The Comment of A. Pelissetto and E. Vicari (cond-mat/0610113) on our article (cond-mat/0609285) is based on misunderstandings of this article as well as on unfounded implicit assumptions. We clarify here the controversial points and show that, contrary to what is asserted by these authors, our paper is free of any contradiction and agrees with all well-established theoretical and experimental results. Also, we maintain that our work reveals pathologies in the (treatment of) perturbative approaches performed at fixed dimensions. In particular, we emphasize that the perturbative approaches to frustrated magnets performed either within the minimal subtraction scheme without $\epsilon$-expansion or in the massive scheme at zero momentum exhibit spurious fixed points and, thus, do not describe correctly the behaviour of these systems in three dimensions.
Before answering in detail to the technical points raised in Comment [1] we would like to point out that the authors of [1] very often quote our results in a biased way, as if our article [2] was written to emphasize the differences between the perturbative fixed dimension (FD) approaches and the non perturbative renormalization group (NPRG) approach (named functional renormalization group (FRG) approach in [1]), an approach that has been employed by some of us in previous articles to investigate the physics of frustrated magnets [3, 4, 5, 6].

This is absolutely not the case. Our aim, in our article [2], was to shed light on the discrepancy between the different perturbative approaches, namely the $\epsilon$-expansion and the FD approaches. The NPRG results were quoted just as side remarks — to show the agreement between NPRG and $\epsilon$-expansion — and we have only dealt in [2] with perturbative methods.

The existence of such a discrepancy is evident from Fig.1 that gathers the curves $N_c(d)$ — the critical value of the number of components above which the transition is predicted to be of second order — obtained from the different RG approaches (the perturbative ones and also that obtained with the NPRG). Fig.1 is, in particular, very symptomatic of the existence of a discrepancy between the different perturbative approaches since one clearly sees that the curves $N_c^\epsilon$ (obtained within the minimal subtraction ($\overline{\text{MS}}$) scheme with an $\epsilon$-expansion [8]) and $N_{c\text{FD}}$ (obtained within the $\overline{\text{MS}}$ scheme without $\epsilon$-expansion [7]) are incompatible for $N \lesssim 6$ and this, independently of the results obtained within the NPRG approach.

![Diagram](image_url)

**FIG. 1**: Curves $N_c(d)$ obtained within the $\overline{\text{MS}}$ with an $\epsilon$-expansion ($N_c^\epsilon$), the $\overline{\text{MS}}$ scheme without $\epsilon$-expansion ($N_{c\text{FD}}$) and the NPRG approach ($N_{c\text{NPRG}}$). The part of the curve $N_{c\text{FD}}$ below S corresponds to a regime of non-Borel-summability.

In this respect, we are a little bit surprised that the authors of [1], in their Comment, do never mention this discrepancy and not even the curve $N_c^\epsilon$ (that have been obtained from a five-loop computation [8]) displayed in Fig.1 from which originates an important part of the controversy. Instead, they focus on the “difference between the perturbative results and those obtained by using

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1 As a precaution we emphasize, as in [2], that a large part of the curve $N_{c\text{FD}}$ obtained within the FD approach, typically that below the “turning point” S, corresponds to fixed points (FPs) that are situated out of the Borel-summability region. Thus the curve $N_{c\text{FD}}$, in its whole, should not be taken too much seriously. However if we take, as estimates of the error-bars associated to this curve, those provided by Calabrese et al. in their computation [7] one can safely trust in a finite portion of the part below S.
Implicitly in their Comment, and more explicitly in their article \[7\] the origin of the problem that we raise would come from the inability of the $\epsilon$-expansion to provide a correct description of the three dimensional physics. In contrast to the authors of \[1\] we think that the agreement between the curves $N'_c(d)$ and $N_{cNPRG}^c(d)$ — that are obtained from drastically different computations — is rather remarkable and leads to trust both the $\epsilon$-expansion and the NPRG approaches. Also, the FD approach (including calculations in the massive zero-momentum (MZM) scheme or \(\overline{\text{MS}}\) scheme without $\epsilon$-expansion) appears to be very isolated as it is the only one leading to the prediction of criticality in $d = 3$ in the Heisenberg and XY cases. This is precisely this fact that leads us to search for — and actually to find — a flaw in the FD approach.

