionary competition. Conversely, this also means that not only a final degeneration but also no final victory of a programme over others can be attested without a considerable time gap in this competition. This relaxation of criteria is intended to ensure theoretical multipolarity and a fruitful competition for the best programme. This may even require the special protection of emerging research programmes (cf. Lakatos 1970, p. 71 f.). However, lenient judgments must not lead to agnosticism. Lakatos opposes the claim that his approach represents "radical skepticism" (*ibid.*).

For even if real clarity about the state of research programmes predominates only ex-post, as outlined, Lakatos did not solely aim for a rational retracing of the history of science (as in Lakatos 1982, repeatedly claiming there was no "instant rationality"), but, of course, also had an intention to bring the concept of progressive and degenerative research programmes into the present discourse. In other words: to anticipate problematic tendencies in contemporary research programmes (cf. Lakatos 1976, p. 11). He aims to provide criteria for the elimination of entire research programmes, thus accepting an intervention in the course of the history of science through his considerations.

The call for action that can be derived from this text analysis can be specified as follows: The claim of concrete criteria for the elimination of a programme fails because, on the one hand, unclear timeframes do not allow for an assessment. On the other hand, in the case of too long periods (whatever this means concretely for Lakatos), a hypothetical benign stagnation was indistinguishable from degeneration. Benign stagnation, however, is neither equivalent to progress. Circumstances are conceivable under which both attributions are inappropriate. These circumstances will be highlighted in the next section.

Hence, we introduce the concept of a *stagnant research programme* as a third state of a research programme. Interestingly, to the best of our knowledge, Lakatos uses the term *stagnation* of the research programme only once, and without a deeper meaning (Lakatos 1976, p. 11): "(...) it is stagnating if its theoretical growth lags behind its empirical growth, that is, as long as it gives only post-hoc explanations either of chance discoveries or of facts anticipated by, and discovered in, a rival programme (degenerating problemshift)." In this sentence, stagnation has got a pejorative character, is intended to grasp the lack of theoretical, not empirical progress, and is used synonymously with degeneration. However, the new category between progress and degeneration called stagnation should be understood quite differently: It expresses a neutral evaluation of the current state of a research programme. Equating standstill with degeneration, borrowed from economic doctrines, shall be explicitly rejected - stagnation is not a reprehensible condition but precisely attests to the *absence of degenerative elements*. Furthermore, stagnation finally fills the white spots in the map of the research landscape, creating a mutually exclusive and collectively exhaustive threefold categorization scheme.

### 3. ANALYTICAL TOP-DOWN DEDUCTION OF CRITERIA FOR STAGNATION

Let us first liberate ourselves from the bias of specific programmes whose condition prompted this critical review of the Lakatosian dichotomy. Instead, this section includes general considerations based on the previous discussion of the primary literature of Lakatos, formulating criteria that a stagnant research programme should meet. For this purpose, several demarcations are formulated, filtering the programme in a top-down approach such that the subset of stagnant research programmes remains in the desired sense of the word presented in this article. First, the development of a third option requires the following separation:

**1) Demarcation from Progress (Criterion of Intergenerational Stagnation).** In this demarcation, the definitional problem of intermittently empirical problem shifts should be avoided. The arbitrary extension of the intermittency appears as an artificial adherence to a dualistic principle of rise and fall, the limits of which may not have been foreseeable at the time of its formulation. For the attribution of *progress*, the permissible time until the empirical confirmation of the theory must be limited. Conservatively estimated, this can be set at the remaining lifetime of those who formulated the theory. What should a theorist feel other than stagnation if, at the end of their life, their theory remains unconfirmed, as correct as it may be? However, a more sharpened diagnosis is an *intergenerational stagnation* of empirical progress. In this period of lacking experimental success, there would be no replacement of a previous generation of researchers by younger ones equipped with new (possibly disruptive) ideas, addressing also the Kuhnian imagination of scientific progress. The new generation would face the same problems of empirical proof as the previous one, without promising solutions. If this time frame of approximately 30 years is established - a degeneration would be present in the classical dichotomous state space<sup>1</sup>. To distinguish from stagnation, it must first be ruled out that, unlike the new generation of the same research programme, representatives of another programme make progress.

