Ensemble approaches for estimating congruence between species delimitation and morphological variation: comparing taxonomic hypotheses for the Pacific Pond Turtle (*Emys marmorata*)

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# Introduction

Molecular systematics has repeatedly demonstrated the existence of cryptic species that can only be diagnosed using genetic data (Stuart, Inger, and Voris 2006; Bickford et al. 2007; Schilck-Steiner et al. 2007; Pfenninger and Schwenk 2007; Clare 2011; Funk, Caminer, and Ron 2012). In attempts to streamline the documentation of biodiversity, several methods of species delimitation that rely almost entirely on genetic data have recently been proposed (Pons et al. 2006; B.C. Carstens and Dewey 2010; Hausdorf and Hennig 2010; O’Meara 2010; Yang and Rannala 2010; J. P. Huelsenbeck, Andolfatto, and Huelsenbeck 2011). Although strong caveats on the utility of these methods have been raised (Bauer et al. 2000; Bryan C. Carstens et al. 2013), they are nevertheless being used to name species (Leaché and Fujita 2010; Spinks, Thomson, and Bradley Shaffer 2014).

In contrast to those genetically-diagnosed species, the majority of extant taxa, and almost all extinct taxa, are delimited by morphology alone. This disjunction complicates interpretations of variation and diversity in deep time, as apparent morphological stasis may not reflect the true underlying diversity (Eldredge and Gould 1972; Gould and Eldredge 1977; Van Bocxlaer and Hunt 2013). It also has serious implications for our records of modern biodiversity: for many museum specimens of extant taxa (e.g. those preserved in formalin), it is difficult to acquire the genetic data needed for non-morphological species delimitation methods.

These considerations have sparked interest in whether geometric morphometric analyses can capture fine-scale variation that can be used for identifying cryptic species. This would make the task of identifying and maintaining endangered or conserved groups much easier and could contribute to improved classifications of extinct taxa and populations. Most such studies focus on using morphometrics to discover differences between taxa that were identified by other means (P.David Polly 2003; Zelditch, Swiderski, and Sheets 2004; Gaubert et al. 2005; Gündüz et al. 2007; P.David Polly 2007; Demandt and Bergek 2009). Additionally, there has been work on automated taxon identification and classification of taxa into groups (Baylac, Villemant, and Simbolotti 2003; Dobigny et al. 2003; MacLeod 2007; van den Brink and Bokma 2011; Vitek et al. 2017).

Here, we investigate the morphometric identification of cryptic species using machine learning approaches. We use an ensemble learning approach where multiple methods are used in order to look for consensus amoung their results. We test our approach on three datasets: plastron shape two species of closely related turtles, plastron shape seven species of closely related turtles, and plastron shape of the *Emys marmorata* species complex. In particular, we ask whether it is possible to determine which among a set of classification hypotheses best aligns with the observed morphology, and examine the implications of our results for the *E. marmorata* complex.

## Background and study system

Machine learning is an extension of known statistical methodology (Hastie, Tibshirani, and Friedman 2009) that emphasizes high predictive accuracy and generality at the expense of the interpretability of individual parameters. The basic statistical mechanics are supplemented by randomization, sorting, and partitioning algorithms, along with the maximization or minimization of summary statistics, in order to best estimate a general model for all data, both sampled and unsampled (Hastie, Tibshirani, and Friedman 2009). Machine learning approaches have found use in medical research, epidemiology, economics and automated identification of images such as handwritten zip codes (Hastie, Tibshirani, and Friedman 2009). There are two major classes of machine learning method: unsupervised and supervised learning. Unsupervised learning methods are used with unlabeled data where the underlying structure is estimated; they are analogous to clustering and density estimation methods (Kaufman and Rousseeuw 1990). Supervised learning methods are used with labeled data where the final output of data is known and the rules for going from input to output are inferred. These are analogous to classification and regression models (Breiman et al. 1984). Our application of the approaches used in this study illustrates only a sampling of the various methods available for fitting classification models. The classification methods used in this study were chosen because they are suited for more than two response classes.