It is now time to answer in detail to the points raised in \[1\].

II- DETAILED ANSWERS TO THE COMMENT

1) According to the authors of \[1\] there is “no theoretical justification” for the requirement that “a physical FP of a given Hamiltonian must survive up to $d = 4$ [...] and become the Gaussian FP in this limit”. We shall comment on the word “physical” in point 2) below. We start here by discussing the question of the “survival” of a FP as a Gaussian one in $d = 4$. We strongly disagree with the statement that there is “no theoretical justification” for this. Indeed, in Field Theory as well as in Statistical Mechanics, the choice of a Hamiltonian implies a choice of the most relevant operators of the theory. The common belief which, as far as we know, is confirmed by all well-controlled situations, is that this choice relies on (i) the existence of an upper critical dimension, (ii) the possibility of a “naive” power counting performed around a Gaussian fixed point (FP) and, therefore, (iii) the fact that the theory is infrared free in this dimension. In the case where there exists, in a dimension $d = 4 - \epsilon$, a non-trivial FP, whose coordinates are of order $\epsilon$, the flow is well controlled everywhere between the UV and the IR scales. When this FP can be extrapolated down to $d = 3$, the perturbative FD approaches performed directly in $d = 3$ are expected to safely describe the physics in this dimension although they do not refer explicitly to the upper critical dimension. Moreover, still in this case, there are strong indications that the field theory can be “constructed” directly in $d = 3$ \[8\]. This is, in particular, the case of the $(\phi^2)^2$ theory where the $\epsilon$-expansion predicts a FP in $d = 3$, which validates the MZM perturbative scheme. Thus, at least in all well-controlled situations, and contrary to what is asserted in \[1\], there is a link between the FD approaches in $d = 3$ and the $\epsilon$-expansion and, thus, an implicit link between this FD approach and the Gaussian behaviour of the critical theory in the upper critical dimension.

Now consider a theory where a FP found in $d = 3$ is related by continuity to a non-Gaussian FP in $d = 4$. (This is what we have found in the FD perturbative approach for frustrated magnets for the FP $C^+$ when — and only when — $N \lesssim 6$ and for the FP $P$ found in the cubic model for $v < 0$.) If this turned out to be true, and not an artefact of the FD approach (whereas we think it is), this would mean that the link between the $\epsilon$-expansion and FD approaches would be broken. Thus the theory would have no more a well identified upper critical dimension and it would be non trivial in $d = 4$. For $(\phi^2)^2$-like theories describing the frustrated and cubic cases this would be completely new, unexpected and, if true, would be of utmost interest!

As a conclusion, we do think that there is an implicit but strong link between the trivial IR behaviour of $(\phi^2)^2$-like theories in $d = 4$ and the validity of the description of the critical physics in $d = 3$ by means of FD approaches.

2) In Comment \[1\], the words “physical” and “survive” in the sentence: “a physical FP of a given Hamiltonian must survive up to $d = 4$ [...] and become the Gaussian FP in this limit” are clearly, from what follows, understood by the authors as “having real coordinates”. This leads them to conclude that “according to this criterion, all FPs with $N \lesssim 21.8$ should be considered as spurious”
and that “this condition is very restrictive and contradicts several well-accepted theoretical results”. This, by no ways, corresponds to what is written in our article. Indeed our criterion to consider a FP either as physically relevant or as spurious, is based on an analysis of the roots of the $\beta$ functions that can be either real or complex. We have explicitly written in foonote [27] of our article that “If one follows $C_{\star}^{\mathrm{FD}}$ along a path starting in $d = 3$, going to $d = 4$ and crossing $N^{\mathrm{FP}}(d)$ above $S$, its coordinates become complex in $d = d_{\star}(N)$ and go to zero for $d = 4$ where it is thus the Gaussian FP”. Our criterion does not consist in rejecting FPs whose coordinates become complex when the dimension $d$ is increased but in rejecting those that are non-Gaussian in $d = 4$. As a consequence all FPs in $d = 3$ having $N \gtrsim 6$ (that is those “above the singularity $S$”) are, according to us, physically relevant; and vice versa. This is, again, explicitly written in page 3, column 2, where we say that, in the frustrated case, the fact that the FPs are not Gaussian “happens for all values of $N \lesssim 6$” and, by no means, for all values of $N \lesssim 21.8$!