**2) Demarcation from Relative Degeneration (Criterion of Singularity).** If there were a competition between at least two programs, there would inevitably exist a relationship between them that transforms stagnation into degeneration. However, if the competition has dwindled, degeneration may not be present: In this case, there have been no promising competing research programmes that could replace the existing stagnant one. Hence, in the majority opinion within the discipline, the current research programme would seem largely irreplaceable and without alternatives in its heuristics and hard core<sup>2</sup>. This condition may indicate that researchers are on the right track and follow the best scientific practices, but face hurdles of a different kind. Nevertheless, this insulation in the landscape of research programmes could still represent degeneration if the programme is engaged in addressing ill-defined problems, where nothing needs to be explained. The programme would face an insurmountable hurdle, and the "problem" needs to be considered a degenerative element of the programme. These programmes must be sorted out in a third demarcation. Criterion 2 thus distinguishes between a dynamic (progressive or degenerative?) and a static (good or bad stagnation) research

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<sup>1</sup>Depending on the discipline and its specific temporal requirements for experimental progress (construction time of apparatus, measurement duration, etc.), deviations from the rough guideline of 30 years are naturally conceivable. The crucial factor in resolving the definitional vagueness of the dichotomous concept is only the existence of a period beyond which stagnation may occur. Definitional clarity does not necessarily require concrete numerical values.

<sup>2</sup>Of course, the competition between individual elements of the protective belt on a theoretical level would not be a reason to end the stagnation of the entire programme. There could be even a coexistence of several static programmes, but only one of them could be grounded on a sound hard core, whereas others would rely on misleading assumptions. Hence, the term of a singularity criterion is still valid.

landscape, each with their own dichotomy.

**3) Demarcation from Malign Stagnation (Criterion of Benign Epistemic Limits).** Scenarios are conceivable in which different limits of an isolated programme are reached, but not all fall within the literal sense of stagnant research programmes:

- (a) The end of the research programme is (assumed to be) reached, so in the absence of problems, there is not even theoretical progress. The tree of knowledge is harvested, and there is nothing new to discover (like no new continents for contemporary sailors on Earth), rendering the programme unassignable to any state. While this is not always immediately apparent, stagnation in the intended sense implies that the programme can, at least in principle, contribute to an increase in knowledge. In 3a) we are not even dealing with a degenerative programme, the programme is simply non-existent anymore.
- (b) The problem from which the research programme (or a part thereof) originated is unsolvable because it is ill-defined and does not pose a problem, but a pseudo-problem. In the case of the absence of relative degeneration (Criterion 2), absolute degeneration may be present: the programme's isolation (missing alternatives) does not necessarily imply a sound hard core. Incorrect basic assumptions can thus generate pseudo-anomalies that cannot lead to benign stagnation. We call it malign stagnation, synonymously to degeneration that does not require any competition with other programmes.
- (c) Benign stagnation occurs only in the case of limited experimental or cognitive capacities of representatives of the research programme, affecting the solution to correctly formulated problems. While the former is often easily determinable (limits of technical feasibility, financial possibilities), an intellectual limit of humans to these problems is speculative. Nevertheless, in both cases, the approach of the research programme is correct and should not be assigned to degeneration, but to stagnation<sup>3</sup>

Criterion 3 thus distinguishes between internal failures (or final success in 3a) within a research programme, which can in principle be prevented or cured, and external, uncontrollable forces - the nature of the human being and the society she lives in. Stagnation can be permanent due to these inherent limits, but it can also be transformed into progress in the distant future since these may be limits of a scientific era and not fundamental epistemic limits. Thus, as Lakatos wishes for both the progressive and degenerative states, it generally remains a provisional attribution that may solidify only through the historical retracing of the discipline. After this filtering it is confirmed that in the stagnant research programme there is in principle still something to be discovered. However, there is still the option that acceptance of the limited experimental reach

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<sup>3</sup>A remark with respect to the demarcation problem: Pseudo-scientific hypotheses cannot be trapped in a stagnant research programme in the envisaged sense of the term, because the argument of benign epistemic limits can never be applied here. There will be always a malignant stagnation.

and intellectual capabilities of Homo sapiens leads to resignation or inactivity or that the scientific value is simply too small to keep the programme alive, necessitating a fourth filter:

**4) Demarcation from Insignificance (Criterion of Scientific Value).**

Lakatos writes about degeneration, among other aspects: "In the methodology of research programmes, the pragmatic sense of rejection (of the programme) becomes crystal clear: it is the decision not to work on it anymore" (1974, p. 152, Fn 245)<sup>4</sup>. A complete rejection of a programme as the ultimate consequence of stagnation presumably occurs rarely in its entirety in this specific subset of programmes filtered in all Criteria 1-3. Instead, a small grouping possibly remains, which, despite the majority's rejection of these activities, continues to dedicate itself to the programme. This form of stagnation still has degenerative characteristics; it resembles the gradual end of research programmes that lag behind in the competition for the greatest empirical adequacy, steadily losing followers, with the difference that in this case, even competitors are absent. However, in the case of the neutral attribution of stagnation, as envisaged in this paper, an opposite criterion is needed, according to which the research programme has already recovered from phases of doubt or resignation and is actively pursuing open questions intensively and with considerable effort. For the scientific discipline, solving the problems of the affected research programme is therefore too significant to bury it. In short, Criterion 4 asks: While there's still more to discover, is it meaningful, and do we want to invest significant effort into these potential findings that might be out of human reach forever?