Geometric morphometric approaches to identifying differences in morphological variation between different classes, including cryptic species, mostly have used methods like linear discriminate analysis and canonical variates analysis (P.David Polly 2003; Zelditch, Swiderski, and Sheets 2004; Gaubert et al. 2005; Gündüz et al. 2007; P.David Polly 2007; Francoy et al. 2009; Sztencel-Jabłonka, Jones, and BogdanowicZ 2009; Mitrovski-Bogdanovic et al. 2013; Dillard 2017). Because of their similarity to multivariate approaches like principal components analysis (PCA), these methods are comparatively straightforward ways of understanding the differences in morphology between classes. They also benefit from producing results that can be easily visualized, which aids in the interpretation and presentation of data and results. Most previous morphometric studies did not assess which amongst a set of alternative classification hypotheses was optimal. For example, studies such as those of Caumul and Polly (2005) and P.David Polly (2007) focused on comparing different aspects of morphology and their fidelity to a classification scheme instead of comparing the fidelity of one aspect of morphology to multiple classification schemes. In this context, the study of Cardini et al. (2009) is noteworthy because they compared morphological variation in marmots at the population, regional, and species level and determined the fidelity of shape to divisions at each of these levels.

Here, we used an ensemble of supervised machine learning methods to compare the congruence of the morphological data to different classification hypotheses. Each of these methods provide different advantages for understanding how to classify taxa, as well as the accuracy of the resulting classifications. Although machine learning methods such as neural networks have been applied to studying shape variation (Baylac, Villemant, and Simbolotti 2003; Dobigny et al. 2003; MacLeod 2007; van den Brink and Bokma 2011) , including in the context of automated taxon identification and classification of groups, the number of cases remains limited. In the current study, we not only consider pure classification accuracy but also use a statistic of classification strength that reflects the rate at which taxa are both accurately and inaccurately classified.

We analyzed the problem of whether there are distinct subspecies or cryptic species within the western pond turtle, *Emys marmorata* (Baird and Girard 1852) (formerly *Clemmys marmorata*; see (Feldman and Parham 2002)). *Emys marmorata* is distributed from northern Washington State, USA to Baja California, Mexico. Traditionally, *E. marmorata* was classified into two named subspecies: the northern *E. marmorata marmorata* and the southern *Emys marmorata pallida* (Seeliger 1945), with a central Californian intergrade zone in between. *Emys marmorata marmorata* is differentiated from *E. marmorata pallida* by the presence of a pair of triangular inguinal scales and darker neck markings. The triangular inguinal plates can sometimes be present in *E. marmorata pallida* although they are considerably smaller. Seeliger (1945) did not formally include the Baja California populations of *E. marmorata* in either taxon, implying the existence of a third distinct but unnamed subspecies.

Previous work on morphological variation in *E. marmorata* has focused primarily on differentiation between populations over a portion of the species’ total range (Lubcke and Wilson 2007; Germano and Rathbun 2008; Germano and Bury 2009; Bury, Germano, and Bury 2010); comparatively few studies have included specimens from across the entire range (Holland 1992). Most of these studies considered how local biotic and abiotic factors may contribute to differences in carapace length, and they found that size can vary greatly between different populations (Lubcke and Wilson 2007; Germano and Rathbun 2008; Germano and Bury 2009). There also has been interest in size-based sexual dimorphism in *E. marmorata* (Holland 1992; Lubcke and Wilson 2007; Germano and Bury 2009), with males being on average larger than females based on total carapace length and other linear measurements. However, the quality of size as a classifier of sex can vary greatly between populations (Holland 1992) because of the magnitude of size differences among populations (Lubcke and Wilson 2007; Germano and Bury 2009). The effect of sexual dimorphism on shape, *sensu* Kendall (1977), has not been assessed (Holland 1992; Lubcke and Wilson 2007; Germano and Rathbun 2008).

Of particular relevance in the context of cryptic diversity in *E. marmorata* is the morphometric analysis of carapace shape carried out by Holland (1992), who compared populations of *E. marmorata* from three areas of the species range. Holland concluded that geographic distance was a poor indicator of morphological differentiation, and instead hypothesized that geographic features such as breaks between different drainage basis are probably more important barriers to dispersal and interbreeding. Additionally, he suggested that morphological differences were more pronounced as the magnitude of barriers and distance increased, but this variation required many variables to adequately capture, implying only very subtle morphological differentiation between putatively distinct populations. Finally, Holland concluded that *E. marmorata* is best classified as three distinct species: a northern species, a southern species, and a Columbia Basin species. This classification is similar to that of Seeliger (1945), except elevated to the species level and without recognition of a distinct Baja species.

