SCIENCEDOMAIN *international*
www.sciencedomain.org

SDI Review Form 1.6

Journal Name:	**International Journal of Plant & Soil Science**
Manuscript Number:	**2013_IJPSS_8827**
Title of the Manuscript:	**Seedling Emergence and Seed Germination of Shepherd's needle (Scandix pecten-veneris) as Affected by Seed Weight and Burial Depth**
Type of the Article	**Original Research Article**

General guideline for Peer Review process:

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:

(http://www.sciencedomain.org/page.php?id=sdi-general-editorial-policy#Peer-Review-Guideline)

SDI Review Form 1.6

PART 1: Review Comments

	Reviewer's comment	**Author's comment** *(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)*
Compulsory REVISION comments	The title is erroneous and misleading! It reads 'Seedling (…) as affected by seed weight and burial depth'. This should mean that the effect of weight and depth jointly were investigated, but nothing of the kind happened. Weight was investigated at 4 cm depth or in petri dishes experiments, depth was investigated with seeds supposedly of the same weight 'to avoid possible interactions between (…) seed weight and burial depth (l. 90-91). Change 'seed weight AND burial depth' to 'seed weight OR burial depth' in the title and wherever it applies in the abstract and text. One of the main problems I find in this paper is the choice of species to investigate the effects of seed weight. In fact *Scandix pecten-veneris* has mericarps with a prominent beak that may represent more than half of its length without contributing to reserves. This has necessarily implications in weight that should be addressed somehow, somewhere. Another problem is the allergy that authors show towards variability parameters. Please state standard error of mean weights (l. 64, 92, all tables), the same in all figures, the same whenever means are used. Also the use of MET (defined in l. 70). MET is an index that like others is dangerously misleading, even more when absolute rather than relative frequencies are used. This is known for a long time (e.g. a discussion in	

	Heydecker W 1966. Nature 210, 753-754). Just to illustrate the issue imagine two species A and B. Records are done in day 1, 2, 3, 4, 5, 6; in species A the emergence is: 6 plants in day 1, 4 plants in day 6; in species B the emergence is 10 plants in day 3. MET is the same! As one of the papers cited in this Ms. puts it (ref. [2]) 'the problem is that germination is a complex and continuous function that cannot be characterized easily by a simple index'. I emphasize 'continuous' .Now, there are a number of ways to account for this continuity and also for the variability of the germination/emergence process (a review and the examination of one of them can be found in Dias LS 2001. J Chem Ecol 27, 411-418). In short, please use some continuous modelling that gives meaningful and unambiguous results. Also the use of Tukey's Honestly Significant Difference test. It is a simultaneous test procedure (STP) based upon the MSerror of ANOVA which makes critical the occurrence of homocedasticity (that the authors failed to check). In addition, Tukey's HSD, like all others STPs varies critical error rates with number of samples involved (e.g. Jones D 1984. Environ Entomol 13, 635-649). Altogether this might explain the very intriguing differences between MET of light and heavy seeds in 2010 (fig. 1; as noted above SE bars would be very helpful). Another consequence of using STP emerges in Table 2. In this table (the same for Table 1) the so called 'transitive law' is again and again violated. For example in 2008-09, 25 Nov., if MET for 2.5 cm = MET for 7.5 cm, and MET for 7.5 cm = MET for 12.5 cm, then MET 2.5 cm should equal MET for 12.5 cm, which it does not! (e.g. Chew V 1976.	