We have seen that defining stagnant programmes first involves a relatively simple demarcation from progressive ones, requiring only the closure of the much-criticized definition gap of "intermittently" empirical problem shift. In contrast, the demarcation from degenerative programmes revealed the multitude of their variants - three criteria were needed to filter out those programmes whose characterization no longer carries pejorative connotations, and even their refinement will be needed when it comes to an empirical validation of Criteria 2-4. In other words, most progressive programmes are alike, each non-progressive programme is stationary in its own way.

Several other authors reflected about possible ways of stagnating research<sup>5</sup> for longer periods within the entire twentieth century, confirming the previously derived set of four criteria: In the 1990s, the idea that a culmination point of many sciences could soon be reached gained broader attention (Horgan 1996). Horgan's work on the end of science can be translated into the terminology outlined in this chapter that an increasing number of scientists from various disciplines will find themselves in stagnant research programmes in the coming decades. Particularly concerning fundamental physics, he observes a departure

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<sup>4</sup>This rejection is not caused because the state of Criterion 3a) is reached.

<sup>5</sup>The sources discussed in the following mostly do not explicitly mention the term of stagnant programmes, but elaborate on the same idea.

from empiricism, as contemporary theories increasingly elude experimental accessibility (similar to Criterion 3). For this, he defines "ironic science" as closer to philosophy, literary criticism and even literature itself offering points of view and opinions that do not converge to the truth.

However, the debate about such an end to scientific progress is considerably older. It particularly supports the Criteria 3 and 4 of benign epistemic limits and scientific value and will be briefly retraced here. Bury (1932, p.1) early on noted that stagnation may not necessarily result from a complete exploration of a field: "How can we be sure that some day progress may not come to a dead pause, not because knowledge is exhausted, but because our resources for investigation are exhausted - because, for instance, scientific instruments have reached the limit of perfection (...)" He also speculates about the scenario of intellectual limits of the human species (ibid.). Stent later adopted a similar stance, differentiating between various disciplines. He believed that biology might one day complete all empirical findings (supporting Criterion 3a), indicating no stagnation. In contrast, physics, with no inherent limits, could, in principle, explore things on ever higher energy and ever smaller length scales but faced principal limitations (supporting Criterion 3c) in empirical reach due to physical, cognitive, and economic constraints (Stent 1969, p. 74). According to Stent, one should thus look for a neutral standstill, according to the criteria he previously derived, primarily in physics. In a later work, he justifies the consideration of imminent stagnation through the paradox of progress (Stent 1978): It is contradictory to conclude from the golden age of progress that this successful course continues indefinitely. The very limitation of things that humans can know and demonstrate would imply that the success story of science in the 20th century could come to a sudden end in the near future.

In the same year, Rescher joined Stent's position, stating that the process of knowledge is at least infinite on an abstract level but societal and political acceptance limits progress due to increasing financial resources with decreasing epistemic returns. In light of the previously derived criteria, he bridges Criterion 3 to Criterion 4: Ultimately, economic constraints, alongside technical feasibility, are the essential reasons for a fundamental limit to the experimentally achievable. Even if a research programme is highly significant (Criterion 4, therefore, remains unaffected by economic constraints) - beyond certain costs, stagnation is inevitable, despite the best scientific methodology (c.f. Rescher 1978). Therefore, good scientific practice includes resource-efficient and economically efficient planning of experimental proof as a much more important operation mode.

Turning to contemporary debates, Dawid (2019, p.105) addresses the time period of absent empirical confirmation or disclosure as follows, thereby supporting our previously introduced Criterion 1 of intergenerational stagnation: "It may still make sense to ignore intermediate epistemic states between ignorance and conclusive knowledge in contexts where they last only for a brief period of time before the case is settled based on conclusive empirical evidence. In contemporary fundamental physics the typical time scale for that intermediate state has grown beyond the length of a scientific career."

Although Criterion 2 was introduced rather for technical reasons to separate relative degeneration in the competition of programmes (where a stagnant programme is outperformed by a progressive one, inevitably causing degeneration by their relation to each other) from an isolated stagnation, this requirement also finds support on a completely different frontier: Dawid's (2013) no-alternative argument, with a very similar statement, is used to justify the complete abandonment of empirically progressive problem shifts as a (non-negotiable) condition for progress in some future. With this non- or post-empirical approach to theory evaluation, as it was named soon after by Huggett (2014), Dawid addresses especially string theory and other areas of fundamental physics whose experimental requirements permanently lie beyond economic and technical reach. This extensive departure from established standards will be initially set aside in this chapter. However, in cases of hopeless stagnation, this approach could represent the only way out of the pessimistic perspectives of the aforementioned authors, even if it initially appears as capitulation and reminiscent of Horgan's (1996) ironic science.