More recently, the phylogeography of *E. marmorata* and the possibility of cryptic diversity was investigated using molecular data (Spinks and Shaffer 2005; Spinks, Thomson, and Shaffer 2010; Spinks, Thomson, and Bradley Shaffer 2014). Based on mitochondrial DNA, Spinks and Shaffer (2005) recognized four subclades within *E. marmorata*, a northern clade, a San Joaquin Valley clade, a Santa Barbara clade, and a southern clade. Analyses with nuclear DNA (Spinks, Thomson, and Shaffer 2010) and single-nucleotide polymorphism (SNP) data suggest a primarily north–south division in *E. marmorata*, although these datasets differed from that of Spinks and Shaffer (2005) in the location of the break point (Spinks, Thomson, and Bradley Shaffer 2014). All three studies discussed the potential taxonomic implications of their results, with Spinks, Thomson, and Bradley Shaffer (2014) going so far as to strongly advocate for the recognition of at least two species (*E. marmorata* and *E. pallida*), and a possible third based on populations in Baja California. However, they did not discuss in detail the morphological characters that would help to diagnose these species beyond those specified by Seeliger (1945). Given that these characters are variable within the proposed species, and that Holland (1992) described shell shape variation that might be consistent with this taxonomy, a geometric morphometric analysis of shell shape might provide a reliable way to diagnose groups (whether species or subspecies) within *E. marmorata*.

In this study, we attempt to estimate the best classification scheme of *E. marmorata* based on variation in plastron (ventral shell) shape in order to determine whether this character is consistent with any of the proposed taxonomies of the *E. marmorata* ecomplex.

We choose to analyze plastron shape for multiple reasons. First, it is very easy to collect geometric morphometric data on plastron shape from two-dimensional pictures as the structure is virtually flat. Additionally, this allows both museum specimens and individuals in the field to be analyzed together. Second, previous work has suggested that there are strong differences in plastron shape among traditionally recognized emydine species (K. D. Angielczyk, Feldman, and Miller 2011). Finally, due to this previous study a large dataset was readily avaliable.

In the case of the *E. marmorata* species complex, we hypothesize that if one or more of the proposed classification schemes are consistent with the morphological data then our ensemble approach fit to those hypotheses will have higher out-of-sample predictive performance than the more inconsistent hypotheses. However, if all of the classification schemes lead to equal out-of-sample predictive performance then we would conclude that the molecular based hypotheses are inconsistent with whatever information is present in the morphological data. Because of unclear geographic boundaries between subgroups of *E. marmorata*, we compare multiple permutations of the (Spinks, Thomson, and Shaffer 2010) and Spinks, Thomson, and Bradley Shaffer (2014) hypotheses.

# Materials and Methods

## Specimens, sampling, morphometrics

Three different geometric morphometric datasets describing turtle plastron variation were assembled for this analysis: 1) specimens from seven distinct emydine species; 2) *T. scripta* specimens from the two main subspecies (*T. scripta elgans* and *T. scripta scripta*); and 3) *E. marmorata* specimens from across the species’ geographic range. The first two datasets are intended to serve as a test of whether machine learning techniques can differentiate species-level groupings of emydine turtles using plastron shape. We expect that the first dataset represents a low-complexity dataset given the high level of plastron shape disparty that exists among these species (K. D. Angielczyk, Feldman, and Miller 2011), whereas the second dataset should be relatively higher in complexity than the first. Ultimately, we expect that the third dataset to be of the highest complexity and our biggest analytical challenge. For all datasets we chose to focus on adults because significant changes in plastron shape occur over the course of ontogeny in *E. marmorata* and other emydines (K. D. Angielczyk and Feldman 2013).

The first dataset we analyzed includes 578 total specimens from the following species: *Clemmys guttata*, *Emys blandigii*, *Emys orbicularis*, *Glyptemys insculpta*, *Glyptemys muhlenbergii*, *Terrapene coahuila*, and *Terrapene ornata*. These specimens are a subset of those used in K. D. Angielczyk, Feldman, and Miller (2011) and K. D. Angielczyk and Feldman (2013).

The second dataset is a compilation of 101 specimens of two subspecies of *T. scripta*: 51 specimens of *T. scripta scripta* and 50 specimens of *T. scripta elegans*. These landmark data are new to this study.