As a conclusion this part of Comment [1] is based on a trivial misunderstanding of our article. Our criterion is by no means “very restrictive” and, contrary to what is asserted in [1], agrees with all “well-accepted theoretical results”.

3) According to the authors of [1], “We must observe that the perturbative results of Ref.[3] find no difference between the FPs with $N \gtrsim 6$ […] and those with $N \lesssim 6$”. This statement is incorrect since it ignores an important fact that has been missed by Calabrese et al. [1], i.e. the existence of a singularity $S$ in the $(N,d)$ plane which makes the coordinates of the FP $C_{\star}$, $u_{\star}^1$ and $u_{\star}^2$, multivalued functions of $N$ and $d$, as explained in our footnote [27] of [2]. This fact is manifest when one follows the FP $C_{\star}$ by continuity along a path encircling the singularity $S$: after a round trip, the coordinates of the FP thus obtained have changed. The existence of such a singularity is also at the origin of the fact that the FPs obtained in $d = 3$ are or are not Gaussian in $d = 4$ depending on whether the path followed to reach the upper critical dimension passes above or below the singularity $S$. Therefore, contrary to the statement of the authors of [1] there is a fundamental difference between the cases $N \lesssim 6$ and $N \gtrsim 6$.

4) According to the authors of [1] “the difference between the perturbative results and those obtained by using the functional renormalization group (FRG) [5-7] is only quantitative […]. But there are no conceptual differences as the authors [of this reply] apparently imply.”

First we, again, emphasize as in the Introduction to this Reply, that the purpose of our article is not to oppose nonperturbative and perturbative approaches. It is to try to understand why two perturbative approaches relying on the same renormalization scheme ($\overline{\text{MS}}$ scheme) and differing only by the way of solving the FP equations lead to qualitatively, and not only quantitatively, different results.

Second, concerning the nature of the difference between the FD approach and the $\epsilon$-expansion, we disagree with the authors of [1]. The very questions we have addressed are (i) to know whether there exists a FP in $d = 3$ for $N = 2$ and 3 (ii) whether this FP is non-Gaussian when followed in $d = 4$ and, finally, at a more technical level, (iii) does there exists a singularity $S$ in the coordinates $u_1^\star$ and $u_2^\star$ of $C_{\star}$ taken as functions of $N$ and $d$. To all these questions the answer is positive in the $\overline{\text{MS}}$ scheme approach without $\epsilon$-expansion whereas it is negative in the $\epsilon$-expansion (and in the NPRG) approaches.

We think that these discrepancies are, indeed, “conceptual”. Moreover, we believe that we have identified the very origin of this “conceptual” difference: the FP identified within the $\overline{\text{MS}}$ scheme without $\epsilon$-expansion corresponds to a spurious solution of the FP equations that are solved at fixed $d$.

5) According to [1] “The behaviour observed here is analogous to that found in the Ginzburg-Landau model of superconductors […] The criterion proposed in Ref.[1] would thus predict a first-order transition for the physical case $N = 1$, contradicting experiments [9], and also general duality arguments [10], FRG calculations [11], and Monte Carlo simulations [12].” The answer given
in point 2) above also applies to this case: our criterion would obviously not lead to predict a first-order transition in superconductors since our criterion has nothing to do with the critical value of $N$ in the upper critical dimension $d = 4$. In particular, we emphasize that our considerations absolutely do not exclude the existence of a FP for $N$ smaller than $N_c$.