It remains to be noted that the derived criteria can be legitimized by referring to various analytical contributions to debates of the past hundred years. On the one hand, these criteria must prove themselves in practical application; on the other hand, it is assumed that the empirical perspective on stagnant research programmes adds some further criteria. This step will be undertaken in the following section.

#### 4. EMPIRICAL CROSS-CHECK OF CRITERIA FOR STAGNATION

In this section, the logic of characterising stagnant research programmes is reversed: The following empirical test of the introduced distinction between progress, stagnation and degeneration will initially be conducted using some specific examples. Additional suitable criteria will be identified during this process. Subsequently, a shift will be made to a holistic perspective that aims at comprehensively examining the tendency towards stagnation in contemporary science and justify the introduction of the concept of stagnation.

**4.1. Examples of Stagnant Research Programmes.** As already hinted in the introduction, the debate around stagnant research programmes is mainly motivated by fundamental physics and its state in the twenty-first century. Going outside of the realm of fundamental physics with particular use cases is, however, beyond the scope of this article, since complicating and discipline-specific features may occur. First, we keep our eyes on programmes that might have overcome the phase of stagnation already.

##### **Historic Examples of Stagnation:**

Fusion power for widespread energy supply was lacking promising experimental results for a considerable amount of time that already fulfils the Criterion 1 of intergenerational stagnation<sup>6</sup>. While nuclear fission quickly transitioned to peaceful use shortly after its military application, achieving technical

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<sup>6</sup>One might argue that exploring nuclear fusion is rather a development programme than a research programme. However, the four criteria can be still perfectly applied.

realization within a few years, effective utilization of fusion energy has yet to be accomplished. The two fundamental reactor types were developed in the 1950s (Tokamak: 1950, Sacharov, Tamm, and Stellarator: 1951, Project Matterhorn - cf. Bromberg 1982) but remain a utopia in their specific application. Very recently, the commercialization of the topic by private companies, due to substantial monetary support and scientific achievements nearing the breakeven point, might have transformed the research programme into a progressive one<sup>7</sup>. Interestingly, there are no larger conceptual deviations from the originally proposed reactor types - stellerators and tokamaks are still the state-of-the-art (fulfilling Criterion 2 of singularity, too). The stellarator *Wendelstein-7-X* created its first plasma in 2015, based on a magnetic field with field lines running within nested torus surfaces for magnetic confinement of particles. Achieving these flux surfaces in a stellarator proved challenging: Only by the end of the 20th century, with the advent of powerful computers, could the necessary calculations be performed. Moreover, simulating the interior of stars in order to initiate the fusion process demands energy scales (in pressure or temperature) way beyond earthly phenomena, hindering empirical progress, too. Hence, one observes limits of technical feasibility for almost half a century (Criterion 3 of benign epistemic limits). The global importance (Criterion 4) of nuclear fission is unquestionable in times of climate change and an increasing worldwide hunger for electricity. The final assessment of the state of this research programme will be the task of future historians of science. It may serve, however, as a historic example for a stagnant research programme.

Another programme that recently overcame its stagnation is the one dedicated to the exploration of gravitational waves. Albert Einstein predicted them in his general theory of relativity in 1915, but he himself doubted that they could ever be detected. In the 1960s, Weber made the first attempt to detect gravitational waves using a specialized detector, although it was unsuccessful (Weber 1968). Subsequently, there was an immediate transition to interferometers as a more suitable apparatus: The LIGO detectors successfully detected gravitational waves for the first time in 2015. These waves originated from the merger of two black holes, releasing a tremendous amount of energy in the form of gravitational waves (LIGO 2016). The efforts for direct detection through interferometers since the 1970s satisfy Criteria 1 and 2 of intergenerational stagnation and singularity. There is no evidence of malign stagnation; rather, the principal obstacles are identified as the technical development level of the last century and construction times. The confirmation of a prediction from the most important physical theory of the last century, alongside quantum theory, and the fact that gravitational waves will play a dominant role in future astronomy, also confirm the fourth criterion for the existence of a stagnant research programme until 2015. Today, it can undoubtedly be described as progressive.

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<sup>7</sup>Hence, despite extensive government funding, the stagnation of the programme could not be averted initially, highlighting once again the rôle of economic constraints.

During the historical retracing of these two manifestations of temporary stagnation, which extended over several decades, the significance of a neutral third criterion may initially be questioned. The actual value of the neutral category becomes apparent when stagnation persists. However, it would be wrong to assume that filtering for degenerative traits in research programmes guarantees that the remaining stagnant programmes will eventually become progressive (or, at worst, remain in constant stagnation despite the best methodology). After phases of intergenerational stagnation, a young and promising research programme may empirically surpass others, rendering the criterion of singularity no longer fulfilled. Similarly, after these extended periods, a reevaluation of the definition of a genuine problem compared to pseudo-problems may have occurred. Lastly, a reassessment of what was once considered significant can also take place.

