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Journal Name:	International Research Journal of Pure and Applied Chemistry
Manuscript Number:	2014_IRJPAC_13156
Title of the Manuscript:	Delayed Shifts in pH Responses to Weak Acids in Spring Water Exposed to Circular Rotating Magnetic Fields: A Narrow Band Intensity-Dependence
Type of the Article	Original Research Article

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This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty**', provided the manuscript is scientifically robust and technically sound.

To know the complete guideline for Peer Review process, reviewers are requested to visit this link:

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PART 1: Review Comments

	Reviewer's comment	Author's comment <i>(if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)</i>
<u>Compulsory</u> REVISION comments	The paper titled "Delayed Shifts in pH Responses to Weak Acids in Spring Water Exposed to Circular Rotating Magnetic Fields: A Narrow Band Intensity-Dependence" measures the effects of a modulated magnetic field configuration on the measured delayed response in pH titration experiments as a function of magnetic field intensity. The pH titration is a biologically relevant mixture consisting mainly of sodium bicarbonate with acetic acid as the titrant – i.e. baking soda and vinegar. The author's observe a "holding" duration that seems to be influenced as a function of the magnetic field intensity. At the largest magnetic field intensity, they observe no changes in pH until the magnetic field is removed. While the experiment design and results are interesting, there are a number of concerns with this report. Materials & methods (Kindly comment on the suitability and technical standards of the methods. Sufficient details of the methods/process should be provided so that another researcher is able to reproduce the experimentsdescribed) 1). Line 115: 4 mM of HCO₃; 1.7 mM Ca; 76 μM of Cl; 1.316 mM of Mg, 41.9 μM of NO₃, 61 μM of SO₄, 17.9 μM of K and 43.5 μM of Na – The chemical are ions and should have the appropriate charges associated with. 2). Line 148: Is there supposed to be a reference	The charges have been added as requested.