6) The authors of [1] state that “There is another condition that is crucial: the three-dimensional FP must be connected by the three-dimensional renormalization-group flow to the Gaussian FP [4,15]. If this is the case, at least for the massive zero-momentum (MZM) scheme, one can give a rigorous nonperturbative definition of the renormalization group flow and of all quantities that are computed in perturbation theory”. They also state that “In a well-defined limit [...] long-range quantities [...] have the same perturbative expansion as the corresponding quantities in the continuum theory for the MZM scheme” and that therefore “everything is defined nonperturbatively and rigorously in three dimensions and there is no need of invoking the existence of a four-dimensional FP”. We agree with all these statements (that we have already partially discussed in point 1). However, there is a fundamental assumption underlying all these statements: they are valid provided there exists an IR stable FP in $d = 3$...which is precisely the dubious point!

As a conclusion, all the arguments invoked in [1] about the nonperturbative and rigorous definition of the MZM scheme approach, as well as the claim that the behaviour of the theory in $d = 4$ would not be relevant for the three dimensional physics, would be of interest...if there were no controversy about the critical behaviour of the frustrated systems in $d = 3$.

7) According to [1] our statement that for $N = 2,3$ one can follow the FP $C^+$ up to $d = 4$ is “incorrect and is based on an incorrect use of the conformal-mapping method”. We completely agree with, and are aware of, what is stated in [1] about limitations of the resummation procedure used in [2]. Concerning this point we have written in our article that when one follows the FPs from $d = 3$ to $d = 4$ “the FPs $P$ and $C^+_{FD}$ lie out of the region of Borel-summability in $d = 4$. Thus their coordinates cannot be determined accurately”. However this point is completely irrelevant to the question raised in our article: the identification of the (non-)Gaussian character of the FP in $d = 4$. And the non-Gaussian character of the FPs with $N \lesssim 6$ at $d = 4$ is doubtless. Indeed, let us suppose on the contrary that a FP, with $N \lesssim 6$, followed from $d = 3$ to $d = 4$ is, actually, a Gaussian one in $d = 4$. In this case, its coordinates just below this dimension would be extremely small and thus it could obviously be obtained within perturbation theory without any resummation procedure. Thus, as we clearly state in our article, our procedure is completely valid to decide whether the FP is or is not Gaussian in $d = 4$ (although not sufficient to determine its coordinates if it is not Gaussian, what we do not mind anyway).

We add, as we have already emphasized above, that the occurrence of a non-Gaussian FP in $d = 4$ is deeply related to the existence of a singularity $S$ in the $(N,d)$ plane. An important fact is that this singularity $S$ lies either inside or just on the border of the region of Borel-summability. Its existence is thus doubtless, according to the standards of [1].

As a conclusion the arguments raised by the authors of [1] concerning our use of the conformal-mapping method are completely irrelevant for our purpose.

8) Concerning numerical results, according to the authors of [1] “All numerical and experimental results are consistent with the predictions of perturbative field theory” since “the existence of a stable FP does not imply that all systems with the given symmetry undergo a second-order phase transition.”. We — obviously — agree with the last argument that the existence of a FP in a field theory does not imply that all systems undergo a second order phase transition. However we disagree with the conclusion (that the numerical and experimental results are consistent with perturbative theory) which is drawn from it. Indeed:

1) While the first order behaviour does not contradict the existence of a FP one could expect, from the existence of such a FP, a basin of attraction of finite extension and, thus, that some materials or numerically simulated systems exhibit a second order behaviour with the predicted
critical exponents. This is not the case apart from an isolated simulation performed on a lattice discretization of the Ginzburg-Landau Hamiltonian that apparently leads to a second order behaviour \[7\]. We say “apparently” since we have been used to claims of the existence of second order behaviours for systems that have been subsequently discovered to undergo a weak first order transition. (As an example, stacked triangular antiferromagnets (STA) have long been thought to undergo continuous transitions until larger system sizes have been considered.) This could also be the case for the simulation performed in \[7\].

2) The numerical simulations leading to a (apparent) second order behaviour (it is now recognized that most of them are of weak first order) were considered in “substantial agreement” \[10\] with the six-loop computation performed in the MZM scheme \[10\], thus giving a credit to the existence of the FP identified in this way. But, strangely, the fact that more accurate simulations eventually found first order instead of second order transitions has never been considered by the authors of \[1\] and \[10\] as contradicting the results of the FD approach. This means that the existence of second or first order phase transitions equally confirm the predictions of this approach!