The following candidates for stagnant research programmes have, unlike the aforementioned ones, remained without empirical successes to this day. They pertain to the problems of the two standard models in fundamental physics - particle physics and cosmology, based on quantum field theory and general relativity, respectively - along with their unification in quantum gravity<sup>8</sup>.

#### **Contemporary candidates for stagnant programmes:**

The physics beyond the Standard Model of particle physics (BSM) addresses all the missing explanations in the Standard Model of the 12 fermions, 12 gauge bosons, and the Higgs boson. Theories within this research programme involve, among others, the generation of neutrino masses, the nature of dark matter (which exceeds known matter by a factor of five), a unification of the three quantum-level understood interactions, the strong CP problem of quantum chromodynamics, and the hierarchy problem of the Higgs mass. Since the 1970s, a three-digit number of theories have been proposed, which remain empirically unconfirmed - an intergenerational stagnation exists (Criterion 1). Distinguishing from degeneration must be approached with caution: the hard core of the programme - phenomenologically, the Standard Model and mathematically, the framework of renormalizable, perturbative quantum field theory - along with some conservation laws and the principle of naturalness<sup>9</sup>, still appears today as the only option for progress in BSM physics. The particles and parameters of the Standard Model are outstandingly empirically confirmed (to some extent, the overwhelming success of the Standard Model is a considerable reason for the stagnation in BSM physics), and a more rigorous quantum field theory in four dimensions is yet to emerge. However, in distinguishing from malignant stagnation, challenges arise: assuming that all problems of the Standard Model are well-defined, both experimental and intellectual limits of humanity are in question. In particular, the high collision energies of accelerators or sensitivities of detectors that could detect the postulated particles, sometimes many orders of magnitude beyond the current

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<sup>8</sup>To avoid misunderstandings, we define physics as a fully empirical branch of science, where no kind of non-empirical theory evaluation suffices to confirm them and to generate an actual progressive problem shift in the Lakatosian sense.

<sup>9</sup>See Craig (2022) for an introduction.

standard, pose a central technical and financial hurdle for an empirically progressive problem shift. However, this assumption is not necessarily justified: firstly, it is unclear whether dark matter has got a particle character, or astronomical observations align better with modifications to the theory of gravity. Therefore, it is possible that the mystery of dark matter is more likely to be solved within the research programme of cosmology and gravity. Regarding the strong CP problem and the hierarchy problem, which represent anomalies to the principle of naturalness, it is debated whether both fundamentally pose a problem or are solvable in no research programme (Giudice 2017, Hossenfelder 2021). There is no guarantee for the validity of the so-called technical and 't Hooft-naturalness as metatheoretical principles in BSM physics. Perhaps the BSM programme needs to be liberated from degenerative elements first before being declared stagnant. The last criterion (distinguishing from insignificance) is unquestionably fulfilled due to high financial expenditures and thousands of particle physicists worldwide, as well as the socially recognized importance of the Faustian aim to understand "what holds the world together in its inmost folds".

Next, turn to a topic at the interface of particle physics and cosmology, the baryon asymmetry. In the 1920s, the idea of the Big Bang as the starting point of the Universe was developed. According to this theory, matter and antimatter should have been produced in equal amounts. Obviously, this is not the case, at least in our cosmic neighborhood. Thus, the ancient philosophical question remains: Why is there something rather than nothing? Its importance is thus certainly undisputed (Criterion 4). Decades later, Sakharov formulated three criteria that must be met to explain baryon asymmetry (1967): violation of baryon number, violation of CP symmetry, and a non-equilibrium situation in the thermodynamic system. To this day, a final explanation has not been achieved (Criterion 1). However, the singularity of the programme has been achieved (Criterion 2) by almost excluding cosmological explanations (no large antimatter regions in the Universe, Canetti et al. 2012) and electro-dynamical explanations (electric dipole moment, Roussy et al. 2023).

We stay in cosmology, specifically addressing the homogeneity problem. The classical Big Bang model could not fully explain certain observations, such as the homogeneous distribution of the cosmic microwave background radiation and the smooth structure of the Universe on large scales. Guth (1980) first proposed the idea of inflation to solve these problems. Inflation postulates that the Universe exponentially expanded in the first moments after the Big Bang and, in Guth's conception, requires a quantum field called the inflaton field. To date, there is no concrete known candidate (Criterion 1). Similar to the question of dark matter, however, the possibility arises that another research programme solves the homogeneity problem. The alternative: Inflation naturally follows from quantized spacetime in loop quantum gravity (Ashtekar, Sloan, 2010). Does this violate the singularity criterion? When we talk about the programme of quantum gravity, we will also attribute stagnation. In this particular case, it would be justified to relax Criterion 2, assuming competition has diminished despite the coexistence of multiple programmes, as all are in a phase of stagnation.