The final dataset dataset is of 366 adult *E. marmorata* museum specimens. These speciemens represent a subset of those included in K. D. Angielczyk and Sheets (2007), K. D. Angielczyk, Feldman, and Miller (2011), and K. D. Angielczyk and Feldman (2013). Because Spinks and Shaffer (2005), Spinks, Thomson, and Shaffer (2010), and Spinks, Thomson, and Bradley Shaffer (2014) did not use vouchered specimens we were not able to directly sample the individuals in their studies, our specimen classifications were based solely on the geographic information, not explicit assignment using molecular data. For each taxonomic hypothesis, each specimen was assigned to one of the possible classes based on geographic occurrence data recorded in museum collection archives. In cases where precise latitude and longitude information were not available we estimated them from locality information. Because the exact barriers between different biogeographic regions are unknown and unclear, we represented each hypothesis with multiple possible realizations representing the classification uncertainty for specimens present at the geographic boundaries. The taxonmic hypotheses and sub-hypotheses for the *E. marorata* used here are presented in Table [tab:hypotheses].

Sex information was known only for a subset of the total dataset and was not included as a predictor of classification. Instead, we estimated the degree by which specimens cluster morphologically by sex in order to determine how much of a potential biasing factor sexual dimorphism could be for our analysis of the *E. marmorata* species complex.

[h] image [fig:plastra]

l l l Abbreviation & Number of classes & citationSP10.1 & 3 & Spinks, Thomson, and Shaffer (2010)SP10.2 & 3 & Spinks, Thomson, and Shaffer (2010)SP10.3 & 4 & Spinks, Thomson, and Shaffer (2010)SP14.1 & 2 & Spinks, Thomson, and Bradley Shaffer (2014)SP14.2 & 4 & Spinks, Thomson, and Bradley Shaffer (2014)

[tab:hypotheses]

Following previous work on plastron shape (K. D. Angielczyk and Sheets 2007; K. D. Angielczyk, Feldman, and Miller 2011; K. D. Angielczyk and Feldman 2013), we used TpsDig 2.04 (Rohlf 2005) to digitize 19 two-dimensional landmarks (Fig. [fig:plastra]). Seventeen of the landmarks are at the endpoints or intersection of the keratinous plastral scutes that cover the plastron. Twelve of the landmarks were symmetrical across the axis of symmetry. Because damage prevented the digitization of all the symmetric landmarks in some specimens, we reflected landmarks across the axis of symmetry (i.e. midline) prior to analysis and used the average position of each symmetrical pair. In cases where damage or incompleteness prevented symmetric landmarks from being determined, we used only the single member of the pair. We conducted all subsequent analyses on the resulting “half” plastra. We superimposed the plastral landmark configurations using generalized Procrustes analysis (Dryden and Mardia 1998), after which, we calculated the principal components (PC) of shape using the shapes package for R ([CSL BIBLIOGRAPHIC DATA ERROR: reference "R2016" not found.]; [CSL BIBLIOGRAPHIC DATA ERROR: reference "Dryden2013" not found.]).

## Biasing effects

We estimated the possible effect of digitization error (Arnqvist and Mårtensson 1998; von Cramon-Taubadel, Frazier, and Lahr 2007; Munoz-Munoz F. and Perpinan D. 2010) on our results by comparing within (replicated) specimen Procrustes distances to the distances between classification scheme centroids. Ten randomly selected *E. marmorata* specimens were each digitized four times, with the original set of digitized coordinates serving as a fifth replicate. These 50 landmark configurations were then Procrustes superimposed. A range of four Procrustes distances was then calculated as the average of the pairwise distances between each of the replicate configurations of a given specimen.

For each specimen, the difference in shape caused by digitization was calculated as the mean of all pairwise Procrustes distances between the five replicates of that specimen. The average distance between any two digitizations was calculated as the mean of all pairwise Procrustes distances between all replicates for all specimens. The ratio between these two values was used to assess the magnitude of variation caused by digitization. The goal of this ratio is to determine if the within group distances are on average smaller than the between individual distances; a value of 0 indicates perfect grouping, a value of 1 indicates no difference between grouping and no grouping, and a value of 1+ indicates that the grouping is counter-intuitive to the data.