3) Rather than focusing on the first order behaviour in general it is more instructive to address the question of the occurrence of weak first order behaviour. Indeed, in presence of a standard FP characterizing a second order phase transition, one can expect weak first order behaviour. However this can happen only for very special initial conditions of the RG flow such that the point representative of the system in the space of couplings is in the runaway region — leading to first order behaviour — but very close to the boundary between the first and second order regions so that the flow is slow and produces a very large correlation length. In a numerical investigation of a whole family of frustrated magnets it has been shown by A. Peles, B.W. Southern and some of the present authors \[11, 12\] that, actually, weak first order transitions occur generically in these systems. As these authors have argued, this contradicts the usual interpretation given for the “occasional” weak first order behaviour described above. Thus there must exist another explanation to these generic weak first order behaviour. The NPRG approach \[6\] provides such an explanation: it shows that the weak first order behaviour that occurs in frustrated magnets does not rely on the usual explanation above but rather on the existence of a generic slow flow (than occurs even in absence of a FP). Thus, contrary to what the authors of \[1\] say the “quoted results [17-19]” are not irrelevant for the discussion. On the contrary: (i) they show that frustrated systems initially thought to undergo a second order phase transition actually have first order behaviour (ii) they show that this first order behaviour is generic (iii) they point out a weakness of the perturbative FD approaches that are unable to explain the existence of generic weak first order behaviour.

Finally the authors of \[1\] argue that “the results of Ref.[3] and of Ref.[20] - they find continuous transitions for \(N = 2\) and \(N = 3\), respectively - are only consistent with the presence of a stable FP and thus do not support the scenario of Ref. [1]”. We recall that the past experience in the domain of numerical simulations of frustrated magnets has been largely controversial (see \[6\] for instance). Also in most cases improving the method of analysis (for instance by using Monte Carlo Renormalization Group methods \[13\] or dynamical methods \[12\]) has lead to the conclusion of a weak first order transitions.

9) Concerning the experimental situation, according to the authors of \[1\], the “results of Ref.[21] cited in Ref.[1] are perfectly consistent with perturbation theory. We discussed in detail easy-axis systems in Ref.[22] and showed two possible phase diagrams compatible with perturbation theory”. Here the authors refer to their own work with P. Calabrese on multicritical behaviour in frustrated systems \[14\] that would explain the first order behaviour found in CsNiCl\(_3\) \[15\]. However, according to the authors of \[14\] (abstract of this article): “the transition at the multicritical point is expected to be either continuous and controlled by the \(O(2) \otimes O(3)\) fixed point or to be of first order”. They also add in their Comment that: “Due to the ‘focus’-like nature of the FP \[23\] the approach to criticality may be quite complex. Effective exponents may even change nonmonotonically . . .”. As in point \(8\) above, such statements make any experimental (and numerical) behaviour, of first or
second order with any set of critical exponents, to be compatible with perturbation theory!

Concerning easy-plane systems, according to the authors of [1], “all experiments observe continuous transitions, and thus they are compatible with the perturbative results”. We emphasize here that there is no definitive statement about the transition in these systems. On the contrary, some of the present authors have shown [6] that several facts go against the belief that the transitions are of second order: the critical exponents found are non universal, scaling laws are violated, the anomalous dimension — or exponent η — is negative, etc. It is absolutely not excluded that, as in the case of CsNiCl₃ and in almost all numerically simulated models that were believed to undergo a second order phase transition, all easy-plane systems will be finally claimed to undergo first order transitions.

10) Concerning the cubic model, the authors of [1] contest its use since the situation would not be, in this case, “well established”, contrary to what we claim. To support this statement they invoke — in this case too! — the possible failure of the ε-expansion in the region v < 0 that we precisely investigate in our article.