Nevertheless, in the homogeneity problem, as well as in the baryon asymmetry, the option of pseudo-problems remains. In this regard, one reads: "These are both finetuning problems that rely on the choice of an initial condition, which is considered to be likely. However, there is no way to quantify how likely the initial condition is, so the problem is not well-defined" (Hossenfelder 2019). Future debates in the philosophy of science will determine whether this argument violates Criterion 3 of benign epistemic limits. Otherwise, attributing stagnation is initially justifiable.

Finally, we discuss what is often referred to be the Holy Grail of physics (referring already to Criterion 4), being the unification of general relativity and quantum theory. Its incommensurability was noted shortly after the formulation of quantum theory, and it most probably presents a benign, well-defined problem beyond the current experimental range (Criterion 1 and 3). In the 1950s and 1960s, physicists like Feynman and DeWitt unsuccessfully attempted to describe gravity quantum mechanically. Loop quantum gravity is another approach developed in the 1980s by Ashtekar (1986) and Rovelli and Smolin (1987). It is based on the idea of breaking down space and time into smallest quantized units and uses a mathematical structure called loops. Despite intensive efforts, there is still no clear and experimentally confirmed theory of quantum gravity. String theory, if ever considered empirically testable, provides an alternative description of gravity<sup>10</sup>, but seems to face persistent experimental hurdles, allowing us to consider Criterion 2 as fulfilled.

#### **Lessons learned from individual examples:**

Given the recurring features of fundamental physics programmes that can be considered stagnant according to the analytically derived criteria, some additional criteria can be added, which may or may not be fulfilled in the case of stagnation:

- Regarding the Criterion 2 of singularity: If the programme is not lacking alternatives, stagnation still exists if the problem solution can be shifted to another stagnant research programme (e.g., dark matter, homogeneity problem). Moreover, the uniqueness can be supported if the positive heuristics of the programme has previously led to empirically progressive problem shifts (no-miracles argument, e.g., success of SM physics before the phase of BSM physics, quantization of gravity).
- Regarding Criterion 3 of benign epistemic limits: The research programme is already highly developed and addresses ultimate, fundamental questions (e.g., quantum gravity as the *Theory of Everything*, smallest building blocks of matter, geometry of the Universe) strongly challenging the cognitive limits of human beings.
- Regarding Criterion 4 of scientific value: The importance of the research programme is expressed through high societal relevance (e.g., nuclear fusion), the fundamental nature of its questions (e.g., as above), and thus the regular approval of new funding.

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<sup>10</sup>See (Oriti 2009) for further information on quantum gravity.

- To distinguish from malignant stagnation of the kind 3b): The research programme has been thoroughly examined for pseudo-problems as degenerative elements.

Concerning a distinction from malignant stagnation (3b), we can sharpen the repeatedly occurring two types of pseudo-problems in fundamental physics. We identified a (somewhat weaker) pseudo-problem, if formulating the problem simply happened in the wrong research programme, and the question in fact belongs to another programme (e.g., dark matter, homogeneity problem, quantum gravity). There is already the danger of degeneration by aiming to solve not an ill-defined, but at least a misplaced problem. The existence of pseudo-problems of another kind represents a serious degenerative element of a research programme: The problem would be ill-defined in any (not existing, but thinkable) research programme (e.g., strong CP problem, Higgs mass, cosmological constant, homogeneity problem, baryon asymmetry), because there is simply nothing to explain<sup>11</sup>. The formalization of this concept defines pseudo-problems in the context of Lakatosian research programmes as follows:

**Definition:** A pseudo-problem of the first kind  $P_1$  in a research programme  $F$  arises from an anomaly that is empirical in nature, but also contradicts a justified hard core of a research programme  $F'$  in another field and can *only* be explained in the latter through progressive problem shift, not in  $F$ .

A pseudo-problem of the second kind  $P_2$  in a research programme  $F$  is caused by an anomaly that contradicts a non-empirical, metatheoretical assumption of the hard core and cannot be explained in  $F$  or *any* existing and fictitious other research programme  $F'$ .

Pseudo-problems of type  $P_2$  historically arose, for example, in attempts to explain the distances of planetary orbits from the Sun. Kepler nested Platonic solids to reproduce the measured numerical values. However, regardless of the worldview of planetary orbits or gravitational theory one might adopt, in each case, these numerical values would not be a genuine anomaly. The pseudo-problem merely emerged from the confusion of fundamental mathematical relationships with coincidentally arising mathematical relations during the evolution of the solar system, like around billions of other stars with other numbers<sup>12</sup>. Potential pseudo-problems in today's fundamental physics could play a pivotal rôle to distinguish stagnation from degeneration in several programmes.