*Emys marmorata* is known to display sexual dimorhpism in plastron shape, particulary the present of a plastra concavity in males (Seeliger 1945). To test for biases resulting from sexual dimorphism in our *E. marmorata* data set, we used a simple permutation test to determine if the distance between the mean female and male shapes is greater than expected when the sex labels are randomly shuffled. Because not all of our specimens have sex identifications associated with them, this analysis was done using a subset of the data (257 of 360).

## Supervised learning approaches

Instead of relying on a single supervised learning method, we chose to use an ensemble approach where muliple model types are used in concert so that any congruence between the them increases our support for that conclusion over another (Hastie, Tibshirani, and Friedman 2009). The supervised learning methods used here are named in Table [tab:methods]. Each of these methods makes different assumptions, treats data differently, and can produce different classification results depending on the nature of the data (Hastie, Tibshirani, and Friedman 2009).

|  |  |  |  |
| --- | --- | --- | --- |
| Method name | abbreviation | R package | citation |
| multinomial logistic regression | MLR | nnet | ([CSL BIBLIOGRAPHIC DATA ERROR: reference "Venables2002" not found.]) |
| linear discriminate analysis | LDA | MASS | ([CSL BIBLIOGRAPHIC DATA ERROR: reference "Venables2002" not found.]) |
| penalized discrminiate analysis | PDA | mda | ([CSL BIBLIOGRAPHIC DATA ERROR: reference "mdapack" not found.]) |
| single-hidden-layer neural network | NN | nnet | ([CSL BIBLIOGRAPHIC DATA ERROR: reference "Venables2002" not found.]) |
| random forests | RF | randomForest | ([CSL BIBLIOGRAPHIC DATA ERROR: reference "Liaw2002" not found.]) |

[tab:methods]

The maximum set of possible predictors or features used for any model of our dataset is comprised of the first 25 principal components (PCs), scaled centroid size, and the interaction between scaled centroid size and PC 1. Additional interaction terms were not considered because of model complexity/sample size concerns. Size and the interaction between size and PC 1 were included as predictors to account for known ontogenetic variation in plastron shape (K. D. Angielczyk and Feldman 2013) as well as potential size differences between classes, even if this is unlikely (Seeliger 1945; Holland 1992). These data constitute a “maximum set” because the best or selected models based on five-fold cross-validation need not, and likely will not, include all predictors possible (see below). Because our supervised learning models use PCs as predictors, this approach is in many ways analogous to PCA regression. PCA regression takes advantage of reduction and orthogonality PCs to improve regression fit (Hastie, Tibshirani, and Friedman 2009). Because the PCs of shape are by definition orthogonal, they can easily serve as independent predictors or features of class membership without fear of collinearity.

We adopted a training and testing paradigm for selecting parsimonious models and estimating their overall error rates (Hastie, Tibshirani, and Friedman 2009; Kuhn and Johnson 2013). Within-sample model performance is inherently biased upwards, so model evaluation requires overcoming this bias. With very large sample sizes, as in this study, part of the sample can be used as the “training set” and the remainder acts as the “testing set.” In this approach, following all cleaning and vetting, the data are split into a training dataset and a testing dataset. The former is used for fitting the model whereas the later is used for measuring model performance, a process called model generalization. In this analysis, we used 80% of samples as the training set while the remaining 20% were used as the testing set.

In classification studies, such as this one, a common metric of performance is the receiver operating characteristic (ROC) which is the relationship between the false and true positive rates (Hastie, Tibshirani, and Friedman 2009). The area under the ROC curve (AUC) is the derived estimate of the model performance; AUC ranges from 0.5 to 1 which correspond to performance similar to random guesses and perfect classification rates, respectively (Hastie, Tibshirani, and Friedman 2009). Both ROC and AUC are preferable to simple classification accuracy when class membership is unbalanced, as it is in these analyses (Hastie, Tibshirani, and Friedman 2009). The standard ROC and AUC calculations are defined only for binary classifications, which is not the case for our seven species and *Emys* complex datasets. To generalize this approach for situations with multiple response classes, we used an all-against-one strategy where the model AUC is the average of the AUC values from the multiple binary comparisons of one class compared to all others (Hand and Till 2001).