1) To our knowledge this is the first time that the use of the ε-expansion is questioned in the context of the study of the cubic model. On the contrary, all approaches used to investigate, for instance, the critical value N′c (not to be confused with the Nc of frustrated magnets) above which the cubic FP is stable, do coincide. For instance, according to an article [10] gathering J.M. Carmona and the authors of [1], it is found that N′c = 2.89(4) from a six-loop perturbative approach performed in d = 3 and Nc = 2.87(5) from a five-loop ε-expansion approach.

2) According to the authors of [1] something special should happen in the case v < 0. Indeed according to them the critical behaviour of: “the antiferromagnet four-state Potts model on a cubic lattice [26-28] […] should be described by the N = 3 cubic model with v < 0 [29]”. They add that “Contrary to the claim of Ref.[1], all numerical results are consistent with a continuous transition: at present there is no evidence of first order transitions”.

While we did not write anything in our article about the behaviour of the antiferromagnetic four-state Potts model we, however, completely disagree with the statement that “all numerical results are consistent with a continuous transition” in these systems. It is true that some simulations [17] have lead to the claim of a second order behaviour for these systems. However it has been recognized with further investigations that this conclusion was hasty since [18]: “the Hamiltonian for the q = 4 antiferromagnetic Potts model on both simple cubic and body-centered-cubic lattice is far apart any fixed point and the largest simulated size L = 96 is still insufficient to extract asymptotic critical behavior. However, we have found that the Hamiltonian moves towards the strong (100)-type anisotropy (large negative v) direction as it is renormalized. Since the recent field-theoretical [12] and Monte Carlo studies indicate the absence of RG fixed point in the v < 0 region, we expect that the transition is a first-order one.” We do not see here what is consistent with a continuous transition.

Also we note that the authors of the Comment themselves, with their collaborators, claim in Ref. [13] (P2) that “the four-state [antiferromagnetic Potts] model is expected to show a first-order transition” and (P5) that “In the four-state case, the weak first order transition expected in the pure case should not be softened by random dilution”.

As for the cubic model itself in [16] (footnote [10]) they claim that “A high-temperature analysis on the fcc lattice indicates that these models have a first-order transition for N > 2.35 ± 0.20. This is consistent with our argument that predicts the transition to be of first order for any v₀ < 0 and N > Nc. More general models that have Eq.(1.1) [the cubic model] as their continuous spin limit for v₀ → −∞ have also been considered in Ref.12. The first-order nature of the transition for negative (small) v₀ and large N has also been confirmed in Ref.11.” Also in [20], where a six-loop computation has been performed, they claim (P1) that “for w < 0 [w being the coupling of the cubic term], the RG flow runs away to infinity, and the corresponding system is expected to undergo a weak first-order transition”. Again we do not see any controversial situation here: everything seems to favor a first order phase transition contrary to what is claimed in Comment [1].
While we acknowledge that the description of the antiferromagnetic four-state Potts model could be problematic, it certainly does not question the structure of the flow diagram of the cubic model in the region \( v < 0 \) as the authors of [1] suggest, at least in their Comment.

11) Still on the cubic model, according to the authors of [1] “the analysis of the perturbative series does not provide compelling evidence for the existence of a new FP in cubic models with \( v < 0 \) and \( N = 3 \).” To support this claim the authors of [1] have repeated our analysis on the cubic model with the help of a FD analysis and have shown that only one half of the different resummations leads to a FP. Thus our “demonstration” would not be convincing. Also comparing their percentage of FPs obtained in the cubic case to that obtained in the frustrated case, the authors of [1] contest our statement that the cubic model show “similar convergence properties” to the frustrated one.

First, we note that the authors of [1] have confirmed an important result of our article which is that FD approaches generate dubious FPs that are not obtained within the \( \epsilon \)-expansion.

Second, the criterion of the authors of [1] for accepting or rejecting a FP is very vague. They consider ranges of values of the resummation parameters \((-1 \leq \alpha \leq 5 \) and \( 2 \leq b \leq 20 \)) that are completely arbitrary. For instance, by considering larger ranges of values of \( \alpha \) and/or \( b \), they could obtain a percentage of rejections as small as wanted.