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<sup>11</sup>This diagnosis might or even should be a joint work of physicists and philosophers of science, as currently performed regarding the naturalness problems.

<sup>12</sup>This example, as well as possibly the unnatural parameters of the standard model of particle physics, reminiscently aligns with the characterization of *philosophical* pseudo-problems according to Popper, behind which, according to him, genuine epistemological problems exist (Popper 1994). The originally misstated problem shifts the domain to questions about the fundamental limits of human knowledge. In this case: Which numerical values in nature require explanation, and, most importantly, are explainable?

### **Outlook: Stagnation beyond fundamental physics:**

Several other research programmes can be identified that can be successfully categorized within the outlined framework of stagnant programmes: The transition from non-living to living matter remains an unresolved puzzle in biology to this day. The cure for certain diseases such as Parkinson's or diabetes has also been a decades-long endeavor with limited success. Successes in non-curative therapy over recent decades must, of course, be clearly distinguished from ultimate cures. Lastly, the mystery of consciousness and the emerging qualia should be mentioned, one of the oldest problems in philosophy and later in natural science, which remains almost entirely unanswered to this day. While neuroscience has provided many functional explanations, the actual problem remains untouched: "Consciousness, however, is as perplexing as it ever was. (...) We do not just lack a detailed theory, we are entirely in the dark about how consciousness fits into the natural order," D. Chalmers (1996) writes<sup>13</sup>. This listing of stagnant research programmes, of course, does not claim to be exhaustive but illustrates their interdisciplinary presence.

**4.2. The Threat of Exhaustive Stagnation in Future Research.** An important questions remain for the final justification of adding a third state of research programmes to the Lakatosian binary distinction of progress and degeneration: What signs, beyond individual examples, indicate the fruitfulness of introducing stagnant research programmes as an archetypal state of scientific practice?

Jones (2009) speaks of a "burden of knowledge," somewhat corresponding to Newton's saying "standing on the shoulders of giants." The ever-longer learning process required to reach the status quo shortens productive phases, limits expertise, and brings about a dependence on collaborative work, which is not always conducive to innovation. Stagnation eventually results from the absence of innovations.

According to Chu and Evans (2021), stagnation can arise as follows: Political measures of recent decades attempted to promote scientific progress by increasing the number of publications. However, this has the opposite effect: The flood of new works can deprive reviewers and readers of the mental space required for fully grasping innovative concepts. In other words, it becomes increasingly challenging to pick out promising ideas from the polyphony of new ones and concentrate on pursuing them: "(...) too many papers published each year in a field can lead to stagnation rather than advance" (2021, p.1). Additionally, self-reinforcing mechanisms within research programmes regarding citation and dissemination contribute to stagnation. Stagnation is thus caused, in both cases, by a lack of attention to promising proposals. Progress comes to a halt as too much focus is placed on mostly supported theories, which may potentially be dead ends. This phenomenon can indeed be considered a variant of Kuhn's conservatism, which unnecessarily prolongs the lifespan of unproductive theories. However, stagnation should not be understood as slowing progress (unless it stagnates on such timescales that it effectively equals complete stagnation).

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<sup>13</sup>However, according to the qualia eliminativist Dennett, attempts to explain the nature of qualia would pose a pseudo-problem of the second kind.

Stagnation must not be understood as a slowing-down progress (unless it persists on timescales beyond Criterion 1, making it de facto equivalent to complete stagnation): A steadily decreasing general productivity, as highlighted by Bloom et al. (2020)<sup>14</sup>, is too general an approach. Stagnation affects individual research programmes struggling with fundamental obstacles, not the symptoms of allegedly flagging research activity in general. Let's now focus on another study supporting the concept.

The (Park, Leahey, and Funk 2023) analyzed 45 million publications from six decades based on the following principle: Disruptive articles bring knowledge gains that influence other (sub)disciplines, leading them to be cited outside their own field. In contrast, consolidating articles with a limited sphere of influence confirm or add details to what is already known. The result: The introduced measure of disruptiveness collapses by 90 percent during the analyzed period. How does this justify the introduction of stagnant research programmes as a regularly occurring state in various disciplines?