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	after settings? Line 132, 146 – 155: It seems that the magnetic field configuration is central to the observed results. There is a lack of a reasonable discussion on why this magnetic field configuration beyond simply saying the “The spectral analyses of the two patterns are shown in Figures 1 and 2”. Which peaks are considered important for the observation of magnetic field effects on the pH? 3). Line 157 – 164: The exact volume/concentration of added acetic acid is unclear. An initial volume of 50uL at 0.83 M acetic acid was added for the first field configuration. However, the manuscript says 50 uL was added every minute for 16 minutes, for a total of 17 injections? The next sentence says a total of 9 injections. This makes no sense. Moreover, why not show the pH meter data as a function of time? 4). Line 174-179: The statistical analysis is not only confusing here but also the rest of the paper. For example, “polynomial analyses revealed the linearity of the curve”. What curve? Results & discussion (Kindly comment on: 1. Are the data well controlled and robust? No. a). Line 188 – 192: “Post hoc analysesindicated the major source of the difference was between the holding durationsbelow 1.3 mG, which did not differ significantly from each other, and the holdingdurations for intensities above 3.3 mG, that did not differ significantly from eachother”. I am not sure what the author is saying here. If they are referring to figure 3, it seems the holding	No there is not. These are empirical results. We included the spectral patterns because they may be more relevant than simply the raw patterns. This has been elaborated in the revision. The apparent discrepancy has been corrected. Whoops, apologies for the misuse of term, it should be “best fit” relation. This is corrected. Actually the data are very robust and controlled. The absence of statistically significant differences below 1.3 mG and
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	durations differ significantly from each other. b). "Polynomial analyses indicated that the first order best fit curve was linear¹⁹³ and there was no significant deviation from this term". Again, if the author is referring to figure 3, why not just show the linear fit in the graph. The plot doesn't look linear at all. Moreover, for all the statistical analysis that was conducted, why not plot error bars on figure 3? c). Line 195: On the y-axis for figure 4 should be ΔpH. The way the figure is shown, it appears that the pH goes from 3 to 0 pH. The authors say this in the discussion but the figure is confusing. d). Line 202: Do the authors mean figure 3 or figure 5? This is unclear. If they mean figure 3, then this paragraph should come before discussion of figure 4. Once again, the discussion of the statistics is confusing and not clear. e). Line 225-228: There is no discussion about this figure, unless the previous paragraph is the discussion. 2. Authors should provide relevant and current references during discussion. Yes. 3. Discussion and conclusions should be based on actual facts and figures. Biased claims should be pointed out 1).Line 268 – 272: "If the sample size.....net change	above 3.3 mG (between the higher dosages) gives credence to the threshold effect. This is a common observation in dose-dependent pharmacological studies in complex systems. This is corrected as you see in the revision. Apologies, you are quite correct; figure caption numbers have been corrected and changed because now we have another figure as requested by another reviewer. That is correct
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	in pH". This sentence seems speculative and is confusing. 2). Line 286: "in other words, the increased would be about a factor of 10". I am not sure what the "increased" means. 3). Line 299-305: The authors suggest that when the stoichiometry of bicarbonate=acetic acid, the magnetic field effect was no longer maintained. Why not use different biocarbonate concentrations to test this hypothesis? 4. Are statistical analyses must for this paper? If yes, have sufficient and appropriate statistical analyses been carried out?) There is considerable statistical analysis carried out for these results. However, the format and discussion of the analyses is difficult to follow. For example: Line 185-188 "Oneway analysesof variance as a function of intensity indicated a significant difference[F(1,49)=24.95, p <.001] between the intensities. The intensities explainedabout 85% of the variance (effect size) in the holding durations" Conclusion (Is the conclusion supported by the data, discussed inside the manuscript? No. Line 281: The authors calculate the energy of the magnetic field. Why not use a simple sinusoidal magnetic field with the same energy to corroborate their results?	Perhaps it is now more clear in light of the clarifications within the revision. This ambiguity has been corrected as indicated That is a most excellent idea. We plan to pursue this in the near future. We have clarified the discussion on statistical analyses. However we think that this class of phenomena is subject to the same types of multiple unknown variances found in biological sciences; hence the employment of statistical analyses. Effect size is a standard method of indicating on large the effect is for statistical studies. We did not employ sinusoidal magnetic fields because we have shown, over the last
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	Line 307-316: I would suspect that the thermal energy would cause the hydrogen bonds to break. This experiment as a function of temperature would support their hypothesis here. Line 318-325: There is no evidence that this experiment is quantum in nature. Overall, this paper has many features of a rough draft and the conclusions do not support the evidence presented. Although the authors appear to observe a correlation between magnetic field intensity and measured pH, they do not carry out appropriate controls that support the conclusion. One example is to use an alternative measure of pH, such as a colorimetric indicator. Concentration and temperature dependence are other controls that should be taken into account as well as a simple magnetic field configuration. While the statistical analyses seem comprehensive, the overall structure is difficult to follow. The experiments are simple enough, as this type of titration is done in freshman chemistry labs, that a thorough investigation is needed to support the claims. Are all the references cited relevant and adequate? Yes Are there any other suitable current references authors need to cite?	40 years, that they should don't work unless the intensities are a factor of 1000 or more greater. The thermal energy idea is brilliant, although it would not be a kT problem. For the use of these fields for interfering with melanoma cells we have noted that a shift of only 1 deg in the incubator does not affect typical cells but will clearly influence the effects of the magnetic fields. The term quantum is used in a metaphorical sense with respect to discrete packets of energy within the system. Colorimetric indicators are interesting and we have employed them. However we find their resolution is less than that of our fast frequency pH meters. Indeed the titration could be done in a freshman chemistry class (we do it) and that's the point. With the appropriate advanced technology even simple and gross measurements can reveal powerful effects that challenge contemporary concepts.
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<u>Minor</u> REVISION comments		
<u>Optional/General</u> comments		