	HortScience 11, 348-357). l. 78 – what is the diameter of Petri dishes? This question is because for me numbers don't seem to add up. I will assume that authors used 10-cm Petri dishes meaning that inner diameter is about 9 cm. Thus the inner surface is 63.6 square centimetres. The authors placed 20 seeds and added 50 mL of soil. If I see it clearly, seeds were hardly covered by soil (without seeds, to completely cover 63.6 sq. cm. with 50 mL of soil implies that soil depth is about 0.8 cm). So, what was the need to dig soil (l. 85) if seeds were very likely uncovered by it? l. 93 – I can only guess but I think that the two periods of time, 25 November and 15 March refer to date of sowing. The authors stated before that *S. pecten-veneris* is a common weed on winter cereals (l. 25-26). They also stated that *S. pecten-veneris* maturity occurs in central Greece at the end of May. I would bet that field germination of this weed is not by mid-March. I would double-bet that field germination is by November (somehow earlier or later according to rainfall). So, unless the authors intend to sow seeds of this weed, what is the point of the March sowing which in natural conditions would probably never occur? This point might have serious implication in the overall relevance and acceptability of the paper. If the 15 March is meaningless then seedling emergence is meaningfully affected in 2008-09 but not in 2010-11 (Table 1) and a lot of rewriting is needed. l. 114-115; 'MET of light seeds was lower (…) compared to that of heavy seeds'; this is not true unless we analyse only within each year. Ranking MET results in L2008<H2008<L2010<H2010! No discussion of this	

	year-dependency is done and the erroneous statement that MET is lower in light seeds is repeated again and again (l. 203, 245). However the importance of the year of harvest is extensively obvious ; for example table 1, germination 25 Nov., NS in 2010-2011, a lot of differences in 2008-2009, and in this case all seeds were supposed to be medium size (l. 90-92)! I could imagine that soil temperatures (see Figure 3) might be useful to understand or explain some results. However I could not find that the authors had any use to these data. Unless I missed something, then either use the data for something or delete it.	

SDI Review Form 1.6

Minor REVISION comments	l. 22-23; 'key events (…) in an agroecosystem' – this might be true for seedling emergence (as stated by Forcella et al 2000) but not necessarily for seed germination. There are weeds, and some very noxious, relying in vegetative reproduction alone, others in sexual and vegetative reproduction. Please rephrase accordingly. l. 48; 'totally random way' – randomness is a good thing but how did the authors achieved total randomness? By contrast, what might be 'partial randomness'? l. 49-50; *S. pecten-veneris* plants are mature by May; harvest was done about 2 months later; why? And what could be, if any, the importance of this delay to the characteristics of the fruits? l. 56; write S, Si and C in full, please. l. 65, l. 94; what was the size of the blocks in the field? l. 65, l. 94; how did you control that seeds did not move upward or downward in the soil? l. 66, 92; a RCB in the field? How are the authors certain that a split-plot or a split-block wouldn't be more adequate and efficient? It usually is, but again it might depend on plot size which is unknown to the reader. l. 66-67; seeds harvested in July 2007 were tested in March 2008, and those harvested in July 2009 were tested in February 2010.Where and under what conditions were they stored? l. 94 – 'seeds were planted'. Seeds are not planted. Seeds	

	are sown. Plants are planted. l. 285; the link for ref. 10 seems to be broken or unavailable. However alternative ways to get there exist and the reference is totally blank in relation to the self-referenced experimental data. Why not replace [10] and what follows it by (not shown)?	
Optional/General comments	l. 69-70; MET (Ellis & Roberts 1981; by the way the correct reference is Ellis RH, not RA) is nothing more than the inverse of the much earlier Kotowki's coefficient of velocity (Kotowski F 1926. Proc Am Soc Horticult Sci 23, 176-181). Bond et al (Oecologia 120, 132-136, 1999) derived a predictive equation for the maximum depth of seedling emergence using as predictor seed mass. According to their equation, the maximum depth for 25 mg seeds (l. 92) of *S. pectin-veneris* would be 8.0 cm. However the authors recorded emergences from seeds placed down to 15 cm. As remarked above we have no clue whether or how authors experimentally controlled that seeds remained at their assigned burial depth. Nevertheless it could be nice to acknowledge this discrepancy and even nicer to discuss it.	

Note: Anonymous Reviewer