Disruptive articles have the potential to replace research programmes (and ignite a competition, violating Criterion 2). In the evolutionary competition described by Lakatos, the weaker, less adapted candidate is consumed by the new research programme, whose hard core has emerged from the new disruptive principles. Degeneration and elimination occur. Consolidating articles cause this competition to falter<sup>15</sup>. The constant affirmation of the established no longer represents progressive problem shifts, nor is the hard core at risk; instead, it becomes more entrenched. This phenomenon is similar to the self-reinforcing mechanisms highlighted by Chu and Evans (2021). If the proportion of consolidating articles becomes predominant, more and more research programmes hover between progress and degeneration. Stagnation becomes, at worst, a widespread phenomenon<sup>16</sup>. However, it must be emphasized that the results presented in (Park, Leahey, Funk 2023) at most give indications to diagnose universal stagnation.

Regarding the analytically derived criteria, the following additional systemic criteria can be added especially if one is willing to apply the concept of stagnation beyond foundational physics:

- Regarding Criterion 2 of singularity: Not isolation due to lacking alternatives, but the opposite may also occur: a flood of competing works that cannot be adequately grasped simultaneously. The consequence is equal

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<sup>14</sup>The definition of knowledge growth is based here on the simple multiplication of research productivity and the number of researchers according to Solow (1957).

<sup>15</sup>There are specific measures to prevent this. The HRHR Policy (High-risk-high-reward) of the OECD (2023) is intended to promote the emergence of disruptive works.

<sup>16</sup>It should be noted that discussions about widespread stagnation in various disciplines remain highly speculative. The inclusion of artificial intelligence could trigger a scientific revolution in a few years, advancing the progress of knowledge generation and processing. The OECD lists numerous application fields and opportunities for artificial intelligence in various research programmes in (Nolan 2021). Nevertheless, it is unclear whether it can offer a way out of stagnation, especially regarding the fundamental intellectual or empirical hurdles that can arise in stagnant programmes. First successes in mathematical and logical thinking, essential for the development of new theories, such as foundational physics, are shown in (Trinh et al. 2024).

to a lack of alternatives - a coexistence of many isolated, unevaluated programmes.

- Regarding Criterion 3 of benign epistemic limits: The research programme is already highly developed and problems therein cannot be resolved by single minds, requiring collaborative teamwork that may inhibit innovation.
- Regarding Criterion 4 of scientific value: The initially promising research programme receives little attention compared to others for sociological reasons (unjustified insignificance).

## 5. CONCLUSION

With the introduction of a third, neutral state of stagnant research programmes, we aimed to present a collectively exhaustive categorization scheme, which now maps the entire landscape of research programmes. As a side effect, progressive and degenerative programmes finally become mutually exhaustive: In an attempt to rescue the classical, dichotomous categorization by specifying a concrete time frame, the absence of progress within this time frame would automatically lead to the diagnosis of programme degeneration without further justification. The historical definitional generosity by Lakatos himself seems inevitable in this case<sup>17</sup>. If, however, degeneration is not the only option missing experimental success can lead to, but a neutral stagnation as well, an unambiguous classification of research programmes should always be possible.

What lessons can be drawn from the diagnosis of stagnation? It preserves a research programme from being prematurely discarded, aligning with the Lakatosian perspective. With the four introduced criteria for stagnation fulfilled, one confirms to active researchers a sound and inherently promising methodology, hindered only by certain external constraints. A diagnosing stagnation provides an explanation for the absence of progressive shifts without diminishing its scientific value or attributing degeneration to it. It thus completes the spectrum of evaluation, which, without this third option, might overlook or misattribute a significant part of today's scientific reality, thereby unjustifiably degrading good scientific practice. Stagnation, within the Lakatosian framework, offers a theoretical justification for maintaining topics of paramount importance for a discipline and occasionally providing additional support, as the time for an eventual breakthrough, though in the distant future, no longer seems implausible. The third option ensures that the research programme has been thoroughly examined for potentially degenerative elements and is grounded in good scientific practice. Moreover, the discussed problem of non-decidability (progress or degeneration?) in the original work is addressed. Another advantage of the introduced trinity is that the Lakatosian model becomes considerably more applicable to current issues. By conceding that stagnant phases may be lengthy

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<sup>17</sup>It should be noted that the classical Lakatosian dichotomy works perfectly fine for the scope of his article - proposing a more rational alternative to the Kuhnian idea of scientific progress and the Popperian idea of "naive" falsificationism. Thus, this article did not intend to imply that Lakatos overlooked something obvious (even if biographical circumstances led Lakatos to rarely deal with stagnation in an era of great scientific progress).

but not endless, the evaluation is not predominantly left to future historians but allows for a more nuanced assessment of the current status quo.

Lastly, the historical classification of stagnant phases in significant research programmes (which eventually yielded progress again) can advise patience for current programmes in a similar state. Hence, radical ideas such as post-empirical criteria according to Dawid for theory confirmation can be refrained from initially. A discussion on exceptional cases in which the Lakatosian requirement of empirical-progressive shift can be softened by a potential post-empirical shift must be continued elsewhere.

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