For a given supervised learning method, we compared the fit of 27 models as the average AUC from 10 rounds of five-fold cross-validation. Cross-validation is an approach for estimating the average out-of-sample predictive error of a model by simulating out-of-sample data from the training dataset itself (Hastie, Tibshirani, and Friedman 2009). In a single round of -fold cross-validation, the training data are divided into blocks where the model is fit to blocks and the values of the th block are predicted. This is repeated for all combinations of blocks. Within each round, the predictive performance metrics are averaged across all folds. Finally, the predictive performance metric is the averaged across all rounds of -fold cross-validation. This process was implemented using the R package caret ([CSL BIBLIOGRAPHIC DATA ERROR: reference "KuhnMAN2013" not found.]). For a given supervised learning method, the “best” trained model is that with the highest mean AUC as estimated from five-fold cross-validation. The selected or final model, however, is the next most parsimonious model that is within one standard error of the best model; this is a variant on the “one-standard error” rule from Hastie, Tibshirani, and Friedman (2009). This rule is applied to ameliorate the chance of selecting an overly complex model.

# Results

## Geometric morphometrics

The results of the PCA of plastron shape in both the seven species and *Trachemys* datasets demonstrate strong association between shape and the recognized classification schemes (Fig. [fig:otherpca]). Additionally, there appears to be both little difference in centroid size across individuals as well as little correspondence between centroid size and the PC axes.

[ht]

[b]0.4 image

[b]0.4 image

[fig:otherpca]

The results of the PCA of plastron shape in the *Emys marmorata* dataset show no clear connection between plastron shape and any of the proposed classification schemes (Fig. [fig:emyspca]). The first PC axis of shape variation appears to be primarily structured by differences in individual centroid size (Fig. [fig:emyspca]); this was the motivation for including centroid size and its interaction with PC1 as predictors in all of the supervised learning models.

[ht] image [fig:emyspca]

Analysis of the differences between sexes of *E. marmorata* indicates that sex does not appear to strongly structure differences in shape (Fig. [fig:sextest]). The difference in mean shape between the sexes is very small; the sexes overlap slightly more than is expected from a null distribution based on permuting the sex-labels.

[ht] image [fig:sextest]

Comparison of the within to between Procrustes distances of the digitization replicates gives an approximate estimate of the error between distinct groupings (Table [tab:repres]). The ratio of the average within-individual distance to the average distance between individuals for the replicated datasets is 1.11; this indicates that the grouping is slightly counter-intuitive to the data and is consistent with all shapes being very similar regardless of individual identity. This value also provides a baseline by which to understand how distinct the groupings are, where other ratios are compared to the correction ratio .

The results from the seven species and *Trachemys* datasets indicate that both of these classification schemes are more recognizable than not given our estimate of digitization error (Table [tab:repres]). In contrast, the different *E. marmorata* classification schemes appear to be barely be distinct, with their within:between ratios approximating 1. This indicates that the magnitude of the differences between groupings is approximately the same as the difference as any two random individuals (Table [tab:repres]).

|  |  |  |  |
| --- | --- | --- | --- |
| **Dataset** | **Scheme** | **Ratio** | **Corrected ratio** |
| Replicates |  | 1.11 |  |
| Seven species |  | 0.33 | 0.37 |
| *Trachemys* |  | 0.76 | 0.84 |
| *E. marmorata* | SP10.1 | 0.93 | 1.03 |
|  | SP10.2 | 0.93 | 1.03 |
|  | SP10.3 | 0.88 | 0.98 |
|  | SP14.1 | 0.98 | 1.09 |
|  | SP14.2 | 0.87 | 0.97 |

[tab:repres]

## Supervised learning

Analysis of the seven morphologically and genetically distinct species and the *T. scripta scripta*–*T. scripta elegans* datasets indicate that these classifications are sufficiently morphologically distinct to be differentiated on the basis of plastron shape. Both in-sample and out-of-sample classification have AUC values of approximately 1 for all methods, implying near-perfect classification rates (Fig. [fig:othersel], [fig:otheroos]). For both datasets, the ROC scores from testing datasets are tightly clustered near AUC = 1 (Fig. [fig:otheroos]). These results demonstrate that when there are distinctions between the states of the classification schemes (i.e. differences in plastron shape that correlate with the different taxonomic groups), the methods used here can recover them.

[ht] image [fig:othersel]

[ht] image [fig:otheroos]

AUC–based model selection revealed some important patterns of variation and congruence between the classification schemes and the actual data. Generally, the best performing models tended to include about half the total number of possible PCs (Fig. [fig:emyssel]).