Third, contrary to what is claimed by the authors of [1], our statement that the cubic-model results show “similar convergence properties” as in the frustrated case is largely justified. Indeed, we have extensively studied the properties of convergence of the exponents \( \omega \) and \( \nu \) at the FP using criterions of best apparent convergence or principle of minimal sensitivity. We have been lead to the conclusion that there exists an error between the four and five loop results that is of order 40 \% in both frustrated and cubic cases. Our study, based on these criterions, is far more instructive that statistics performed varying the resummation parameters on arbitrary domains that does not provide any reliable information on the nature (spurious or physical) of the FP.

Finally concerning the sentence “The difference between the two cases [cubic and frustrated] […] are so evident, that no additional comment is needed !”. We have no problem to admit that the cubic FP could display weaker stability properties with respect to variations of the resummation parameters than the frustrated one, what remains to be proven using other criterions than those of [1]. However, this is not a relevant point for our purpose. Indeed, there is absolutely no reason that the spurious FPs obtained in two different models display the same (quantitative) behaviour. The relevant point is a comparison to the Ising, or more generally \( O(N) \), model for which, at the same order, one has an error one hundred times smaller than in both the frustrated and cubic cases! As the authors of [1] say “Differences are so evident, that no additional comment is needed !”.

To conclude, our study of the cubic model is appropriate to demonstrate the spurious character of the FP found in the frustrated case. Indeed, (i) it is largely less controversial than the frustrated case (ii) it admits a FP that very probably is a spurious one (iii) the properties of the convergence of the physical quantities at this FP are very close to those found in the frustrated case if one adopts criterions based on optimization of the results.

12) The authors of [1] propose a way of “Reconciling the different approaches”. According to them the differences observed between the perturbative FD and FRG approaches would be related to the “crudeness of the approximations used in Refs.[5,7] [the FRG approach]”.

First we note, again, that these authors focus on the opposition between perturbative and non-perturbative methods, which is not our true motivation. However let us imagine that these authors are right. Then, how to explain the agreement between the five-loop \( \epsilon \)-expansion (that is certainly not a crude approximation according to the standard) and FRG approach ?

Second, we definitively reject the argument of crudeness of the FRG computations. Indeed, as explained at length in [6], the FRG analysis of the frustrated magnet succeeds several tests: agreement with the results obtained within a low-temperature expansion of the nonlinear sigma model around two dimensions [21], agreement with the weak-coupling expansion of the Ginzburg-Landau model
around four dimensions \[22, 23\], agreement (better than 1 \%) with the large-N results (performed up to order \(1/N^2\) \[23, 24, 25\]) in any dimensions between 2 and 4 dimensions, agreement with the \(N = 6\) Monte Carlo results \[26\], agreement with the \(\varepsilon\)-expansion performed at five-loops everywhere between 2 and 4 dimensions \[8\]. Finally, the stability of the results with respect to changes of the field content has been also checked by using more refined truncations.

One cannot say that the same checks have been performed for FD computations. In fact, there is even a discrepancy between the different FD results if one considers the MZM scheme in which no FP is found between \(N = 5\) and \(N = 7\) (while we recall that it is well established from Monte Carlo simulation that there is a second order phase transition in the \(N = 6\) case). Also, as said above, if one considers the numerical and experimental situations there is only one case, a Monte Carlo simulation \[7\], where a second order transition with critical exponents close to the perturbative FD predictions have been found. However, looking at the most recent experiments and numerical simulations one finds more and more systems where the transition, considered in the past as a second order transition, is discovered to be, in fact, of weak first order, in contradiction with the FD perturbative approaches.

### III- CONCLUSION

The points raised by the authors of \[1\] come from either misunderstanding of our article or from unfounded assumptions, as we have shown here. Also, contrary to what is claimed in the Comment \[1\], our results agree with all existing and well established theoretical or experimental results. Moreover, we maintain to have provided a solution explaining the manifest contradiction between the \(\varepsilon\)-expansion (that agrees with the NPRG approach) and the FD perturbative approaches that predict an unobserved second order critical behaviour both in frustrated magnets and in the cubic model.

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