[ht] image [fig:emyssel]

Observed AUC values for all of the optimal models are lower for the *E. marmorata* dataset(Fig. [fig:emyssel]). In most cases the different proposed classification schemes are generally poor descriptors of the observed variation. It appears that the data set is overwhelmed by noise (likely biological and analytical), making any accurate classifications difficult at best. This observation is cemented with the generalizations of the models to the testing data set (Fig. [fig:emysoos]).

[ht] image [fig:emysoos]

Mean AUC values for the model generalizations, in most cases, are approximately equal to the observed AUC values from the training data set (Fig. [fig:emyssel], [fig:emysoos]). The cases in which the AUC from the generalizations is less than the observed indicate poor model fit and a poor classification scheme. Comparison of AUC values from the model generalizations do not indicate a clear “best” classification scheme (Fig. [fig:emyssel], [fig:emysoos]).

# Discussion

As expected, our ensemble approach yields high out-of-sample classification performance for the first two datasets. These results indicate that in cases of clear class seperation (Fig. [fig:otherpca]) our approach is able to detect this and make good out-of-sample prediction.

In the case of the *E. marmorata* data set, our results show that none of the proposed taxonomic hypotheses for the *E. marmorata* species complex are more consistent with morphological differentiation than any other proposal (Fig. [fig:emysoos]). Both the low out-of-sample AUC values and the significant difference between the correctly and incorrectly classified observations support the conclusion that none of the hypothesized classification schemes are good descriptions of the observed plastral variation within *E. marmorata*. An analytical explanation of this result is that the level of digitization error in the *E. marmorata* dataset is so great as to swamp out any biological signal. We think this is unlikely because all of the specimens considered in our three analyses were digitized by one of us (K.D.A.), and digitization error was not a problem in the seven species or *Trachemys* examples. There are also no features of the plastron of *E. marmorata* that would make it significantly more difficult to accurately digitize than the plastra of the other speices.

Biological explanations include the possibilty that genetic differentiation is not associated with plastron shape variation and/or that local selective pressures (e.g. from hydrological regime) overwhelm morphological differentiation. Both of these options seem plausible given that shell shape is influenced by selection for both protection and streamlining, but not necessary mate choice (Rivera 2008; Rivera and Stayton 2011; Stayton 2011; Rivera et al. 2014; P.D. Polly et al. 2016) and that shell shape in *E. marmorata* is known to vary among populations inhabiting water bodies with different flow regimes (Holland 1992; Lubcke and Wilson 2007; Germano and Bury 2009). Plastron shape does not seem to preserve a strong phylogenetic signal at the interspecific level in emydine turtles, at least compared to the effect of the presence or absence of a plastral hinge (K. D. Angielczyk, Feldman, and Miller 2011), and our current results suggest that this may be the case for phylogeographic signal within emydine species as well. A final possibility (explored below) is that the proposed classification schemes themselves do not represent significant evolutionary lineages.

Despite the negative result for *E. marmorata*, it is important to note that plastron shape is an extremely effective method for differentiating classes in the additional datasets we investigated. The magnitude of shape differences between the species (measured as Procrustes distance between the seven species’ mean shapes) is approximately an order of magnitude greater than the differences between the *E. marmorata* subgroups, and not surprisingly the machine learning methods had no trouble classifying the specimens correctly. However, the magnitude of the shape differences between the *T. scripta* subspecies is comparable to those separating the different *E. marmorata* subgroups, yet even in this case the machine learning methods returned an almost perfect classification. These results demonstrate that plastron shape is normally a good marker for differentiating real subgroups in close relatives of *E. marmorata*, and that our lack of results for *E. marmorata* is not simply a shortcoming of the methods we applied. Indeed, it begs the question of what factors have suppressed morphological differentiation of plastron shape in *E. marmorata* and *E. pallida* if they are distinct species. Invoking issues such as the role of the plastron in protection or the need for streamlining are insufficient because the other species are expected to be subject to similar constraints (Stayton 2011; P.D. Polly et al. 2016). Although it may seem counterintuitive that plastron shape is both useful for species delimitation but has weak or absent phylogenetic signal, it is important to remember that these are different goals. While phylogenetically similar species may not be morphologically similar (e.g. compare the box turtles of the genus *Terrapene* to the closely related spotted turtle *Clemmys guttata*), the variation within a species typically is much less than the variation between species. Therefore, the consistent plastron shapes that characterize different emydid species leads to plastron shape being a useful tool for species delimination, even when other selective factors have overprinted similarities stemming from patterns of descent from common ancestors.

## Is there more than one species of Western Pond Turtle?

The lack of morphological support for the distinctiveness of *E. pallida* does not, on its own, preclude the recognition of this taxon. However, this apparent lack of congruence does prompt a reexamination of the methods and concepts that led to that taxonomic revision, especially considering that plastron shape is demonstrably capable of differentiating species and subspecies among other emydids. In other words, before we can assess the significance of the morphological non-diagnosablity, it is essential to evaluate the methods and concepts that led to the initial taxonomic revision.

Spinks, Thomson, and Bradley Shaffer (2014) elevated *E. pallida* based on a species delimitation analysis of SNP data using BPP (Yang and Rannala 2010). However, Spinks, Thomson, and Bradley Shaffer (2014) did not heed the caveats about such species delimitation methods raised by Bryan C. Carstens et al. (2013). In addition to specifically addressing the shortcomings of validation methods such as BPP that rely on guide trees and “should be interpreted with caution,” Bryan C. Carstens et al. (2013) also strongly emphasized that “Inferences regarding species boundaries based on genetic data alone are likely inadequate, and species delimitation should be conducted with consideration of the life history, geographical distribution, morphology and behaviour (where applicable) of the focal system…” These caveats evoke the development of the Unified Species Concept (Dayrat 2005; De Queiroz 2007), Integrative Taxonomy (Padial et al. 2010), and other pluralist approaches to species delimitation. None of these considerations were brought to bear on the *E. marmorata* system until now, and in doing so we find the proposal that *E. pallida* is a distinct species to be lacking in a normally robust morphological marker.

The natural history and geographical distribution of *E. marmorata* and *E. pallida* also make the recognition of these taxa implausible. The data from Spinks, Thomson, and Bradley Shaffer (2014) show extensive introgression and admixture in Central California, which is expected because there are no significant barriers to gene flow in this region. Combined with the well-demonstrated ability for testudinoid turtles, including emydids and even *Emys*, to hybridize (e.g. Buskirk, Parham, and Feldman (2005; Spinks and Shaffer 2009; Parham et al. 2013)) it is hard to imagine how *E. marmorata* and *E. pallida* could maintain their integrity in the face of such admixture. Because the geography, natural history, demonstrated genetic admixture of *E. marmorata*, and comparisons with other morphologically diagnosable species and subspecies conflict with the recognition of *E. pallida*, we hypothesize that our inability to classify the morphological data by proposed species is because *E. pallida* is not a distinct species.

We fully agree with Spinks, Thomson, and Bradley Shaffer (2014) that *E. marmorata* (*sensu lato*) is a species deserving of strong conservation efforts, and we do not wish to trivialize this need. Moreover, the genetic diversity uncovered by the analysis of Spinks, Thomson, and Bradley Shaffer (2014) should be accounted for explicitly in any conservation plan. Given the apparent lack of morphological distinction, however, we consider that this diversity should be considered Evolutionary Significant Units or Distinct Population Segments instead of distinct species.

Finally, it is important to note that the data and analyses we present do not let us definitively say whether the apparent lack of morphological divergence within *E. marmorata* truly reflects the presence of a single species, or if it is an artifact of plastron shape being a poor morphological marker for phylogenetic and phylogeographic divergences in the case of *E. marmorata*. This is because we could not carry out our morphometric analyses on the specimens from which the genetic data were obtained. The comparisons with the other emydid taxa suggest that our negative result is is because *E. marmorata* is a single species. However, tests of both our preferred conclusion (*E. marmorata* as a single species) and that of Spinks, Thomson, and Bradley Shaffer (2014) should include morphological and molecular analyses of the same set of voucher specimens, as well as additional tests of species delimitation using alternative methods and corroborating evidence as suggested by Bryan C. Carstens et al. (2013). From a morphological standpoint, support for the validity of “*E. pallida*” may come from other aspects of morphology, such as carapace shape or other features. Likewise, further investigation of the phylogeographic utility of plastron shape in other turtle species will help to clarify whether the lack of differentiation seen in *E. marmoarata*, and the strong differentiation among the other emydids, is typical or an unusual case.

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